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Jonas Cuzulan Hirani

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Family planning, fertility and abortion: evidence from a historical policy experiment

Jonas Cuzulan Hirani

The Danish Center for Social Science Research (VIVE), Copenhagen, Denmark

ABSTRACT
A large literature considers family planning programs with a focus on birth control and finds that access reduces fertility. In this paper, I study the fertility effects of access to a Danish family planning program introduced in 1939 and designed as a political response to decades of declining fertility and widespread use of illegal abortions. I exploit variation in the timing of program implementation and use digitised data for Danish towns and counties from 1921 to 1947 to estimate the causal fertility effects using the synthetic control method. I find significant and positive non-marital fertility effects of program access but no effects on marital fertility. Suggestive evidence indicates that mothers, who in the absence of the program would have aborted illegally, now give birth and either adopt away or raise the child outside of marriage.

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Family planning; fertility; abortion; economic history; demography

1. Introduction

Economic and demographic research has a long-standing interest in fertility management and control. An extensive literature considers the fertility effects of family planning programs (G. Miller & Babiarz, 2016). Today, family planning programs have approached global coverage (de Silva & Tenreyro, 2017) and research highlights the importance of family planning on world population growth (Bongaarts et al., 1990).[1] Existing evidence considers family planning programs which offer services and information concerning contraceptive methods and fertility control with reductions in population growth and fewer unwanted births as policy targets.

This paper adds to the understanding of how public family planning programs can influence fertility in a historical context. In 1939, a Danish family planning program became part of the public health care sector and expanded from Copenhagen in two waves. In the first expansion wave in 1939, the family planning program was introduced in the five largest towns in Denmark and two medium-sized towns (Sønderborg and Næstved) and in 1948 to universal coverage (Medical Reports for the Kingdom of Denmark, 1921–1947). The political background for the expansion of the family planning program was declining fertility through the early part of the twentieth

CONTACT Jonas Cuzulan Hirani jjh@vive.dk The Danish Center for Social Science Research (VIVE), Herluf Trolles Gade 11, Copenhagen 1052, Denmark

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[1]According to de Silva and Tenreyro (2017), the negative relationship between income and fertility has shifted to the left during the last 60 years. The same GDP per capita level is associated with lower fertility today compared to 1960 in cross-country comparisons. They mention the global spread of family planning programs as the underlining driver. Bongaarts et al. (1990) project that in absence of family planning programs, the population in the developing world would reach 14.6 billion in 2100 instead of 10 billion (World Bank projection).

century and the program became an instrument to increase fertility (The Population Commission, 1938). This paper's main contribution is to estimate the impact of a family planning program with the objective of increasing fertility (opposite to most family planning programs) at a point in history without contraceptives. The results of this paper have policy relevance in developing countries where the uptake of contraceptives is low and where pregnancies outside of marriages are stigmatised.

In this paper, I analyse the non-marital and marital fertility effects of the 1939 family planning program in Denmark. I digitise historical data for Danish towns and counties to construct a panel dataset with yearly observations on the number of live births in and outside of marriage covering 1921–1947. To examine the fertility effects of the program, I use the synthetic control method (Abadie et al., 2010, 2015; Abadie & Gardeazabal, 2003). I use the first expansion wave in 1939 as a natural experiment. However, as the top five largest towns in Denmark were all treated and none of the control towns represent suitable comparisons, I focus on the two treated medium-sized towns and 25 suitable control towns.

I find that access to the family planning program had an impact on fertility. However, the impact differed as intended across marital status of the mother: the non-marital birth rate increased while the marital birth rate was unchanged in response to program access. I explore two potential explanations: the results may either be driven by pregnant (unmarried) women from neighbouring towns and rural upland travelling to the treated towns to give birth (i.e. migration but no change in birth-giving) or an actual increase in fertility for residents exposed to the program. The evidence shows that both explanations play a role. The findings indicate that the family planning program reduced the cost of giving birth outside of marriage by providing alternatives to illegal abortion. The alternatives were easier access to adoption and legal, social and health related advice. In line with this, marital fertility and the marriage rate did not respond to the family planning program.

My analyses cannot determine whether lifetime fertility was affected as it focuses on short-term effects. However, the results indicate that unmarried pregnant women who were inclined to pursue illegal abortions substituted toward the alternatives as recommended by the family planning program. This directly increased non-marital fertility through a reduction in the use of illegal abortions. The results of this paper, combined with the previous literature, suggest that family planning policies have the ability to both increase and reduce fertility. The exact content and context of the program determines how fertility responds.

By studying a program uniquely designed to increase fertility, this paper contributes to the literature on how public policies and access to family planning, contraceptives, abortions impact fertility. Previous research into family planning programs consistently indicates that increased access reduces fertility and unwanted pregnancies (Angeles et al., 2005a, 2005b; Bailey, 2013; Bailey et al., 2019; Kearney & Levine, 2009; Phillips et al., 1982).[2] Studies directly investigating oral contraception and abortion likewise find that access affects fertility decisions (Bailey, 2006; Goldin & Katz, 2000, 2002; Guldi, 2008; Lahey, 2014a, 2014b; Myers, 2017). Moreover, an extensive literature documents that a range of public policies (Medicaid, China one-child policy, Affordable Care Act, parental leave, child benefits) influences fertility decisions (Abramowitz, 2018; Cohen et al., 2013; DeLeire et al., 2011; González, 2013; Joyce et al., 1998; Lalive & Zweimüller, 2009; Malkova, 2018; Zavodny & Bitler, 2010; Zhang, 2017). Related and recent studies from the USA (Fischer

[2]Phillips et al. (1982) evaluate a large-scale experiment with family planning services in Bangladesh in 1977. They find that the program led to an increase in the use of contraceptives which reduced fertility by 22–25% in the first years of the project. They conclude that the family planning program met an unsatisfied demand for contraception which fuelled the decline in fertility. Angeles et al. (2005a, 2005b) study family planning programs in Indonesia and Peru in the 1990s and find that access to family planning services reduced fertility both directly and indirectly by allowing women to obtain more education which increased the alternative cost of having a child. Evidence from the USA supports those from the developing world, Bailey (2013) and Kearney and Levine (2009). Bailey et al. (2019) show that family planning programs in the USA and the avoidance of unwanted children through increased fertility control for women improved these families economic resources and benefited future children. Some research shows mixed results as reviewed by DiCenso et al. (2002). The reviewed studies are randomised experiments and might suffer from small sample sizes.

et al., 2018; Lindo et al., 2017; Lu & Slusky, 2019; Packham, 2017) evaluate the effects of restricted access to family planning services and abortion through clinic closures in Texas and find that restricted access reduces the number of abortions and increases birth rates.[3] As family planning programs usually provide information and supply contraceptives (condoms, birth control pills and abortion services), most of the existing literature considers programs which target reductions in population growth and unwanted pregnancies and births. The studies document robust evidence that such programs reduce fertility. This paper fills a gap in this literature by studying a program with the opposite policy target in a context without effective contraceptive methods.

This paper also contributes evidence towards the general understanding of what determines fertility. Recent literature highlights education, infant mortality, urbanisation, income and poverty, women's integration into the labour market and social norms (Baudin et al., 2015; Bloom et al., 2024; Gozgor et al., 2021; Spolaore & Wacziarg, 2022). The 1939 family planning program mainly provides in-kind assistance and inform and council women in an attempt to change social norms and the stigma surrounding non-marital birth-giving.

2. Background

2.1. Non-marital pregnancies and children in 1700–1900 Denmark

In 1763, King Frederik V of Denmark issued a public statement emphasising the responsibility of fathers of children born outside of marriages. The King ordered fathers to bear an equal share of the burden associated with a childbirth. If a father failed to do so, the King encouraged the local community to force the father to hand over a part of his income to the mother. According to Skalts and Nørgaard (1982), this might be the first law in the world advocating the rights for unmarried mothers and their children.

At the same time in Copenhagen, a birth ward specialising in assisting unmarried pregnant women opened. Help was free of charge and included supervised births, clothes, food and nourishment for the infant. Unmarried women had the right to remain anonymous when admitted at the hospital during childbirth.[4] In 1771, the hospital placed a box outside the hospital for women to deposit unwanted infants. The hospital would then place the child in care. The founding box was a measure to combat killings of infants born out of wedlock. In the 1800s, the progress to assist unmarried pregnant women stopped due to moral concerns. The argument was that too much help might spur promiscuity outside of marriages (Skalts & Nørgaard, 1982).

2.2. Declining fertility and the lead up

The main motivation for expanding the family planning program in Denmark was low birth rates in the first part of 1900s (Skalts & Nørgaard, 1982; The Population Commission, 1938). Fertility declined by 25% in Denmark in the beginning of the 1900s as shown in Figure 1. From 1921 to the mid-1930s, the crude birth rate in Denmark dropped from 24 live births per 1000 people to 18. However, from 1940 and the following 5 years the crude birth rate recovered to the 1921 level. This development can be tracked in both urban and rural areas (right panel). The crude birth rate in urban areas increased more than in rural areas.

The low birth rate concerned politicians and to develop solutions they formed the Population Commission in 1935 (Skalts & Nørgaard, 1982). The Population Commission was instrumental in the formation of the family planning program. The Commission had three main recommendations. First, direct financial support for mothers. Second, clinics to teach basic sexual education

[3] Lindo et al. (2017) estimate statistically significant effects on abortion but argue that the effects are too small to detect them in the birth rate. Packham (2017) finds that restricted access increases teen birth rates by 2–3%. Fischer et al. (2018) and Lu and Slusky (2019) also estimate positive fertility effects from restricted access using the same source of variation.

[4] The right to remain anonymous was abandoned in 1938.

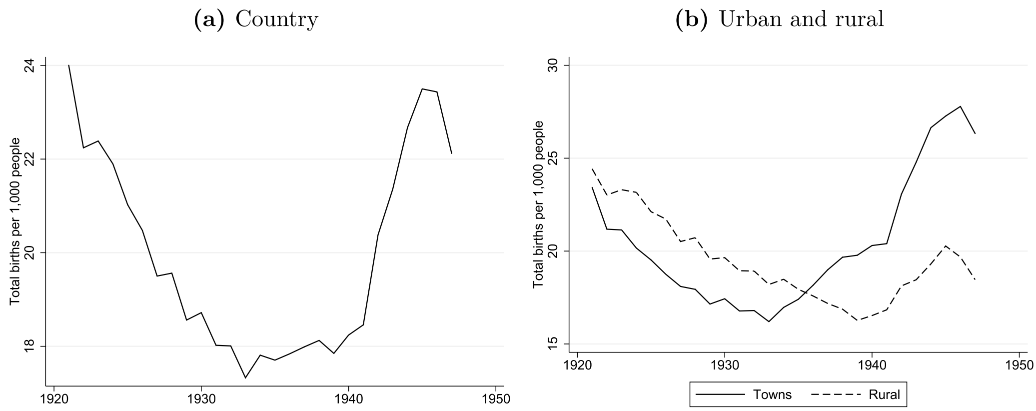

Figure 1. The total crude birth rate in Denmark: (a) country and (b) urban and rural.
Notes: Own calculations based on data from The Medical Report for the Kingdom of Denmark, various years.

across the country. Third, an expansion of family planning services to provide aid and care to pregnant women. The only recommendation implemented immediately – and throughout the period considered in this paper – was to increase the availability of family planning services. Abortion was illegal and only became legal in 1973 well past the study period of this paper.

2.3. The family planning program

The Danish parliament adopted the government-funded family planning program by law in 1939. Implementation began on 1 April 1939 where the public healthcare sector took over and expanded a private organisation in Copenhagen called *Mothers Aid Institutions*. Branches opened in the five largest Danish towns and two medium-sized towns – Sønderborg and Næstved.[5] Financial considerations restricted the implementation although the aim was to eventually expand across the country. A key feature in Sønderborg was that a public birth ward had opened in 1934 (Medical Report for the Kingdom of Denmark, 1934). Sønderborg was the first town outside the five largest towns to have a public birth ward which may have been important in the allocation of the family planning program in Sønderborg as the presence of a well-developed health care infrastructure was a factor in the allocation process (The Population Commission, 1938). Denmark was occupied as part of the German expansion during World War 2 from 1940 to 1945. Sønderborg is close to the German border and would likely have had a larger troop presence during the Occupation compared to Næstved. As a sanity check and to ensure that the effect is driven by the program and not the Occupation having two different treatment towns is important.

When designing the program, The Commission had three targets in mind: (i) accessing unmarried pregnant women; (ii) preventing illegal abortions; and (iii) improving the public perceptions of non-marital pregnancies. The Commission feared that unmarried and abort-wanting women would not contact the program because of their decision to abort. Handing the power to grant legal abortions to the program gave the targeted women incentives to contact the program. Thus the program acted as the entrance into the abortion system. When in contact, the staff should advice against aborting illegally. Abortion was illegal unless severe medical reasons justified it. It was debated heavily whether social considerations should be taken into account but a law from 1937 only had medical reasons as a factor (Skalts & Norgaard, 1965). Besides the legal issue, the program

[5]The five largest towns in Denmark were Copenhagen (capital), Aarhus, Aalborg, Odense and Esbjerg. The five largest towns were chosen as locations for the program on the grounds of demand and efficiency. Their large populations secured demand and social workers, hospitals and so on could increase efficiency by complementing the family planning program. The Population Commission note that Sønderborg and Næstved have the potential to establish the required infrastructure in the future.

provided in-kind aid (milk, food and clothes), guidance through the adoption system and legal aid in paternity cases to provide women with alternative options to abortion. Finally, to improve the public perception of non-marital pregnancy, the program was available for all pregnant women and had high-quality staff. The program was free of charge and available for all women regardless of socio-economic background such as wealth and marital status. Women could attend even if not resident in the town or county of the institution. However, the content of the program would most likely appeal to poor and unmarried women who were in doubt as whether or not to keep the child.

The program was designed based on recommendations described in The Population Commission (1938). The Population Commission (1938) highlighted a need to change the social stigma associated with non-marital birth-giving by showing and signalling that society equally cared for and valued children born outside marriages. The commission did not regard financial aid to be an effective instrument in this pursuit but recommended a family planning program to help legitimise non-marital pregnancies, births and children. Similarly high on the agenda was to reduce illegal abortions among unmarried pregnant women, as this would have a direct positive effect on the birth rate. In their report from 1938 an entire section '*Family planning as a mean against abortions*' deals with the exact strategy. The following points summarise the program: First, to get in contact with women without ambitions to keep their children and make them attend the program by placing the authority to grant legal abortions within the program. Second, to advice women with a wish to abort – but were not granted a legal abortion – to keep the child by explaining the dangers of illegal abortions and that they could face legal prosecution and by providing alternatives. Third, to make the program an integrated and socially accepted part of every pregnancy by being a universal offer and by attracting married and well-off women, too. Fourth, to facilitate acceptance, usage and impact by employing educated and professional staff ranging from doctors to midwives, nurses and social workers. Fifth, to provide health examinations and advice on topics such as nourishment and hygiene. Sixth, to provide legal counsel in paternity cases to inform unmarried pregnant women on their legal rights as potentially single parents.

The program lowered the cost of non-marital birth-giving compared to having illegal abortions by offering adoption options, in-kinds and legal assistance for unmarried pregnant women. This cost reduction could increase the non-marital birth rate by incentivising more women to avoid a costly, risky and illegal abortion in favour of giving birth to either raise the child or give it up for adoption. Marital fertility was not targeted with any specific actions and should show minor or zero response to the program. However, we might imagine two forces pulling in opposite directions. First, legal advice in paternity cases could improve the incentive for men to marry their pregnant partner as the likelihood of legal and financial obligations increased with the introduction of family planning. Second, easier access to plausibly less costly pathways for single mothers (adoption and in-kind aid) could decrease the willingness for men to engage in marriage (Akerlof et al., 1996), if fathers care about the utility of the mother and see these services as utility improving for unmarried mothers. The same two forces affect the number of marriages – in particular shotgun marriages – as legal advice in paternity cases could make it more costly for fathers not to marry the mother after birth. If so, then some of the response in non-marital fertility could be traded in to more marriages. The empirical analysis investigates these hypotheses in depth.

3. Empirical method

The synthetic control method (Abadie, 2021; Abadie et al., 2010, 2015; Abadie & Gardeazabal, 2003) is a technique to estimate treatment effects in comparative case study settings. The interest in comparative case studies is the evaluation of a treatment (intervention) at an aggregated level (town, regions, country and so on) where treatment occurs at a single point in time and exposes a few units. Another common feature is that the pool of untreated units are different with respect to pre-treatment trends in outcome and other characteristics compared to the treated unit(s) and thus do not constitute suitable control units on their own.

While difference-in-differences (DiD) is the traditional framework to estimate treatment effects when panel data is available and when treatment varies across time and space, I use the synthetic control method instead of DiD for several reasons. First, the introduction of the family planning program in the two medium-sized towns fits the description of a comparative case study. Second, the synthetic control method optimally identifies a control group allowing the counterfactual to be based on a set of control units that most accurately resemble the treated unit prior to treatment. Third, inference in the synthetic control method relies on randomisation – and not asymptotic inference – which is suitable in small sample sizes (Conley & Taber, 2011).

The basic application of the synthetic control method is the case of a single treated unit, a number of control units and treatment occurring at a single point in time. In period T_0, the treated unit is exposed to a treatment. J units are untreated and remain so for the entire period of interest. The sample of untreated units is the donor pool. Thus the total sample contains $J + 1$ units. These units are observed in periods $T_\tau, T_{\tau+1}, \ldots T_{-1}, T_0, T_1, \ldots, T_T$. $T_\tau, \ldots, T_{-1}$ is the pre-intervention period and $T_0, T_1 \cdots, T_T$ the post-intervention period. Let y_{it}^N be the outcome in the absence of treatment and y_{it}^I the outcome under treatment. Before treatment $\{T_\tau, \ldots, T_{-1}\}$, these outcomes are equal $y_{it}^I = y_{it}^N$ if units do not respond with anticipation to future treatment. The treatment effect for the treated unit in period $t \geq T_0$ is

$$\alpha_{it} = y_{it}^I - y_{it}^N \tag{1}$$

In practice either y_{it}^I or y_{it}^N is observed. Specifically, in the post-intervention period, it is necessary to estimate y_{it}^N to calculate the treatment effect for the treated unit because $y_{it} = y_{it}^I$ for $t \geq T_0$. The post-intervention outcomes for the synthetic control unit can be used as an estimate for the treated units counterfactual outcome. The estimated treatment effects are then

$$\hat{\alpha}_{0t} = y_{0t} - \sum_{j=1}^{J} w_j y_{jt} \tag{2}$$

The optimal synthetic control unit implies choosing a set of weights that minimise the weighted discrepancy between the treated unit and the synthetic control units pre-intervention outcomes and covariates,

$$W^* = \arg\min \sqrt{(X_I - X_N W)' V (X_1 - X_N W)} \tag{3}$$

where X_I is a $(K \times 1)$ vector of pre-intervention covariates and outcomes for the treated unit. X_N is a $(K \times J)$ matrix of the same pre-intervention covariates and outcomes for the J units in the donor pool. W is a $(J \times 1)$ vector of weights which are restricted to sum to 1. V is a $(K \times K)$ positive and semidefinite matrix. V assigns weights to each linear combination and reflects how important a specific variable in X is for the prediction of the evolution of the outcome. In practice V is chosen to minimise the root mean squared prediction error (RMSPE) between the pre-intervention outcome for the treated unit and the synthetic control.

Inference in the synthetic control method should reflect uncertainty in the estimated treatment effects. This uncertainty arises due to uncertainty concerning the validity of the synthetic control unit and the implied counterfactual. Abadie et al. (2010) suggest to evaluate significance based on exact inference through placebo or permutation tests. For each unit in the donor pool, I estimate placebo treatment effects. The true treatment effect is significant if it stands out as an extreme event in the distribution of placebo effects. A second graphic representation of inference is to evaluate the distribution of the ratios of post and pre-intervention root mean squared prediction errors (RMSPE) for all units in the donor pool. The estimated treatment effect is significant if the ratio for the treated unit is an extreme observation in the distribution. This approach can be used to calculate rudimentary p-values for the treatment effect by calculating the probability of estimating a pre/post RMSPE ratio at the size of the true effect if treatment was assigned at random.

The synthetic control method requires three assumptions. First, the treatment of one unit does not spill-over to the outcomes of untreated units (similarly to the stable unit treatment value assumption (SUTVA) in the potential outcomes framework, see Rosenbaum, 2007 for details). If violated, the estimated effect cannot be interpreted as the causal effect of the treatment. There exist no formal test to assess this assumption but its validity should be argued on background knowledge from the context of the study. In practice, I deal with this assumption by using aggregation levels where spill-overs between units in the sample are less likely. Specifically, I estimate at both town and county levels. In the county specification, I allow for within-county spill-overs but spill-over across counties are not allowed (that is selective mobility of pregnant women across counties).

Second, treated units do not respond in anticipation of the treatment. In my case, the implementation of the program fell in the same year as the law was passed which limits the scope for the anticipatory behaviour.

Third, treated units' counterfactual can be estimated by a weighted average of donor pool units post-treatment outcomes. The validity of this assumption can be evaluated by the quality of the pre-treatment fit between the treated unit and the synthetic control unit. Abadie et al. (2010) and Abadie (2021) note that in some cases it may be impossible to construct a suitable synthetic control unit due to poor pre-treatment fit. In such instances, they recommended not to use the method. However, it lies with the researcher to subjectively decide the quality of the fit and whether to proceed with the analysis. I evaluate the fit of the synthetic control unit by graphical inspection and when the pre-treatment fit is bad, I avoid causal claims.

Furthermore, Abadie et al. (2010) mention that interpolation bias may be present even if the pre-treatment fit is good. To minimise interpolation bias, Abadie et al. (2010) recommend that the treated unit(s) and the donor pool should be somewhat comparable prior to the implementation of the synthetic control method. This implies that researchers should restrict the donor pool to units with pre-treatment characteristics in the neighbourhood of the treated unit. To comply with this recommendation, I evaluate the effects of the family planning program for the two medium-sized towns (Næstved and Sønderborg) while excluding the five largest – and treated – towns.

A critique of the synthetic control method is a lack of guidance when choosing the predictors and their functional forms (Ferman et al., 2018). Ferman et al. (2018) recommend specifications where the number of predicting pre-treatment outcomes increases when the pre-treatment period increases and that the sensitivity of the specification is thoroughly tested.[6]

4. Data and descriptive statistics

4.1. Sample construction

I combine data from several sources. From *Causes of Death Statistics*, I obtain demographic variables (population size, number of live births and number deaths) across Danish towns and counties. Counties are administrative regions and may include several towns, villages and rural areas. From *Income & Wealth Tax Records*, I collect annual income and wealth data from 1921 to 1936 at town level.[7] From *Business Statistics* – a decennial publication – I obtain data on workforce sector shares. From *Marriages, Births & Deaths* and *Births, Deaths & Population Movements*, I digitise the number of marriages and non-marital live births at county and town levels.[8] *Medical Reports for the*

[6]Specifications to avoid: the mean of the pre-treatment outcome on its own and specifications rules such as the first, the middle and the last value of the pre-treatment outcome.

[7]In 1936, the outlook of the publication changes. Therefore, I stop in 1936 which also coincides relatively well with treatment initiation in 1939.

[8]From 1910 to 1925, these data are available in *Marriages, Births & Deaths* and from 1934 onward in *Population Movements*. In the intermediate period (1930–1933), marriage and non-marital live birth data are collected directly from original documents at The Danish National Achives, Statistics Denmark. The remaining years (1926–1929) are interpolated using the number of total live births and population.

Kingdom of Denmark contain data on aggregated and county-level population and demographic variables that I use for descriptive statistics and in the county-level analysis. The reports also include data on the number of children in different kinds of out-of-home care.

The final panel data set contains yearly observations from 1921 to 1947 for 27 Danish towns with a population of at least 7000 and 16 counties (excluding the top-five largest towns and their counties). Denmark had 87 market towns at the time. The restriction from 87 market towns to the 27 towns included in the analysis is due to data availability. I collect the birth rate data split by marital status from hand-written documents from the National Archives which only included these 27 medium-sized towns. However, the towns excluded are the smallest Danish towns and thus unlikely to be suitable control units for medium-sized towns why the loss of the smaller towns likely does not matter the results. I exclude no medium-sized towns in the analysis. Sønderborg is remotely located far away from large towns like Copenhagen, while Næstved is closer to Copenhagen. The two treated towns are located far apart and towns in the donor pool are scattered across Denmark.[9] In the study period, Denmark had 21 counties. Again we exclude the five counties with the top five major cities leaving us with a sample of 16 counties where two counties are treated.

As outcomes, I use the crude marital birth rate, the crude non-marital birth rate, the share of non-marital births and the number of marriages. I define the crude (non-)marital birth rate as the number of (non-)marital live births per 1000 people. The share of non-marital births is the number of non-marital births as a share of the total number of live births.

An important data issue arises from the reporting practices used in the majority of years in the sample (1921–1943). In 1921–1943, the number of live births – both non-marital and marital – is reported at birth-place level. In 1944–1947, the numbers are reported both in terms of birth place and residence. Thus I have to use birth-place data in the analyses. A significant treatment effect in the birth-place measured birth rates can be caused by two potential channels; (i) changes in fertility and/or (ii) changes in the number of births in the town by women not living in it. Though, home births were the norm at the time (Vallgårda, 1996) supervised births outside of home cannot be excluded a priori. I try to pin down the exact channels by estimating the treatment effects at both town and county levels to reduce the discrepancy between birth place and residence. Counties are larger geographical areas than towns. A county includes rural upland and often several towns. Thus pregnant women would have to travel beyond their home county at the time of birth in order for me not to be able to interpret the county-level results as actual fertility effects.

The synthetic control method uses both pre-treatment outcomes and relevant pre-treatment covariates (correlated with the outcome) in the construction of the synthetic control unit. As pre-intervention covariates, I use income and wealth per capita (yearly), the population density and population size (selected years and averaged), industry workforce share (selected years) and the share of women (selected years). Income and wealth are important predictors for fertility (Becker, 1981). The share of women places a natural restriction on fertility. Higher population density decreases fertility through access to education, better infrastructure and health services (De la Croix & Gobbi, 2017). The industry workforce share might proxy the degree of industrialisation and development at town level. These measures might predict the development of fertility and also the responsiveness of the treatment. Furthermore, I include pre-intervention outcomes as predictors.

4.2. Summary statistics

Table 1 shows summary statistics for the two treated towns and the donor pool. There are relatively large differences between both treated towns and the donor pool. Towns in the donor pool are more densely populated compared to the treated towns. Income and wealth per capita are relatively similar in the treated towns and in the donor pool. Both treated towns are more industrialised than the

[9]Online Appendix Figure A1 shows a map of Denmark and the geographical location of towns in the sample.

Table 1. Pre-intervention descriptives for treated towns and donor pool.

	Means		
	Sønderborg	Næstved	Donor pool
Population density	53.89	85.73	116.85
Income per capita	992.33	943.24	898.35
Wealth per capita	2235.27	2040.43	2205.06
Female share	0.53	0.52	0.53
Industry workers per 1000	185.50	212.50	166.70
Population	10,607.72	11,229.72	14,382.51
Non-marital birth rate	1.97	1.56	1.53
Marital birth rate	22.29	17.19	17.47
Share of non-marital births	0.08	0.08	0.08
Marriages per capita	9.26	9.18	8.46

Notes: The pre-intervention period is 1921–1938. Income and wealth per capita are averaged across 1921–1936. Industry workers per capita are averaged across 1925 and 1935. Women shares are averaged across 1921 and 1925 and across 1930 and 1935 and population densities are averaged across 1925, 1930 and 1935. Population is averaged over 1921–1938. The means for the non-marital and marital birth rate, share of non-marital births and the marriage rate are averages across 1921–1938. The donor pool includes 25 towns. From the original sample of 32 towns, I exclude the top 6 largest Danish towns and the two treated towns.

average donor town but with smaller populations. The bottom-four rows show means for the pre-intervention outcomes. Sønderborg has higher non-marital and marital birth rates compared to Næstved and the donor pool. Næstved and the donor pool have similar non-marital and marital birth rates before the family planning program. The share of non-marital births is 8% in both treated towns and the donor pool while the marriage rates are slighter higher in the treated towns.

A key feature in Sønderborg was that a public birth ward opened in 1934 (Medical Report for the Kingdom of Denmark, 1934). Sønderborg was the first town outside the five largest towns to have a public birth ward. The birth ward might have been important in the allocation of the family planning program in Sønderborg as the presence of well-developed health care infrastructure was a factor in the allocation process (The Population Commission, 1938). The presence of the birth ward may either increase or reduce the impact of the family planning program depending on whether the birth ward and the family planning program are complements or substitutes.

4.3. Descriptive evidence

Figure 2 presents the relationship between population size and the number of non-marital births in averages before and after the family planning program for all towns in the donor pool and the two treated towns. Panel (a) shows the average number of non-marital births reported at birth-place level in 1921–1930. Panel (b) shows the same relationship but uses fertility data reported at residence level in 1944–1947.[10]

Figure 2 shows that population size and the number of non-marital birth follow a close linear and positive relationship. Prior to the program the number of non-marital births in the treated towns lies within the linear relationship. After treatment, both treated towns are to the right of the relationship between population and non-marital births, even when measured at residence level in panel (b). Compared to control towns with the same population, the treated towns have almost 100 more non-marital births.[11] The presented figures demonstrate a close link between

[10]In the residence-level plot in panel (b), I reduce the pre-intervention period to 1921–1930 to reduce the potential effect of mobility of pregnant women from the areas surrounding the towns while the post-intervention period is reduced to 1944–1947 where residence-level data is available.

[11]Online Appendix Figure A2 shows the same relations for the number of marital births. Only Sønderborg – and not Næstved – breaks the linear relationship between population size and marital births in the post-treatment period and only when measured at birth-place level.

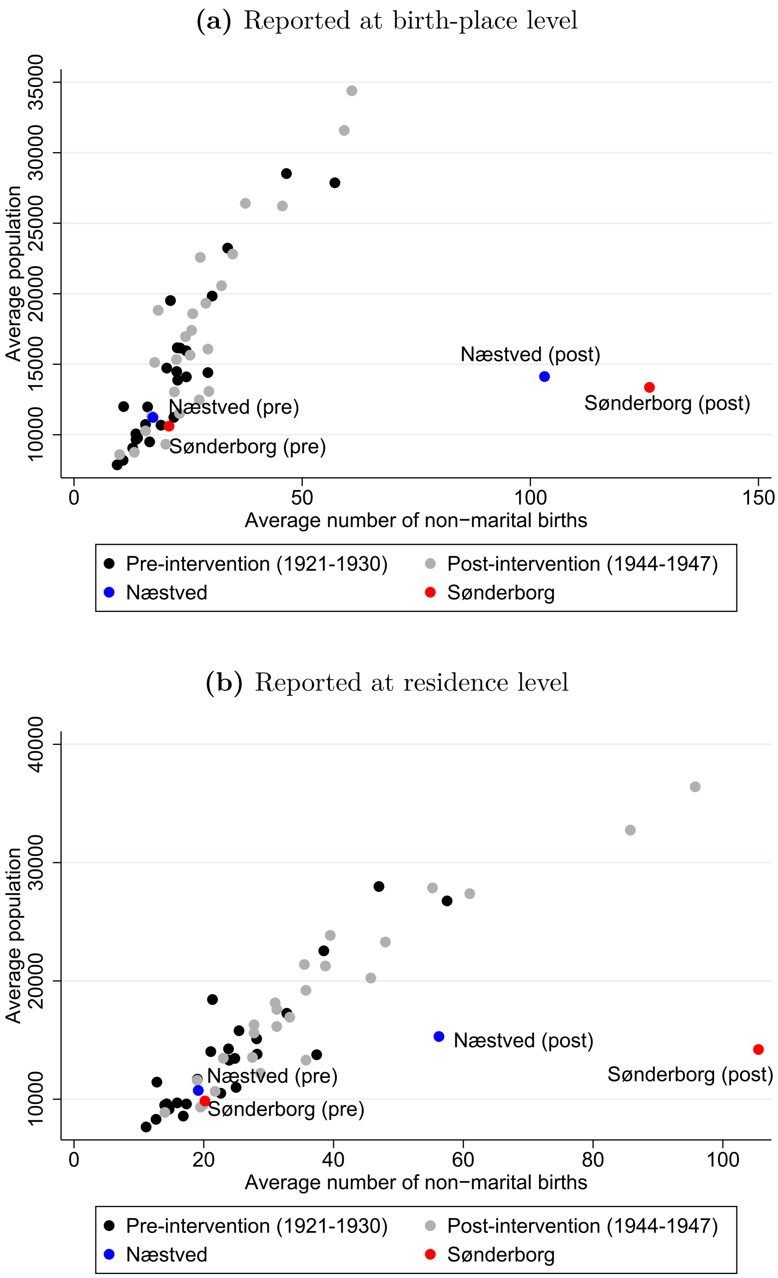

Figure 2. Pre/post intervention relationship between population size and non-marital births: (a) reported at birth-place level and (b) reported at residence level.

Notes: Own calculations based on data from Marriages, Births & Deaths, Population Movements and The Causes of Death Statistics (all various years). The pre-intervention period is 1921–1938 and the post-intervention period is 1939–1947. In panel (b), the non-marital birth at residence-level averages is taken from 1921 to 1930 and 1944 to 1947 due to data limitations. Blue dots are observations for Næstved and red dots are Sønderborg.

population size and the number of non-marital births in Danish towns and that the treated towns were no different from other Danish towns in this regard prior to the introduction of the program. Furthermore, a few years after program implementation the two treated towns have markedly more non-marital births than would have been expected based on their population sizes which indicates that the program had a positive effect on non-marital fertility.

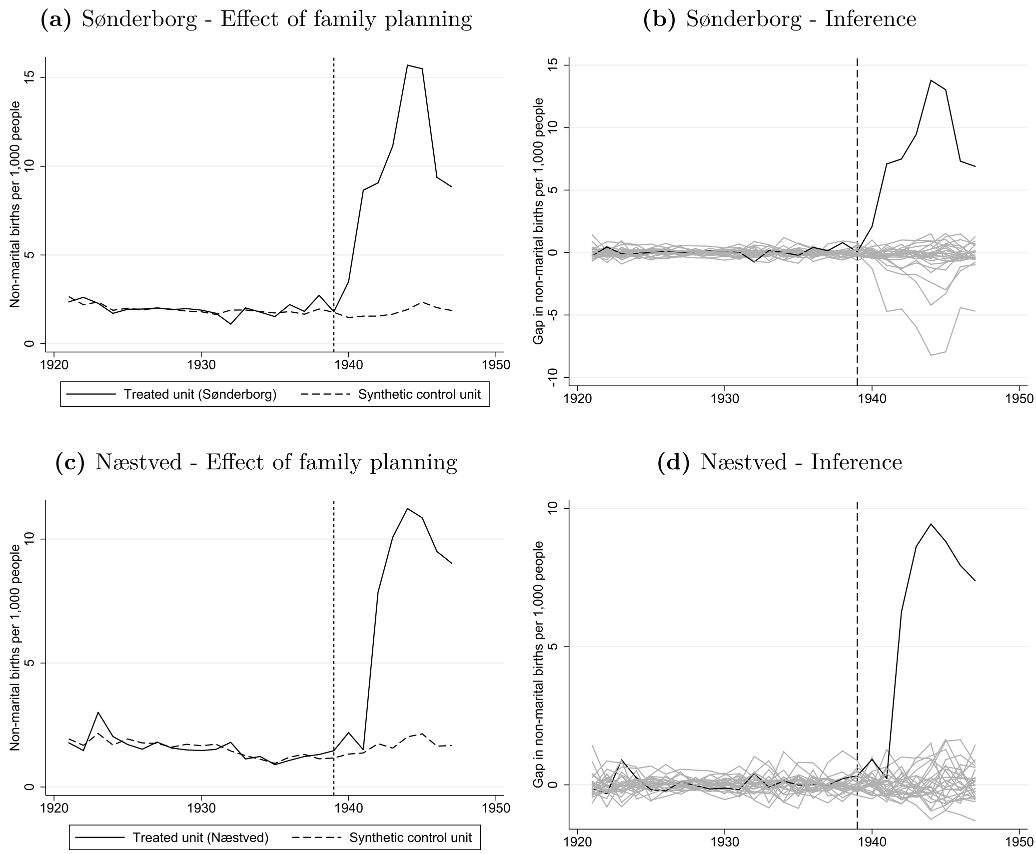

Figure 3. Evolution of the crude non-marital birth rate at town level: (a) Sønderborg – Effect of family planning; (b) Sønderborg – Inference; (c) Næstved – Effect of family planning and (d) Næstved – Inference.

Notes: The treatment effect of the family planning program is estimated using the synthetic control method. The pre intervention period is 1921–1938 and post-intervention 1939–1947. The following variables act as covariates; Income and wealth per capita are averaged across 1921–1936. Industry workers per capita are averaged across 1925 and 1935. Women shares are averaged across 1921 and 1925 and across 1930 and 1935 and population densities are averaged across 1925, 1930 and 1935. Population is averaged across 1921–1938. The pre-intervention outcome is included as a predictor for the years 1922,1924, 1928, 1930, 1934–1938. The donor pool includes 25 towns. Right panels show inference based on placebo testing.

5. Results

5.1. Non-marital birth rate

Figure 3 displays the evolution of the crude non-marital birth rate at town level for the two treated towns and their synthetic control towns and show that the family planning program increased the non-marital birth rate in both treated towns. Overall, the effects of the family planning program in the two towns are similar. The crude non-marital birth rate is fairly constant at 2 births per 1000 people prior to the program. Shortly after treatment initiation, the crude non-marital birth rate in the treated towns starts diverging from the synthetic controls. The gap widens until 1945 from where it narrows a bit. In 1945, the number of non-marital births per 1000 people was above 15 in Sønderborg and above 10 in Næstved. These are fairly substantial treatment effects of access to the family planning program. The right graphs of Figure 3 display randomisation inference and the dynamic treatment effects. The effects are highly significant as the true effects are extreme events in the distribution of placebo effects. The impact of the program is gradual in the first few years before plateauing and slightly decreasing indicating that the program took time to reach its full effect after introduction.

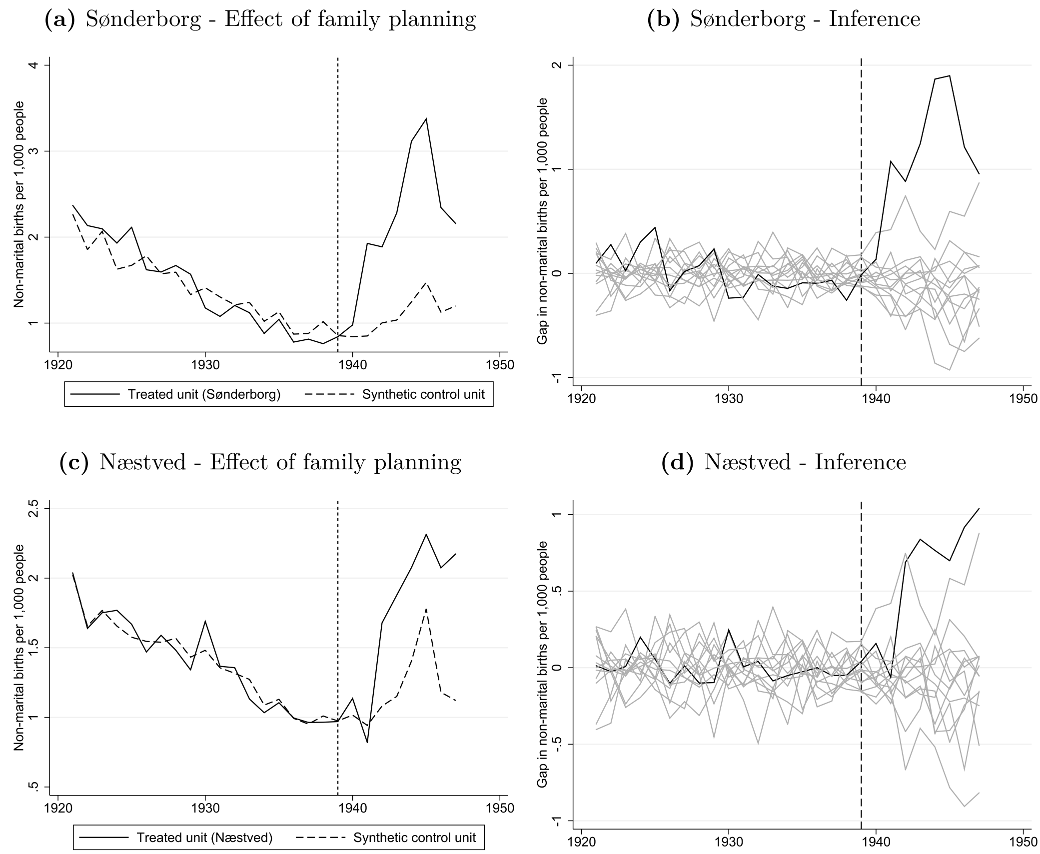

Figure 4. Evolution of the crude non-marital birth rate at county level: (a) Sønderborg – Effect of family planning, (b) Sønderborg – Inference, (c) Næstved – Effect of family planning and (d) Næstved – Inference.

Notes: The figures shows treatment effects and inference of the family planning program. The unit of treatment is counties. Panels (a) and (b) show results for Sønderborg and panels (c) and (d) for Næstved. As covariates average population and yearly outcomes are used. Right panels show placebo inference where the black solid line indicates the true treated county. The donor pool includes 14 untreated counties.

The effects in Figure 3 might be caused entirely by an influx of pregnant unmarried women from surrounding areas into the treated town at the time of birth. To detect if the family planning program increased non-marital fertility, I increase the aggregation level to counties. Figure 4 presents the estimated treatment effects on the non-marital birth rate at county level. The family planning program significantly increased the non-marital birth rate at county level. The treatment effects are between 1 and 2 non-marital births per 1000 people when estimated at county level and the effects are significant as shown in the right panels. The effect on the non-marital birth rate is smaller in Næstved than Sønderborg. The results indicate actual non-marital fertility responses and are in line with the content of the program: to increase fertility by targeting abort-seeking - and often unmarried women – and advising them to give birth.[12] Online Appendix Tables A1–A2 show the synthetic control weights for all combinations of fertility outcomes (non-marital birth rate, marital birth rate and non-marital birth share), treated unit (Sønderborg and Næstved) and aggregation level (town and county). In all estimation, the weights are sparse, i.e. most assigned weights are zero and relatively few control units contribute to the synthetic control which is typically not the case in

[12]Figure A3 in the Online Appendix provide further support that the estimated effects are significant. Figure A3 contains inference based on the ratios of RMSPE for the effects on non-marital birth rate at both aggregation levels in both treated areas.

DiD regressions (Abadie, 2021). Sparsity allows for an accurate interpretation of the estimated counterfactual.

The estimated number of additional children born outside of marriage in the treated counties can be calculated based on these results.[13] In the county of Sønderborg, 821 children were born from 1939 to 1947 caused by the increase in non-marital fertility. In the county of Næstved, the number is 529 children. In total, the estimate across the two counties is 1350 children.

5.2. Marital birth rate

Figure 5 shows the synthetic control analysis for the marital birth rate at town level. In panel (a), a gap opens for Sønderborg between the two groups from 1934, which is 5 years before treatment. The gap coincides with the construction of the birth ward. I cannot conclude how the marital birth rate in Sønderborg was affected by the family planning program due to the poor pre-treatment fit of the synthetic control.[14] In Næstved, there is evidence of a positive effect on the marital birth rate reaching above 10 marital births per 1000 people in 1947. From a pre-treatment level of 20 marital births per 1000 people this corresponds to a 50% increase. However, the divergence between Næstved and the synthetic control occurs prior to treatment.[15] Overall, the town-level results do not provide enough evidence for me to establish whether the program affected the marital birth rate.

To investigate if marital fertility responded to the program, I estimate the treatment effects using county-level data. Figure 6 shows the results: in neither of the treated counties did the crude marital birth rate respond significantly. This result allows me to conclude three things: (i) marital fertility was unchanged by the program, (ii) increases in the town-level marital birth rate is caused by within-region movements of pregnant women into the treated town and (iii) the 1934 birth ward in Sønderborg only attracted pregnant women from the surrounding municipalities but had no effect on fertility.

5.3. Non-marital birth share

Non-marital fertility increased and marital fertility was unchanged in response to the program. These responses imply that the share of non-marital births must have increased relatively in the treated towns. Figure 7 shows how the family planning program affected the share of non-marital births. The evidence supports the previous patterns as the share of non-marital births in the treated towns went from 6–7% to over 20% while the share stayed roughly constant in the synthetic control towns. The effects are highly significant as shown in the right panels as none of the placebo runs are close to matching the magnitude of the true effects. The fact that I once again obtain the same results across the two treated towns are strong indications that the effects are caused by the family planning program.

The town-level results might be biased if the degree of mobility of non-residential pregnant women differs across married and unmarried women. To account for this, I estimate the effects on the share of non-marital births at county-level presented in Figure 8. At county level, the estimated effects range from 5–8% points in Sønderborg and 3–4% points in Næstved.[16] At town level, the estimated effects are homogeneous across the two towns. The heterogeneous effects at county level are caused by mobility from pregnant non-residential women which seems to be more prevalent in Næstved than Sønderborg. Figure 9 shows the post-treatment average non-marital birth rate

[13]For each year after the introduction of the program, I multiply the estimated effects α_{0t} with the population at time t. Afterwards, I accumulate the annually estimated non-marital births caused by the program. This total is an estimate of the number of children born outside of marriages from 1939 to 1947 as a consequence of the family planning program.

[14]The estimated effect is not significant based on inference from the ratios of RMSPE as presented in the Online Appendix Figure A4.

[15]Figure A4 panel (b) shows inference from the ratios of RMSPE. The probability (p-value) of estimating a ratio the size of the treated town or larger by random treatment assignment is $2/26 = 0.08$ which is borderline significant on usual confidence levels.

[16]Inference based on RMSPE supports that all the estimated effects are significant as shown in Online Appendix Figure A6.

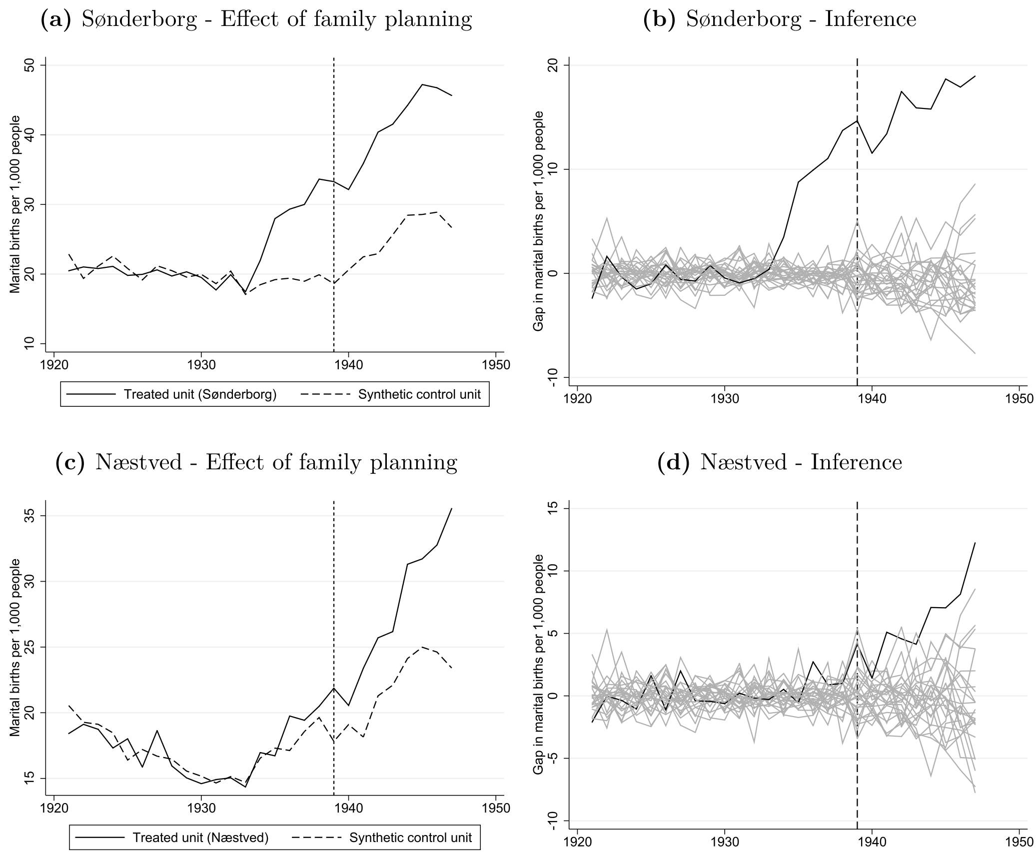

Figure 5. Evolution of the crude marital birth rate at town level: (a) Sønderborg – Effect of family planning, (b) Sønderborg – Inference, (c) Næstved – Effect of family planning and (d) Næstved – Inference.

Notes: See notes to Figure 3.

at both birth place and residence level for donor pool towns and the two treated towns.[17] The difference between the birth-place and residence-level birth rates indicates the degree of mobility from non-residential pregnant women. The figure shows that this type of mobility from unmarried pregnant women is more prevalent in Næstved than Sønderborg.[18] Figure 9 shows that approximately half of the town-level increase in the non-marital birth rate caused by the program can be attributed to mobility of non-residential unmarried pregnant women in Sønderborg while 4/5 of the increase in Næstved town can be attributed to this type of mobility.

5.4. Additional outcomes

As additional outcomes, I use marriages, non-marital infant mortality and total fertility.[19] I estimate the effects on the marriage rate to determine if the increase in non-marital fertility is subsequently traded into more marriages. If not, then the extra number of children born outside of marriage grew

[17]The statistics are calculated based on data from 1944 to 1947 due to data availability. For more information consult the data section.
[18]Online Appendix Figure A5 shows that marital births from non-residential women are a much more common phenomenon in Sønderborg than Næstved most likely caused by the birth ward.
[19]I only show results for Sønderborg. However, the results for Næstved are similar and available on request. I estimate the effect at county level as that is the aggregation level where I draw my fertility conclusions from. Town-level results are available on request.

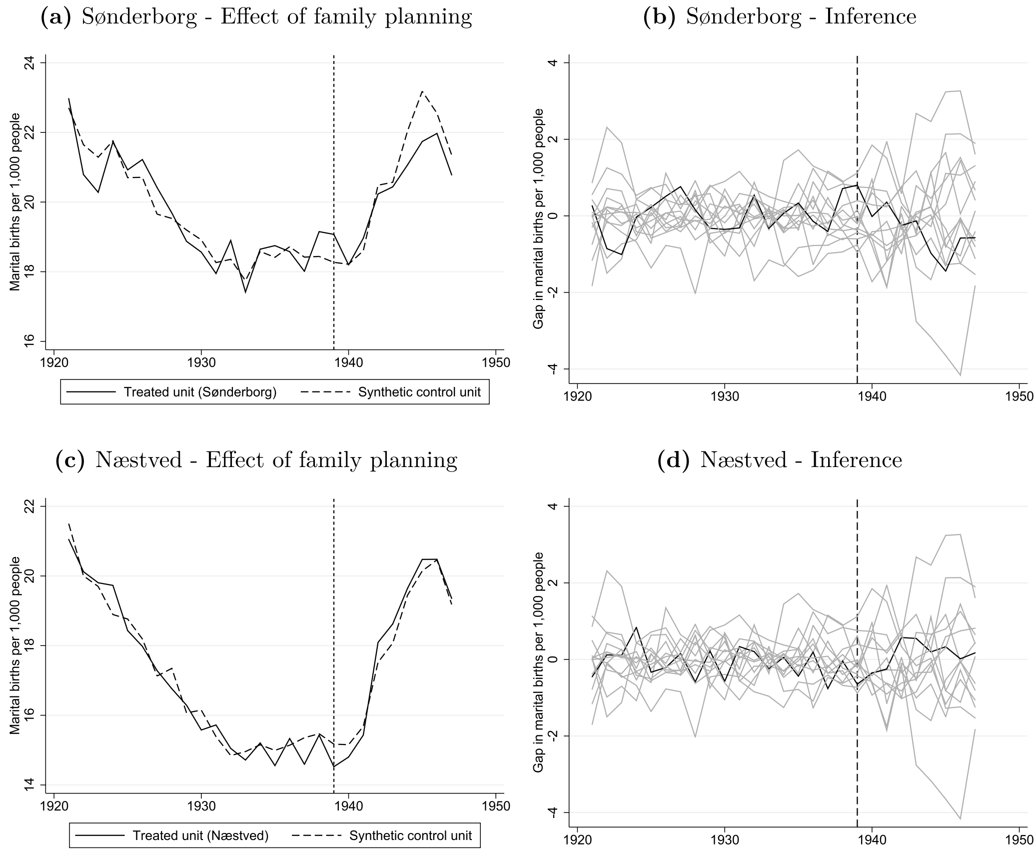

Figure 6. Evolution of the crude marital birth rate at county level: (a) Sønderborg – Effect of family planning, (b) Sønderborg – Inference, (c) Næstved – Effect of family planning and (d) Næstved – Inference.

Notes: See notes to Figure 4.

up in non-marital arrangements or with adoptive parents. Figure 10 panel(a) shows the estimated treatment effects on the number of marriages per 1000 people along with inference.

There is no significant evidence to suggest that the number of marriages increased in response to the program. The estimations are noisier and the pre-treatment fit is bad but the treated county did not experience significantly different marriage rates compared to the synthetic controls. This indicates that the children born, as non-marital fertility increased, grew up in non-marital living arrangements.

The family planning program could impact infant mortality of children born outside of marriages in two opposite directions. First, the health of infants might improve due to free-of-charge access to health professionals which evidence indicates has health benefits in a similar context (Hjort et al., 2017; Wüst, 2012). Second, children who would otherwise have been aborted are now being born. These children are most likely children with the worst health and thus increase non-marital infant mortality. However, I find that the family planning program did not have any significant impact on non-marital infant mortality as seen in panel (b) of Figure 10.

As marital fertility in general increased in the post-treatment period, panel (c) in Figure 10 investigates whether the increase in non-marital fertility was sufficiently large to positively impact total fertility in the treated areas. The effect on total fertility is not significant. The introduction of the program coincides with the early baby boom (Van Bavel & Reher, 2013) and adds to a literature on the determinants of the baby boom (Albanesi & Olivetti, 2014, 2016). Thus the program may

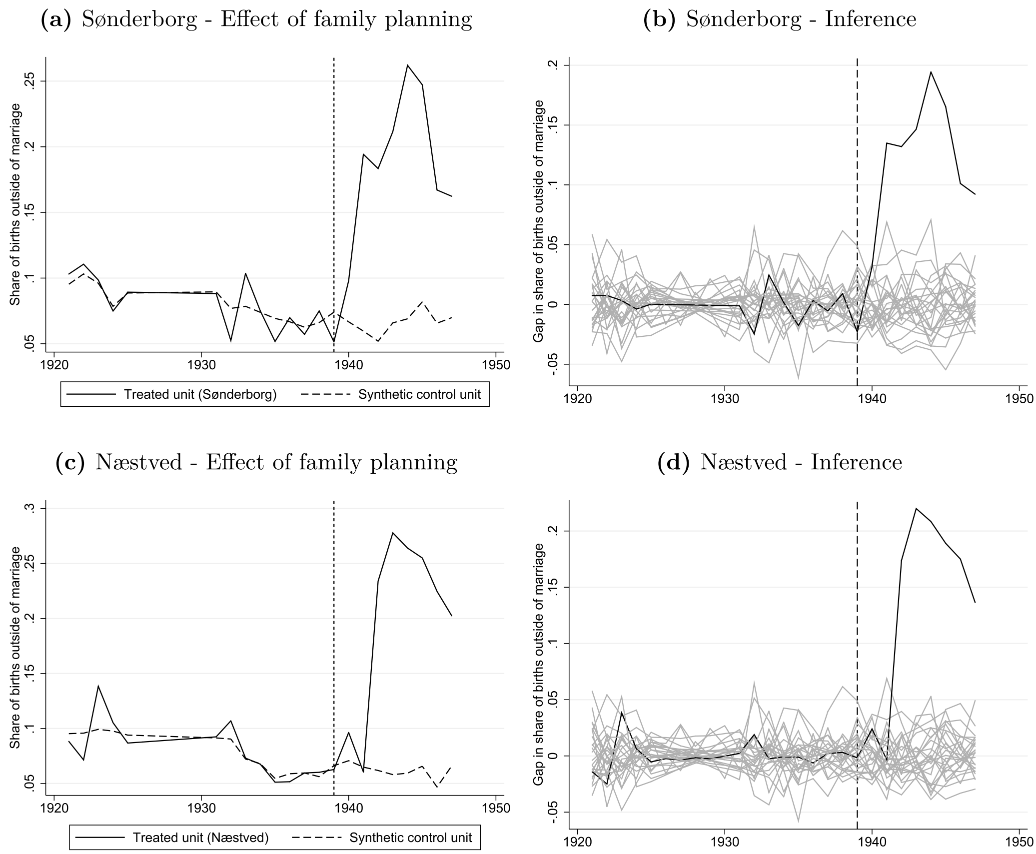

Figure 7. Evolution of the share of non-marital births at town level: (a) Sønderborg – Effect of family planning, (b) Sønderborg – Inference, (c) Næstved – Effect of family planning and (d) Næstved – Inference.

Notes: See notes to Figure 3.

have contributed to the fertility development during the Second World War. However, as I find no significant increase in the treated areas the family planning program was not instrumental in explaining the overall increase in fertility from 1939.

5.5. Robustness

I perform several robustness checks to assess the sensitivity of the results. First, I estimate placebo effects on outcomes that are unrelated to the treatment. Null effects strengthen the causal interpretation of the estimated fertility effects while significant effects indicate that other uncaptured differences between the treated and synthetic control units might explain the previous results.[20] Online Appendix Figure A7 shows results on two placebo outcomes: (1) the overall death rate and (2) the death rate from suicides, homicides and accidents. I find no significant effects in either placebo tests.

I test the sensitivity of the synthetic control specification using five different specifications.[21] For each specification, I rerun the synthetic control procedure and compare the estimated treatment

[20] The placebo estimations are only carried out and showed for Sønderborg. The same set of results for Næstved are available on request.

[21] The five specifications are: (1) pre-treatment outcome values for all years, (2) the first 3/4 of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values and (5) even pre-treatment outcome values. Note that these five specifications are different from the one I use in the main specification. My specification is more sporadic in the middle of the sample and denser closer to the intervention and at the start of the sample and therefore

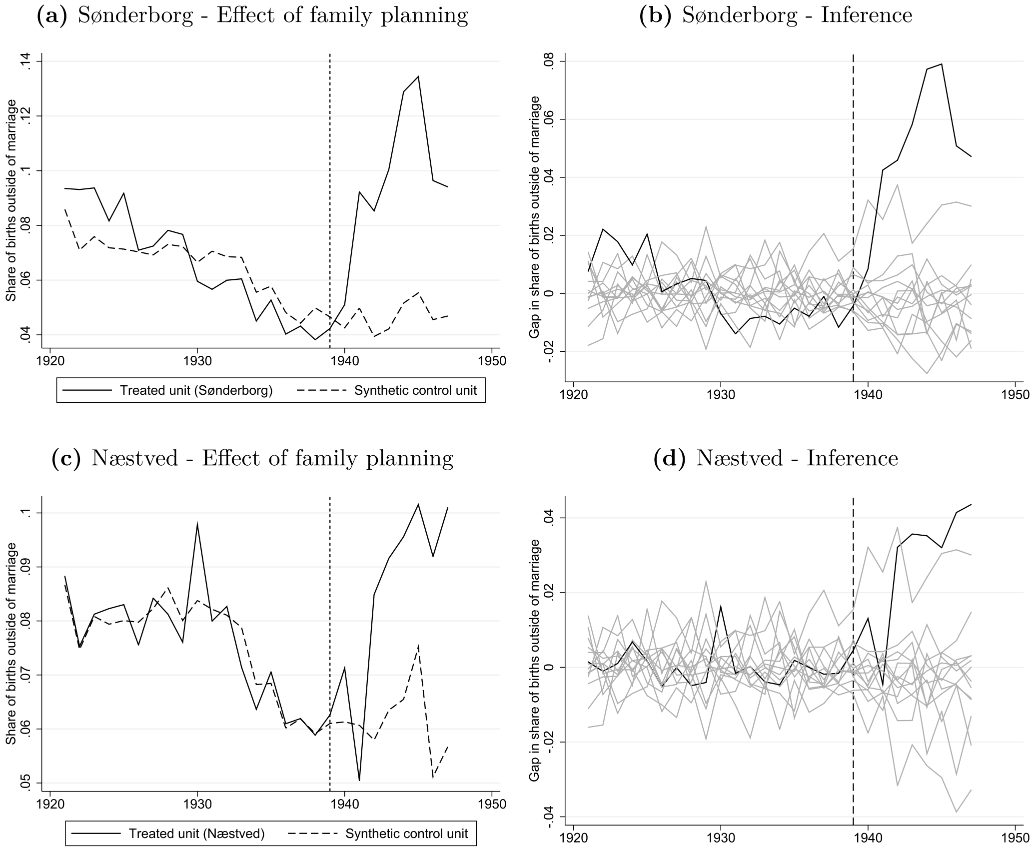

Figure 8. Evolution of the share of non-marital births at county level: (a) Sønderborg – Effect of family planning, (b) Sønderborg – Inference, (c) Næstved – Effect of family planning and (d) Næstved – Inference.

Notes: See notes to Figure 4.

effects. Ideally, the estimated treatment effects and inference should be approximately equivalent regardless of specification. Online Appendix Figure A8 shows the evolution of the non-marital and marital birth rate at town and county levels for Sønderborg along with counterfactuals estimated with the synthetic control method for the alternative specifications. All specifications produce virtually identical counterfactuals at both aggregation levels and for both outcomes. The reason is that the weights which form the synthetic control from the donor pool do not change much across specifications. Online Appendix Table A3 shows the correlation between weights across the alternative specifications. The exact specification of the predictors used in the construction of the synthetic control unit is not central to the results. Regardless of the predictor set, the weights, the counterfactual and the estimated effects are stable.

Finally, I compare the synthetic control findings to those produced by the DiD approach. I estimate the treatment effects with and without controlling for covariates. The treatment is not staggered but at the same time in the two treated towns and I use the same set of towns as control group as was used as donor pool in the synthetic control analysis. Specifically I run the following regression:

$$y_{it} = \alpha + \beta Treat_i t + \omega_i + \gamma_t + X_i \times year_t + \epsilon_i t \quad (4)$$

lies somewhere in between these five alternatives. I do the sensitivity analysis with Sønderborg as example. Results for Næstved are available on request.

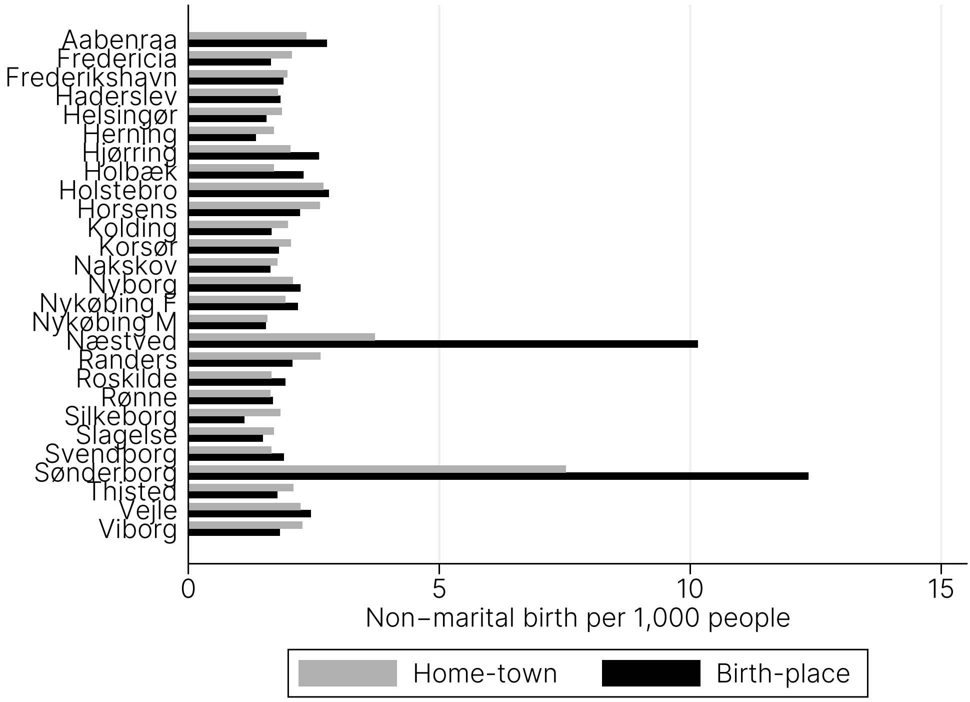

Figure 9. Crude non-marital birth rate at home-town and birth place level – 1944–1947 average.

Notes: The figure shows the average crude non-marital birth rate at home-town (gray) and birth place (black) level between 1944 and 1947.

y_{it} is the outcome for town i measured in year t. $Treat_i t$ is an indicator equal to 1 for the treated tows after treatment initiation and 0 otherwise. I also control for year and town fixed effects and pre-determined town characteristics interacted with yearly indicators (average pre-1939 values of income and wealth per capita, population density, women share and share employed in manufacturing). Standard errors are clustered at town/county level. I run the regressions for three outcomes: (i) non-marital birth rate, (ii) marital birth rate and (iii) share of non-marital births. Moreover, I estimate event studies to evaluate the common trend assumption. Online Appendix Table A4 presents the DiD results. The estimates generally support the conclusions from the synthetic control analysis. However for the marital birth rate, the estimates are highly significant at town level while being insignificant in the synthetic control analysis. DiD hinges on the common trend assumption. To evaluate the common trend assumption, the applied literature usually estimates event studies. Online Appendix Figure A9 shows event studies for the non-marital and marital birth rates and the share of non-marital births at town and county levels. The event studies for the non-marital birth rate show that the pre-treatment estimates are small and mostly insignificant (although at town level some pre-treatment estimates are slightly negative and significant) and the subsequent treatment dynamics resemble those obtained from synthetic control. For the marital birth rate, the pre-treatment estimates are significantly negative and trending at town-level. With the share of non-marital births as outcome, the event studies produce insignificant pre-treatment estimates at both aggregation levels. At county level, there is a tendency towards positive and significant estimates in the pre-treatment period. The DiD results underline two things. First, the overall evidence supports those obtained from the synthetic control analysis. Second, the event studies suggest differences in the trends between treatment and control prior to treatment which invalidates causal inference as the unobserved components cannot be differenced out. This suggest that the synthetic control method is a more appropriate tool to match the unobserved component by choosing the most suited control group.

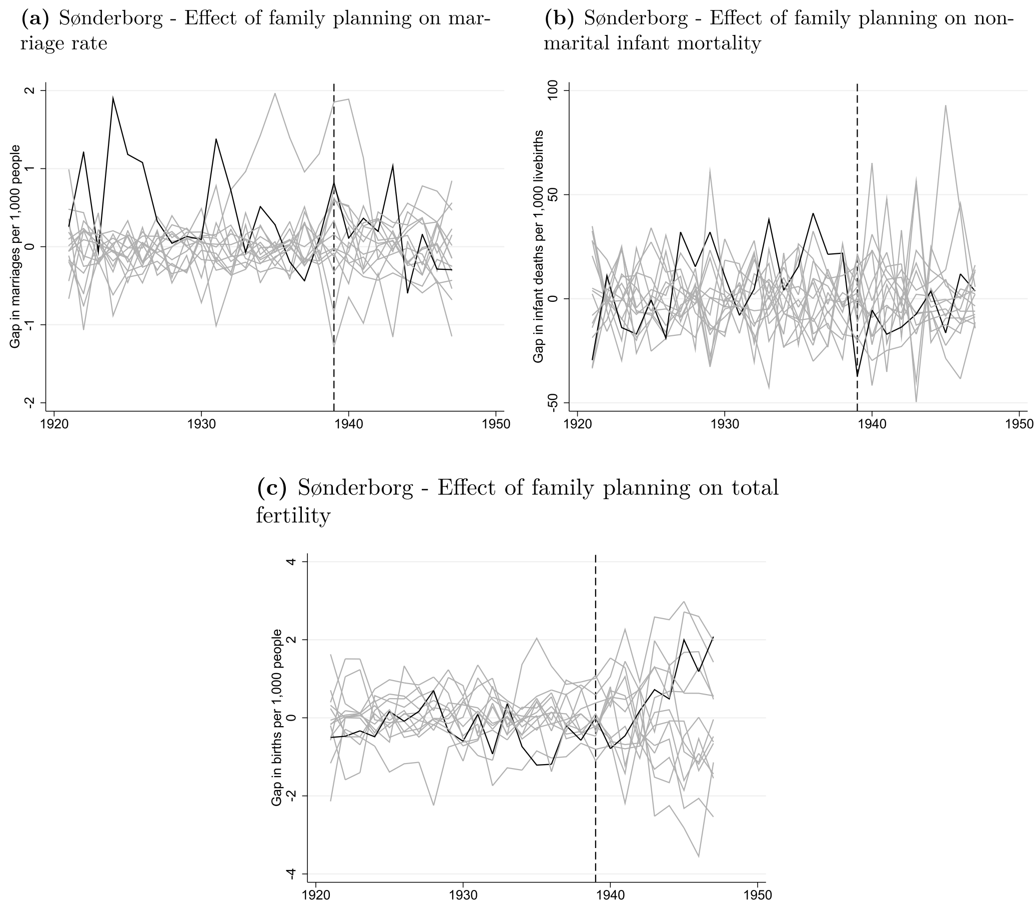

Figure 10. Evolution of marriages, infant mortality and total fertility at county level (Sønderborg): (a) Sønderborg – Effect of family planning on marriage rate, (b) Sønderborg – Effect of family planning on non-marital infant mortality and (c) Sønderborg – Effect of family planning on total fertility.

Notes: The figures shows the estimated treatment effects on the number of marriages per 1000 people (panel a), number of non-marital infant deaths per 1000 non-marital livebirths (panel b) and number of births per 1000 people from the 1939 family planning program along with placebo inference in Sønderborg county (panel c). The black solid lines are the treatment effects for the true treated county. The pre-intervention period is 1921–1938 and post-intervention 1939–1947. As pre-intervention covariates I include averages of population size and yearly outcomes. The donor pool includes 14 untreated counties.

6. Mechanisms

In this section, I explore possible mechanisms for my main results. Specifically, I investigate whether the strategy of the family planning program plausibly explains the fertility effects. The family planning program had a policy that preferred adoptions over abortions if the circumstances were not too severe. Moreover, the program offered in-kind transfers (milk, food, clothes) to single mothers who decided to keep the child. If these alternatives were viable substitutes to illegal abortions there should be abrupt increases in the number of children living in non-marital households and/or in adoptive care in the period following the introduction of the program.

Adoption data is not detailed enough to perform a synthetic control estimation. Neither is data on abortions since these were illegal and not registered even at aggregated level. Country-level data on adoptions is available from 1933 and onward (Medical Reports for the Kingdom of Denmark). The number of children in foster care and orphanages and living in households where the parents are unmarried are available from the same source for the entire period.

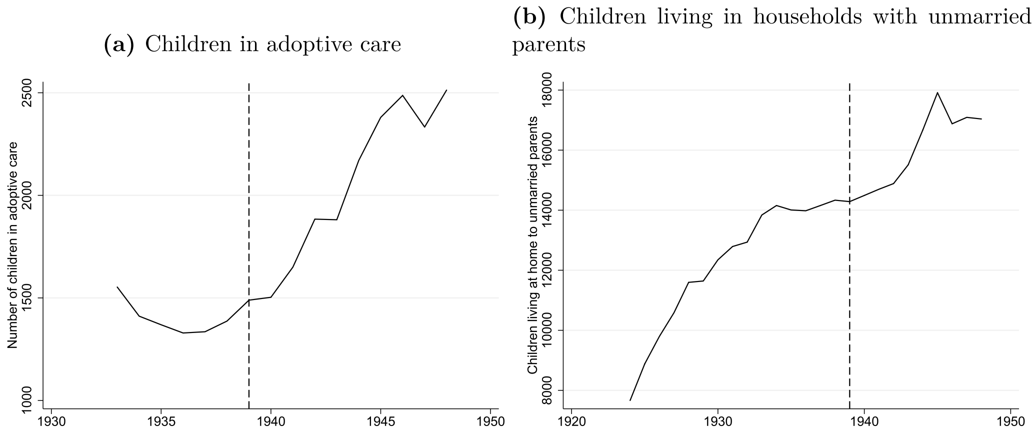

Figure 11. Living arrangements of children: (a) children in adoptive care and (b) children living in households with unmarried parents.

Notes: The lines plot the number of children living in adoptive care in panel (a) and the number of children living in household where the parents are not married in panel (b). Source: The Medical Report for the Kingdom of Denmark (various years).

Panel (a) in Figure 11 plots the number of children in adoptive care from 1933 to 1948 in Denmark. The number of children in adoptive care increased from 1500 to 2500 children from 1933 to 1948. The introduction of the family planning program coincides with the increase in the number of children in adoptive care. This is suggestive evidence that the adoption policy in the family planning program worked and that increased adoption options were a mechanism for the increase in non-marital fertility.

The number of children living in households with unmarried parents increased in the aftermath of the introduction of the family planning program suggesting that not all children born outside of marriage were given up for adoption by their biological mother (see panel (b) Figure 11). This increase is in line with the services provided by the family planning program. Legal aid in paternity cases and non-financial aid (milk, food and clothes) made it easier to keep the child for single mothers.

The welfare effects of less illegal abortions are difficult to evaluate from the perspective of the children. For the mothers, studies show associations between abortion and subsequent mental health disorders (Fergusson et al., 2008, 2006) but it is unclear how adopting away affects mental health relative to abortion.[22] Moreover, the birth and subsequent raising of an unwanted child is not cost-free (Gipson et al., 2008; S. Miller et al., 2020). Other welfare consequences are women labour market participation and general health. As illegal abortion was quite dangerous we could expect a direct increase in the health of the women who would otherwise have aborted illegally. In terms of labour market participation, a large literature has documented child birth to cause substantial and permanent declines in employment for mothers implying that the program might have decreased labour market attachment for the mothers (Kleven et al., 2019, 2024). While relevant, these considerations were secondary to the policy makers at the time as their primary focus was to address the low fertility and halt illegal abortions.

7. Conclusion

Most family planning programs provide information and supply contraceptives, such as condoms and birth control pills, and abortion services. The political target of these programs is to reduce

[22]Some studies investigate the psychological effects of openness in the adoption process and find that an increase in openness (e.g. phone calls, visitation, involvement in choosing the adoptive parents) has positive effects on mental health (Cushman et al., 1997). In terms of child well-being, Case and Paxson (2001) show that adoptive children are equally well-off as children raised by a biological mother while Bramlett et al. (2007) find that adoptive children do have worse health and cognitive development but receive more parental investments compared to biological children.

population growth and the frequency of unwanted pregnancies and births. The current state of the literature documents robust evidence that such programs reduce fertility by up-takes in both contraceptives and abortions. In contrast, in the 1930 Denmark the demographic issue was low and stagnating fertility. Thus the family planning program introduced in 1939 was designed to increase fertility by improving the conditions for unmarried pregnant women and advising against illegal abortions. The program was introduced in the five largest towns and in two medium-sized towns. The two medium-sized towns form ideal natural experiments to study the causal effects of the program.

I combine several historical data sources to build a panel dataset of Danish towns and counties from 1921 to 1947. Using the synthetic control method, I estimate the causal effects on marital and non-marital fertility from the family planning program. The results show that non-marital fertility increased significantly in response to the program while marital fertility was unaffected. This heterogeneous impact is a consequence of the intentions of the program as the services provided by the program were largely focused on unmarried pregnant women.

The effects are comparable across the two treated towns minimising the likelihood that other factors drive the estimated effects. Marital and non-marital live births were reported at birth-place level at the time. The consequence of this reporting practice is that the very large town-level effects might not be actual fertility responses but caused by pregnant women from neighbouring areas migrating to the treated town at the time of birth. To test for actual non-marital fertility responses, I increase the aggregation level to counties. The county-level results show that non-marital fertility did in fact increase. The results show that particularly women with unplanned pregnancies respond to the program. Married pregnant women – where the pregnancy is more likely to be wanted and planned – do not respond to the incentives provided by the program. 1350 children were born by unmarried women in the two treated areas caused by the program. In line with the policy of the program, the number of children in adoptive care increased in the years after the introduction.

The results are consistent with changes in the timing of birth-giving as lifetime fertility might be unchanged. Nevertheless, the 1939 Danish family planning program could have had long-term effects. David et al. (1990) discuss the differences between Denmark and the USA in approaches to family planning. Denmark has less unwanted pregnancies and induced abortions compared to the USA. According to David et al. (1990), two factors explain the differences: (i) a more positive public opinion on sexuality and (ii) the universality of family planning services. The universal principle in family planning services originated with the program from 1939. Furthermore, the program also actively worked on the public perception to break the stigma around non-marital pregnancies and births. The large effects on non-marital fertility that I have documented in this paper prove a societal impact which could have long-lasting effects contributing to these factors.

The evidence from this study – coupled with the existing literature – show that family planning programs can affect fertility in either direction depending on the content and context of the program.

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