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Time Constraints and Family Planning*

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Abstract

Understanding how households use family planning to navigate constraints is important for policy design. We exploit a large Danish retirement reform that affected intergenerational time transfers to study how family planning responds to changes in time constraint tightness. Using administrative data on the universe of abortions and prescriptions for planned and emergency oral contraception, we show that alleviating time constraints reduces both abortions and contraceptive use and thereby shifts behavior toward childbirth. Our results highlight that time constraints shape responses to especially unplanned pregnancies and that policies easing household constraints may influence abortion behavior and family planning.

JEL-codes: J13, I38, D64.

Keywords: Time constraints, Unintended pregnancies, Abortion, Contraceptives, Childbirth, Childcare, Retirement Reform.

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1 Introduction

Household decisions about family planning are shaped by both financial and time constraints. While existing research shows that fertility responds to financial incentives, much less is known about how time constraints affect fertility and, crucially, how they shape whether pregnancies are prevented, terminated, or carried to term.[1] This distinction is important because policy implications differ depending on whether fertility responds through prevention or abortion. For example, policies that ease household constraints may influence abortion rates and thus potentially mitigate adverse consequences of abortion restrictions (Miller, Wherry and Foster, 2020).

We study how family planning responds to changes in time constraint tightness. We exploit a 1994 reform in Denmark that unexpectedly expanded an early-retirement transition (TR) scheme. The reform created sharp discontinuities in early retirement ages of up to 10 years across adjacent cohorts, advancing retirement of the generation whom we refer to as grandmothers. This, in turn, generated quasi-experimental variation in their daughters' availability of grandparental time transfers towards, e.g., childcare. We use this variation to study how time constraints influence the family planning behavior of daughters, utilizing high-quality, longitudinal, third-party reported Danish register data on the universe of induced abortions, oral contraceptive pill ("The Pill") prescriptions, and the morning-after pill prescriptions.

Our main contribution is to show that easing time constraints reshapes fertility control by shifting behavior across prevention and abortion margins. We find that both contraceptive use and induced abortion decline. First, we estimate a decrease in planned pregnancy prevention through reduced oral contraceptive use of around 2%, although not statistically significant. Second, the number of abortions per woman decreases by 0.012 annually, corresponding to about a 7% reduction. Similarly, we find a 0.5 percentage point decline in emergency contraceptive use, suggesting that time constraints are particularly important for how unintended pregnancies are resolved. Taken together, these changes lead to a significant increase in fertility among daughters of affected grandmothers of 3.5%.

We also find suggestive evidence that fertility control responses to time constraints varies by the presence of children prior to the reform. We see the largest reductions in abortions and emergency contraceptive use for daughters who were childless before the reform took effect. This suggests that in cases where receiving help from a mother is most needed (i.e. with the

[1] Fertility has been shown to respond to wealth increases (Lovenheim and Mumford, 2013; Dettling and Kearney, 2014; Atalay, Li and Whelan, 2017; Daysal, Lovenheim, Siersbæk and Wasser, 2021), child subsidies and tax reliefs (see e.g. Milligan, 2005 and Cohen, Dehejia and Romanov, 2013), childcare costs (Blau and Robins, 1989; Del Boca, 2002; Mörk, Sjögren and Svaleryd, 2013) and tax rates (Jakobsen, Jørgensen and Low, forthcoming).

first child), the TR expansion primarily affected how unplanned pregnancies were resolved.

Finally, we investigate the mechanisms through which the reform affected daughters. Using linked survey data on inter-generational time transfers, we show that the reform primarily alleviated time constraints rather than affecting intergenerational financial transfers. While we find no effects on daughters' liquid wealth, daughters of affected cohorts are 11 percentage points more likely to report that they can rely on help from their parents with childcare and household tasks.

Our results have implications for both policy and research. We show that abortion behavior and emergency contraceptive use respond to changes in time constraints, even in a setting with a generous welfare state and highly subsidized childcare. This suggests that policies easing household constraints may influence abortion patterns, potentially mitigating adverse consequences of abortion restrictions (Foster, Biggs, Ralph, Gerdts, Roberts and Glymour, 2018; Foster, Biggs, Raifman, Gipson, Kimport and Rocca, 2018; Foster, Raifman, Gipson, Rocca and Biggs, 2019; Miller, Wherry and Foster, 2023). More broadly, our findings highlight the importance of modeling fertility as a process involving both prevention and abortion decisions, particularly in environments characterized by uncertainty and constraints.

We contribute to several strands of active research. First, we contribute to the sparse but growing literature documenting that abortion behavior and contraceptive use adjust to financial incentives and circumstances. González (2013) and González and Trommlerová (2024) leverage a reform in Spain that temporarily increased universal child benefits (baby bonuses) to estimate the effect of unconditional cash transfers to parents on abortion behavior and fertility. Using province- and time variation, they find that this reform led to a decline in the number of abortions.[2] Abiona (2017) and Alam and Pörtner (2018) study the effect of (agricultural) income shocks on contraceptive use. We contribute to this literature by documenting that not only financial constraints, but also time constraints, affect abortion behavior and contraceptive use.

We also contribute to the growing literature on the effects of intergenerational time transfers on fertility. Using Italian data, Eibich and Siedler (2020) find that early maternal retirement increases the likelihood of their daughters giving birth to a grandchild by 15 percentage points (pp). Ilciukas (2023) estimates a 3.3% reduction in fertility from a Dutch retirement reform involving a mix of changes in replacement rates and delayed retirement age.[3] Our

[2]González, Guirola and Zapater (2026) find that abortion behavior, also in Spain, responded to an unexpected political outcome of a 2004 general election.

[3]Battistin, Nadai and Padula (2014) also find increased fertility from a retirement reform in Italy. Laczek (2023) used the same Danish reform as we do to study the labor supply effects on the children of the directly affected cohorts. Also focusing on labor supply, Bratti, Frattini and Scervini (2018) study a retirement reform

estimates are in line with this literature. While these studies use similar identifying variation, we contribute by unpacking the fertility response into several modes of control. This allows us to show that both planning and emergency methods are used to adjust to shocks to time constraints.

The remainder of the paper is organized as follows. In the next section, we present and describe the Danish register data and the Danish institutional setting. In Section 3, we outline our triple difference identification strategy and report the empirical results in Section 4. We investigate the mechanisms through which the retirement reform alleviated constraints for daughters in Section 5. We conclude in Section 6.

2 Data and Institutional Background

Here we first describe the data sources and the institutional context, before discussing descriptive statistics.

2.1 Data

Sample Selection. We combine data from different sources and link mothers and daughters through family identifiers. We define our main sample as daughters of mothers born from 1944 to 1950, as these birth cohorts were differently affected by the TR reform in 1994, as we will elaborate on below. We use data around the reform, from 1985 to 2009, and restrict attention to daughters aged 15–43. In turn, we use data for daughters when their mothers were 40–59 years old. To avoid confusion, we refer to the mothers potentially directly affected by the reform as "grandmothers" throughout the remainder of the paper, regardless of whether their daughters have children.

Register Data. We use Danish longitudinal register data for our main analysis. For both grandmothers and daughters, we combine a range of labor market and health registers. For the grandmothers, we use the income register to identify their labor market status (whether working, in transition retirement, or in regular retirement). We also use information on governmental unemployment insurance (UI) membership.

For the daughters, we combine population-, hospital-, and abortion registers to investigate their fertility outcomes, focusing on the accumulated number of children, the accumulated number of abortions, and an indicator for being childless and for never having an abortion before. For their contraceptive behavior, we use data from the prescription register

similar to that in Battistin, Nadai and Padula (2014) and Eibich and Siedler (2020).

(starting in 1995), focusing on prescriptions for oral contraceptives (The Pill) and emergency contraception (The Morning-after pill). During the period we consider, oral contraception was available only by prescription. The emergency contraception was available only by prescription until 2001. After 2001, the emergency contraception was also available without a prescription, which we do not observe. For analyses of emergency contraception, we only use the period 1995-2001. For further details, see Appendix B.

1995-Cohort Survey Data. We augment and link the administrative register data with a Danish longitudinal survey collected by The Danish Center for Social Science Research (VIVE) that follows about 6,000 children born in the fall of 1995, just after the reform, and their families from birth into adulthood ("Børneforløbsundersøgelsen" in Danish). The 1996 and 1999 waves contain questions that we utilize to investigate whether the reform affected intergenerational time transfers of grandmothers. First, we denote a daughter having "access to informal childcare" if she reports that she can always count on her own parents to take care of her child in case of sudden sickness or work. Second, we denote a daughter having "access to household help" if she reports that she can always count on her own parents to help with cooking, cleaning, or other household chores. For further details, see Appendix B.

2.2 Institutional Background

Health- and childcare in Denmark. Healthcare is universal and free in Denmark. This also includes abortion procedures, which were legal up to the 12th gestational week. Women aged below 18 required consent from a parent. Women in our age interval can obtain prescriptions for oral contraception and the morning-after pill (without consent of their parents) after consulting a general practitioner (GP). The contraception can then subsequently be purchased at a pharmacy. Formal childcare was generally universally available in the years studied herein, and costs were highly subsidized, such that parents pay around 25% of the underlying cost of childcare provision, and low-income households pay less.

The Danish pension system. The Danish pension and retirement system has changed over time for different birth cohorts, and we here focus on the system around 1992-2005 (at the time around the reform) for the relevant birth cohorts born 1944–1950. The Danish retirement benefit system consisted of three main elements: Early retirement pension (ERP, "efterløn" in Danish), old age pension (OAP, "folke-pension" in Danish), and a transition retirement scheme (TR, "Overgangsydelse" in Danish). ERP is a voluntary program allowing individuals to retire with ERP benefit from the age of 60 and stay on the program until they can transfer into the old age pension. Eligibility for ERP required 20 years of membership in an UI fund prior to entrance to the program. OAP was available to all individuals at the age of 67 and is fully government-financed through taxes.[4]

Finally, TR allowed retirement already from age 55 for individuals with weak attachment to the labor market. Individuals unemployed for at least 12 months within the last 15 months who would be eligible to enter the ERP system at age 60 could enroll in the TR scheme and remain on the program until transitioning into the ERP system. While the actual amount varied on the individual level, the long-term level of benefits was 80% of the maximum unemployment benefit level, which, in 1994, amounted to around 3,000DKK a week, approximately $\$2,000$ a month in 1994 prices.

The transition retirement (TR) reform. The TR scheme was originally introduced as a temporary scheme in 1992, allowing individuals born in the years 1932–1940 to retire at age 55. Surprisingly, rather than following the original plan of closing for new entrants into the scheme in 1995, in 1994 the TR scheme was extended to allow the original birth cohorts 1932–1940 to enter until 1998 and, importantly, temporarily expanded to allow early retirement as early as age 50 for those born before 1947. The expansion was temporary, and enrollment into the program for these younger cohorts would be closed after 1996. This expansion meant that individuals born in 1946, who then were 48 in 1994, had one year (turning 50 in 1996) in which they could enroll in the TR scheme and retire at age 50. Individuals born in 1944 (turning 52 in 1996) had three years in which they could enroll in the TR program and thus retire at age 50, 51, or 52. On the contrary, individuals born in 1947 and later saw an unchanged earliest retirement age at 60.[5]

Figure 1 illustrates the expansion of the TR program, showing the earliest retirement age for eligible individuals *not already* on the program in panel a). The three red-white hatched cells before age 50 illustrate that these cells are associated with anticipated early retirement ages introduced by the RF reform. Panel b) shows the percent of a birth cohort-age cell that entered into the TR program. There is a clear relationship, showing how access to TR for the slightly older cohorts led to a significantly earlier retirement age.[6]

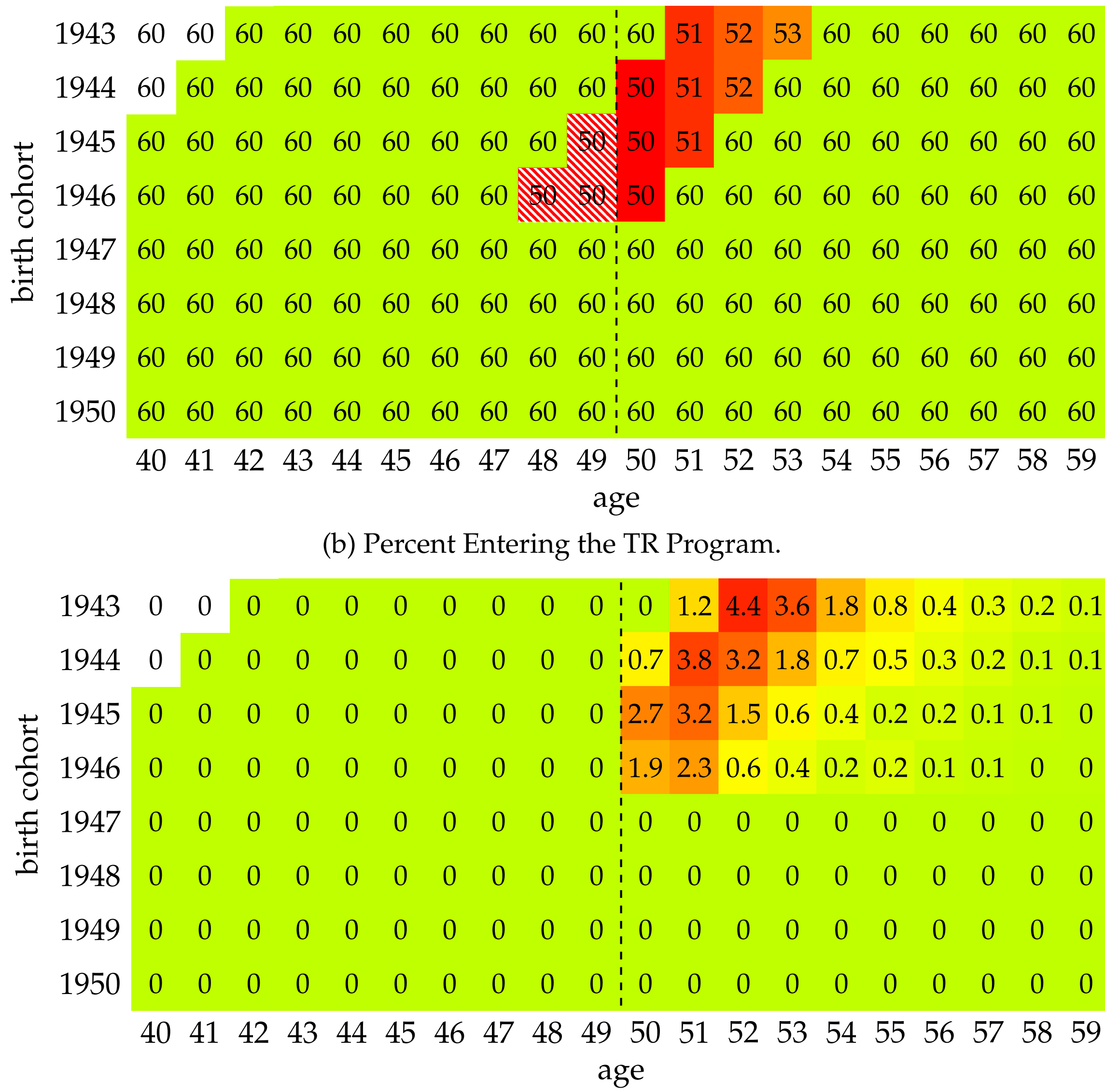
[4]In 1998, a reform reduced the OAP statutory retirement age to 65 for all the cohorts studied herein equally and increased the membership requirement for ERP from 10 years to 25 out of the last 30 years.

[5]In 1996, the program was suddenly closed for new enrollment despite the extension was only put in place a few years before.

[6]Some individuals are recorded enrolling outside the suggested cells due to the fact that the program participation is recorded in November in a given year in the register data, while actual birthdays are distributed across the year. Some enroll after 1996, likely due to delayed administrative handling of applications.

Figure 1: The 1994 Transition Retirement (TR) Reform.

(a) Early Retirement Ages (eligible), 1994 TR Reform.



(b) Percent Entering the TR Program.

Notes: The figure shows the effects of the expansion of the TR program in 1994. Panel a) shows the earliest retirement age for eligible individuals *not already* on the program. The three red-white hatched cells before age 50 illustrate that these cells are associated with anticipated early retirement ages introduced by the RF reform. White cells refer to age/birth cohorts not included in our sample. Panel b) shows the percent of a birth cohort-age cell that entered the TR program. Some individuals are recorded enrolling outside the suggested cells due to the administrative handling of applications could be delayed, and due to the fact that the program participation is recorded in November in a given year in the register data, while actual birthdays are distributed across the year.

2.3 Descriptive Statistics

We present descriptive statistics in Table A.1 in Appendix A. We report means of relevant variables for the entire sample and split by grandmother birth cohort groups, where we label grandmothers born 1944-1946 as the treatment cohorts and grandmothers born 1947-1950 as the control cohorts. All statistics are based on information when the grandmothers are 47 years old (prior to the TR reform), except for contraceptive pills, where data is not available that early, such that we report averages at age 52. Panel A presents information at the grandmother level, while Panel B shows characteristics of the daughters. While 54% of our sample qualifies for the TR scheme according to birth cohort, only 11% are eligible according to our UI-criterion described above. The statistics show almost no differences between grandmothers in the treatment and control cohorts.

Panel B in Table A.1 shows that daughters in our sample period are relatively young, around 21, with daughters of treatment cohorts being slightly older. The groups are generally similar, with 36% having a partner, 69% living in the same municipality as their mother, and 40% enrolled in education. Daughters are mostly childless, with a slightly higher proportion in the control group. The only notable difference between the groups is in the contraceptive use: 51% use oral contraceptives in the control group, compared to 45% in the treatment group, illustrating a general upward time trend in the use of oral contraceptives.

3 Empirical Strategy

We study how a retirement reform affecting the older generation influences the fertility behavior of their daughters, utilizing the TR reform, described above and illustrated in Figure 1. To this end, we employ a triple-difference approach and consider four groups defined by the interaction between satisfying the TR eligibility criteria (eligible) and the birth cohorts of grandmothers. We split the grandmothers according to the birth cohort and label women born between 1944–46 as the treatment cohorts, as they have access to transition retirement, and women born 1947–50 as control cohorts. We further distinguish between grandmothers who were generally eligible to take up the transition retirement. As discussed above, the eligibility requirements were tied to the labor market status and eligibility to enroll in the ERP program at age 60.

We denote an individual as eligible for enrollment in the TR scheme if they have been a member of an UI fund for the last four years and have been unemployed for at least around a year in that period (at least 22.5% unemployment in those four years). We base the eligibility classification on age 47 information (and keep it constant over time) as this was the last age at which all treated, and control cohorts had an expected retirement age of 60, as shown in Figure 1. This definition is conservative in the sense that ineligible individuals by this definition could, in fact, become eligible in later years.

Illustration and Grandmother's Retirement. To illustrate the empirical strategy, we show in Figure 2 how our identification strategy identifies the direct effect on the retirement of grandmothers. Figure 2, Panel a) presents retirement rates for grandmothers eligible for the transition retirement scheme as a function of age. We define retirement as receiving any TR, ERP, or OAP benefits, as well as receiving any disability insurance or pension savings payout. The solid line corresponds to the treated cohorts, women born in 1944-46 (those for whom the scheme became available by the 1994 reform), while the dashed lines represent the control cohorts (women born in 1947-50), for whom the retirement age remained 60.

The figure shows a sharp increase in retirement among eligible treatment cohorts, rising from approximately 3 percent at age 49 to around 40 percent at age 55, with a sharp increase at age 50. In contrast, while the retirement rate for the control cohorts is similar until age 50, this group displays a smooth and muted increase in retirement across age, reaching around 20 percent at age 55.

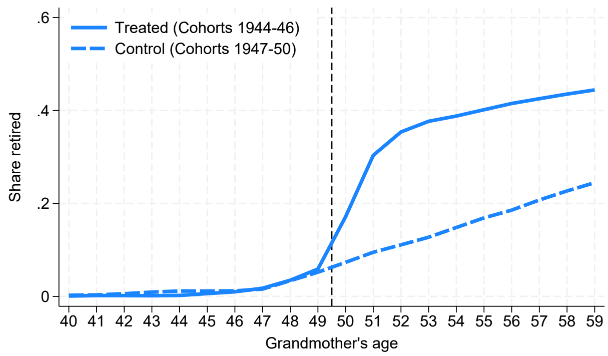
Panel b) of Figure 2 shows similar statistics for grandmothers who we label *not eligible* for the TR scheme, as discussed above. For both cohort groups, the share retired is approximately linear in age, with only slight differences between the cohort groups. The treated birth cohorts display a slight increase in the retirement rate at age 51. This small effect for the treated birth cohorts in the ineligible group is due to our conservative definition of eligibility at age 47. These individuals can become eligible at later ages.

Panels c) and d) of Figure 2 plot the differences between treatment and control cohorts for eligible and ineligible grandmothers, respectively. For both groups, there are negligible differences prior to age 50 (note that we have not normalized any differences to zero in these panels). From age 50, a large gap of around 20 percentage points opens up between the treated and control birth cohorts among the eligible grandmothers (Panel c)). Panel d) shows the small difference between treated and control cohorts for the ineligible group of around 3 percentage points.

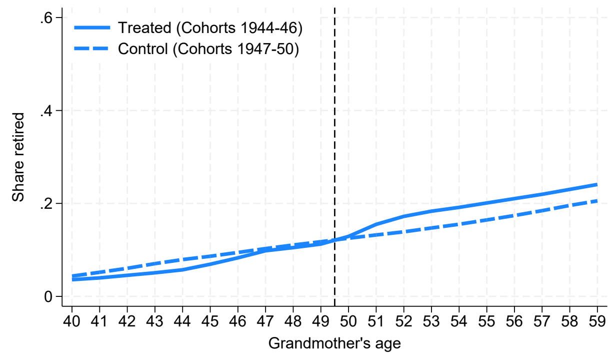
Panel e) of Figure 2 plots (dynamic) triple-difference estimates as the difference between the retirement rate differences in panels c) and d), relative to the difference in differences at age 47. The estimates show no discernible differences prior to age 50, supporting the assumption of parallel pre-trends. After age 50, however, there is a sharp increase in retirement rates for the treated group relative to the control group. The effect reaches approximately 23 percentage points at age 52 and remains substantial at older ages with a static estimated increase

Figure 2: The 1994 TR Reform and Grandmothers' Retirement.

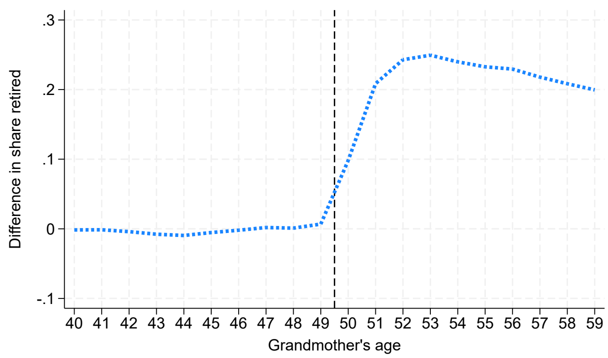
(a) Share Retired, Eligible for TR.



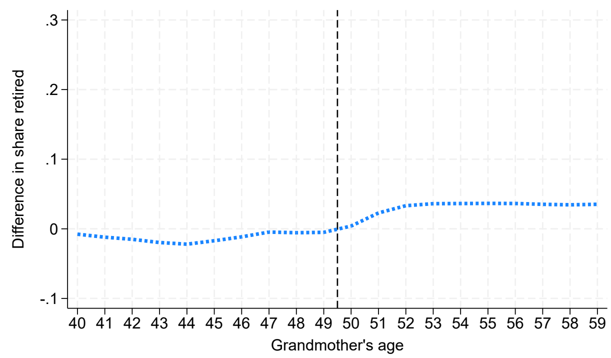
(b) Share Retired, Not Eligible for TR.



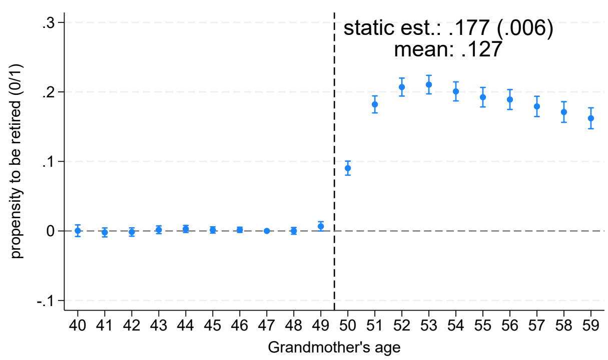
(c) Difference in Share, Eligible.



(d) Difference in Share, Not Eligible.



(e) Dynamic triple-DiD: Retirement.



Notes: The figure shows the identification strategy, using a triple-difference strategy around the TR reform. Panel a) focuses on grandmothers who we label as eligible for enrollment in the TR system at age 47 and shows the share of grandmothers retired for birth cohorts 1944–1946 (solid, treated) and 1947–1950 (dashed, control). Panel b) shows the same statistics for those whom we label as not eligible at age 47. Panels c) and d) plot the difference between the two groups of birth cohorts (dotted lines) for the eligible and not-eligible groups, respectively. Finally, panel e) plots the triple-difference, showing the difference (in differences) between the eligible and not-eligible group, relative to age 47. The panel plots regression coefficients from eq. (1), excluding age dummies for the daughter, with a retirement indicator as the dependent variable. Whiskers show 95% confidence intervals based on standard errors clustered at the grandmother level.

in the retirement rate of around 0.177. Compared to a baseline retirement rate of 13 percent, the scheme more than doubled the number of women in retirement in the eligible cohorts.[7] The large and persistent impact of this scheme on the treated cohorts makes it particularly well-suited for analyzing its downstream effects on daughters' fertility outcomes.

Triple-Difference Estimator for Daughters' Outcomes. We estimate the impact of the TR reform on daughters by employing a dynamic triple Difference-in-Difference estimator. The estimation is implemented through regressions of the form:

$$
\begin{aligned}
y_{i,t} = & \sum_{s=40, s\neq 47}^{59} \beta_s \cdot \mathbf{1}\{\text{GM}_i \text{ born} \in [1944, 1946]\} \times \mathbf{1}\{\text{GM}_i \text{ eligible}\} \times \mathbf{1}\left\{\text{GM}_i \text{ age} = s\right\} \\
& + \sum_{s=40, s\neq 47}^{59} \alpha_s \cdot \mathbf{1}\{\text{GM}_i \text{ born} \in [1944, 1946]\} \times \mathbf{1}\left\{\text{GM}_i \text{ age} = s\right\} \\
& + \sum_{s=40, s\neq 47}^{59} \gamma_s \cdot \mathbf{1}\{\text{GM}_i \text{ eligible}\} \times \mathbf{1}\left\{\text{GM's age} = s\right\} \\
& + \sum_{s=40, s\neq 47}^{59} \delta_s \cdot \mathbf{1}\left\{\text{GM}_i \text{ age} = s\right\} + \text{age dummies for daughter}_{i,t} + \mu_{GM(i)} + \eta_t + \epsilon_{i,t}
\end{aligned}
\tag{1}
$$

where $y_{i,t}$ is the outcome for daughter i at time t. $\mathbf{1}\{\text{GM}_i \text{ born} \in [1944, 1946]\}$, $\mathbf{1}\{\text{GM}_i \text{ eligible}\}$ and $\mathbf{1}\{\text{GM}_i \text{ age=s}\}$ are dummy variables for grandmothers' birth cohort being treated by the TR reform, eligibility status and age, respectively. The parameters of interest are the β's, which measure the causal intention to treat effects of having access to informal child-care through grandmother's early retirement. We normalize the outcomes to zero when the grandmother is 47 years old and include a full set of age dummies for the daughters and controlling for grandmother-fixed effects, $\mu_{GM(i)}$, and calendar year fixed effects, η_t.[8] Standard errors are clustered at the grandmother level throughout.

We also estimate static versions of the form:

$$
\begin{aligned}
y_{i,t} = & \beta_a \cdot \mathbf{1}\{\text{GM}_i \text{ born} \in [1944, 1946]\} \times \mathbf{1}\{\text{GM}_i \text{ eligible}\} \times \mathbf{1}\left\{\text{GM}_i \text{ age} > 49\right\} \\
& + \alpha_a \cdot \mathbf{1}\{\text{GM}_i \text{ born} \in [1944, 1946]\} \times \mathbf{1}\left\{\text{GM}_i \text{ age} > 49\right\} \\
& + \gamma_a \cdot \mathbf{1}\{\text{GM}_i \text{ eligible}\} \times \mathbf{1}\left\{\text{GM}_i \text{ age} > 49\right\} \\
& + \delta_a \cdot \left\{\text{GM}_i \text{ age} > 49\right\} + \text{age dummies for daughter} + \mu_{GM(i)} + \eta_t + \epsilon_{i,t}.
\end{aligned}
\tag{2}
$$

The identifying assumptions underlying the triple-difference estimator are as follows.

[7] On average, the age at retirement for these eligible cohorts decreased by 1.7 years.

[8] Note that due to the prescription data starting in 1995, we need to normalize to zero when the grandmother is age 49 for the contraceptive outcomes.

First, in the absence of treatment, the difference-in-differences across the first two dimensions would have evolved similarly across the third dimension. This rules out the presence of differential time trends across subgroups. Figure 2, Panel e) provides support for this assumption: there is no indication of differential pre-trends, and the triple-difference estimates remain flat prior to treatment.

Second, no other contemporaneous event should differentially affect the outcomes across the birth cohorts and eligibility status. In particular, there should be no reforms or shocks that disproportionately influence eligible women in the treatment cohorts relative to either non-eligible women or women in later cohorts. To the best of our knowledge, no such confounding policy changes or external shocks occurred during the relevant period.

Third, the composition of groups should remain stable and not change in a manner correlated with treatment across the three dimensions. Because birth cohort and age are predetermined and cannot be manipulated, compositional changes can arise only through eligibility status. However, eligibility is determined based on characteristics measured at age 47, which predates the announcement of the scheme (see Figure 1). This makes endogenous selection into eligibility unlikely.

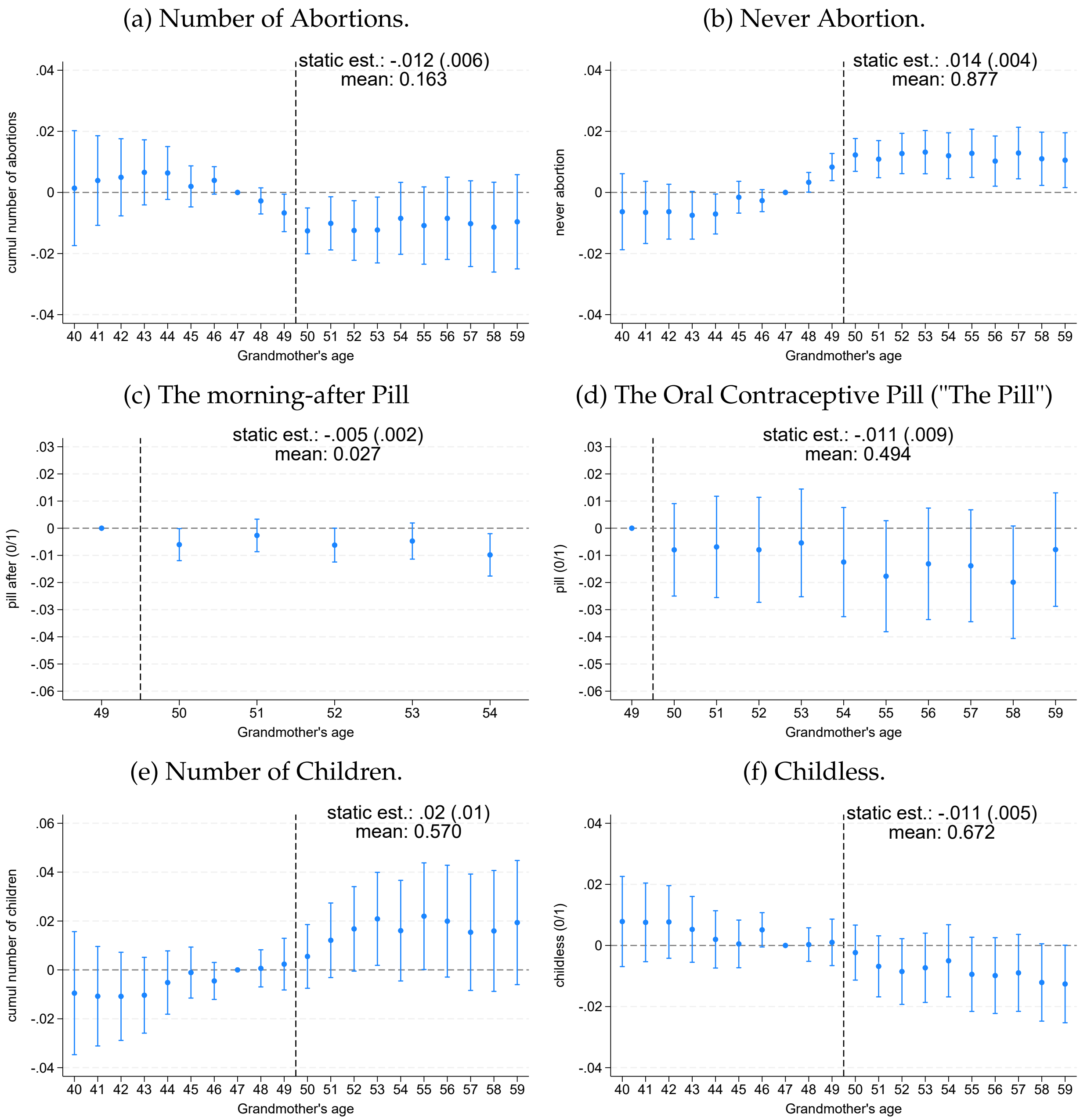
Fourth, the stable unit treatment value assumption requires that treatment assigned to one group does not affect outcomes in other groups. In this context, this implies the absence of spillover effects across cohorts or subgroups. While we cannot entirely rule out the possibility that norms regarding early retirement spill over to other groups, any such effects would likely attenuate the estimated treatment effect, implying that our estimates should be interpreted as conservative.

4 Results

We report in Figure 3 dynamic triple-DiD estimates from equation (1) for daughters' *i)* number of abortions in Panel a), *ii)* likelihood of having no abortions in Panel b), *iii)* likelihood of using the morning-after pill in Panel c), *iv)* likelihood of using the oral contraceptive pill in Panel d), *v)* number of children in Panel e), and *vi)* likelihood of being childless in Panel f). For the planned and emergency contraceptive pill prescriptions, we only have data from 1995 onwards and thus cannot estimate effects for grandmothers' ages 40-47. We also report the static estimates from equation (2) along with the overall average of the outcome for context.

Margins of Fertility Control. Panel a) of Figure 3 shows that the number of abortions is reduced significantly by around 0.012 abortions per woman (*p*-value 0.051), corresponding to about a 7 percent decline. The share never having abortions at all also goes up by around

Figure 3: Daughters' Family Planning and Fertility Outcomes.



Notes: The figure shows fertility outcomes for the daughters. Panel a)-f) plot the triple-difference, showing the difference (in differences) between daughters of eligible and not-eligible grandmothers, relative to age 47 of the grandmother. In Panels c) and d), the normalization is at the age of 49 for the grandmother due to data limitations. The Panel plots regression coefficients from eq. (1). The dependent variable refers to the daughters and is number of abortions (Panel a)), an indicator for never having an abortion (Panel b)), an indicator for a purchases of the morning-after pill (Panel c)), an indicator for purchases of the pill (Panel d)), number of children (Panel e)), and an indicator for being childless (Panel f)). Whiskers show 95% confidence intervals based on standard errors clustered at the grandmother level.

1.4 percentage points (p-value 0.0005) in Panel b). The dynamic estimation reveals that much of the effect on the number of abortions is driven by the responses in the years just after the grandmothers enter the retirement scheme. This suggests that daughters whose mothers were eligible for TR experienced a lower number of abortions following the introduction of the reform. Notably, for both outcomes, we observe statistically significant effects already when grandmothers are aged 49. This pattern is consistent with anticipation effects, whereby daughters adjust their behavior in advance, anticipating that their mothers will become eligible for the retirement scheme at age 50 (see Figure 1). While to the best of our knowledge no existing research has documented that alleviating time constraints affects abortion behavior, González and Trommlerová (2024) and González (2013) find that the introduction of a universal child benefit reduced abortion rates.

We also see a significant decrease in emergency contraceptive use (morning-after pill) in Panel c) of Figure 3, especially in the years immediately following the reform, when grandmothers are around 50. Restricting all coefficients to be equal, we estimate a static effect decrease of 0.5 percentage point (p-value 0.012) in the likelihood of using the morning-after pill. While we also see a slight decrease in the use of planned birth control through the use of "The Pill" in Panel d), this effect is not statistically significant. The static effect is around -0.011 (p-value 0.222). Recall that due to data limitations on prescriptions, we can only estimate effects from 1995 onwards, when the grandmothers are at least 49 years old.

Fertility. We now turn to how fertility in turn responded, taking all modes of adjustments into account. Access to the early retirement scheme for grandmothers increased daughters' fertility by approximately 0.02 (p-value 0.045) children in Panel e). Compared to the baseline of 0.570, this corresponds to around a 3.5 percent increase, comparable to estimates in the literature. Ilciukas (2023) finds that a delay of the retirement of a grandparent reduced fertility by 3.3 percent, and Eibich and Siedler (2020) find that maternal retirement causes an increase in fertility of 15 percentage points. Scaling our estimate (0.020) by the share of grandmothers who actually went on early retirement (0.177 in Figure 2 Panel e)), we find an increase in the number of children of around 19%. This slightly larger estimate should be viewed in the light of the fact that the TR reform allowed the grandmothers to retire up to 10 years earlier. We estimate an increased likelihood of childlessness by roughly 1.1 percentage points (p-value 0.028) in Panel f), indicating that the policy also affected the decision whether to have any children.

In sum, we find that fertility increases when time constraints are alleviated, and this fertility adjustment is driven at least partly by abortions and emergency contraceptive use. In turn, our results suggest that time constraints are especially important concerns in relation to unintended or unplanned pregnancies.

4.1 Heterogeneity

We now investigate whether pregnancy resolution differs across groups of daughters. This should illuminate the mechanisms through which alleviated time constraints operate in different circumstances. In all heterogeneity analyses, we split the sample based on characteristics of the daughter, measured when the grandmother was aged 47. All results are reported in Table 1.

Table 1: Heterogeneous Responses.

	(1) No. of abortions	(2) Never abortion	(3) Morning-after pill	(4) The Pill	(5) No. of children	(6) Childless
Panel A: age 15-20 when GM aged 47						
Age 15-20	-0.005	0.007	-0.009	-0.007	0.039**	-0.024***
	(0.011)	(0.007)	(0.006)	(0.018)	(0.016)	(0.008)
Age 21-43	-0.016**	0.017***	-0.003	-0.012	0.016	-0.009
	(0.008)	(0.004)	(0.003)	(0.011)	(0.012)	(0.005)
Panel B: partnered when GM aged 47						
Having a partner	-0.025***	0.024***	-0.004	-0.011	0.007	-0.014***
	(0.009)	(0.005)	(0.003)	(0.013)	(0.014)	(0.006)
Not having a partner	-0.000	0.005	-0.005	-0.009	0.028**	-0.008
	(0.010)	(0.005)	(0.004)	(0.013)	(0.013)	(0.007)
Panel C: childless when GM aged 47						
Childless	-0.017**	0.013***	-0.006*	-0.016	0.025**	.
	(0.007)	(0.004)	(0.003)	(0.011)	(0.012)	
Not childless	0.002	0.016**	-0.000	0.009	-0.001	.
	(0.015)	(0.007)	(0.003)	(0.017)	(0.017)	
Panel D: live in same municipality as GM when GM aged 47						
Live in same	-0.012	0.016***	-0.005	-0.018	0.021*	-0.010*
municipality as GM	(0.008)	(0.005)	(0.003)	(0.011)	(0.012)	(0.006)
Live in different	-0.016	0.013**	-0.008*	0.001	0.019	-0.014*
municipality than GM	(0.011)	(0.006)	(0.004)	(0.016)	(0.017)	(0.008)

Notes: The Table reports estimation results of eq. (2) for different sample splits. All splits are based on information when the grandmother was 47 years old (prior to the reform). Standard errors in brackets are clustered on the grandmother level.

We find larger effects for daughters after adolescence in Panel A. We estimate a much larger reduction in abortions for daughters aged 21-43. While the difference is not statistically significant, this is likely driven by a higher propensity to have an intimate partner for this group. This is confirmed in Panel B, where we split based on being partnered (married or cohabiting) prior to the reform. We find that partnered daughters reduce abortion use dramatically, compared to non-partnered daughters.

We find substantial heterogeneity depending on whether daughters had children prior to the TR reform (Panel C). For daughters who already had children before the reform, the estimated effects are small and mostly insignificant. A possible explanation is that daughters whose mothers had not yet reached age 47 at the time of their first birth may have relied less on intensive support from grandmothers.[9] In contrast, among initially childless daughters, we observe a reduction in both the use of the morning-after pill and the number of abortions. Notably, the decline in abortions is quite large, while for daughters who already had children prior to the reform, the coefficient is close to zero. These findings suggest that, for unplanned pregnancies among childless women, access to maternal support may play a decisive role in the decision to carry a pregnancy to term.

Finally, we see relatively few differences based on the geographical distance between grandmothers and their daughters. The only difference is a slightly larger effect in the usage of the morning-after pill by women whose mothers live in a different municipality, where receiving help from a working mother is more difficult; thus, alleviating time constraints primarily affects decisions related to unintended pregnancies. This pattern again indicates that access to parental support may be particularly important when facing an unintended pregnancy.

4.2 Sensitivity and Robustness

Here, we investigate the sensitivity of the results to our definition of treatment and control cohorts, and the definition of eligibility for the TR benefits, discussed above. While the statistical power varies across alternative definitions and specifications, the results are generally robust to changes in these definitions.

In Table A.3 in Appendix A, we show results from varying the included grandmother birth cohorts. Panel A expands the sample by adding grandmothers born in 1943. As Figure 1 demonstrates, this birth cohort was partially treated, as they received access to TR by age 51 from the reform. Results from this exercise are overall very similar to the main results, as presented in Figure 3 and Table A.2. In Panels B-D, we successively narrow the set of birth co-

[9] This also suggests that the primary burden of childbearing is associated with the first child, consistent with the literature on the child penalty (see Kleven, Landais and Søgaard, 2019).

horts considered, reducing the sample gradually. This makes birth cohorts on each side of the threshold more comparable to each other, but comes at the cost of a much smaller sample size. Panels B-D confirm overall qualitatively similar patterns with quite similar point estimates across the samples. As standard errors increase, however, the results become insignificant, in particular in Panel D, where we only consider 2 birth cohorts.

In Table A.4 in Appendix A, we evaluate the effect of changing the definition of TR eligibility. Instead of requiring 4 years of UI membership and around 22% unemployment in those 4 years, we consider only 2 years of UI membership and 11% of unemployment in the past two years. Both the first stage and the effect on daughters' fertility outcomes are robust to this change, with only small effects on the point estimates.

We further test the sensitivity of our results in light of the variation in treatment timing as grandmothers turn 50 in different years. We implement the estimator by Callaway and Sant'Anna (2021), allowing for treatment heterogeneity across grandmother age in 1994. Results in Table A.5 show that our estimates are robust to allowing for treatment heterogeneity across grandmothers' birth cohorts.

5 Mechanisms

We have used the TR reform throughout as an exogenous shock to informal childcare availability and thus a relaxation of daughters' time constraints. Here, we provide several complementary analyses to substantiate the claim that this was the dominant mechanism through which the TR reform affected daughters' fertility decisions and resolution of pregnancies.

Intergenerational Financial Transfers. The TR reform could affect inter vivo transfers, since it potentially changed the income of grandmothers, besides their available time. To investigate the potential strength of this alternative channel, we provide in Panel A of Table 2 columns (1) and (2) triple-DiD estimates using an indicator for grandmother employed and the log disposable income of the grandmother, respectively, as outcome variables in eq. (2), again excluding age dummies for daughters. While we see that the TR reform led to a significant decrease in grandmother employment of around 17 percentage points (p-value 0.000) (potentially freeing up time), we only see a minor decrease in disposable income of less than 3% (p-value 0.000), suggesting little room for changes in financial transfers. The small decrease in disposable income is driven by a high degree of redistribution in Denmark and the fact that treated grandmothers had a relatively low attachment to the labor market and thus were likely at the bottom of the income distribution. In column (3), we also see no effect on liquid wealth in the daughter's bank accounts. While these are all indirect measures of intergenerational financial transfers, these results point to money transfers being the unlikely mechanism through which the reform affected daughters' fertility decisions. Note also that a reduced potential for intergenerational financial transfers would typically tend to *reduce* fertility, which is the opposite of the effect of time transfers and opposite to what we find.

Intergenerational Time Transfers. To investigate whether the TR reform affected the time transfers and alleviated time constraints of daughters, we augment our analysis with linked survey evidence on the propensity of daughters to receive help with household chores and childcare from grandparents. Specifically, we use two waves of the 1995-cohort survey conducted by VIVE.

Since this is a much smaller sample than the register data, we use a less strict definition of eligible/ineligible groups to improve power in these analyses. Concretely, we define grandmothers as eligible if, between the ages of 44 and 47, they have ever been unemployed while being a member of UI fund at the same time.[10]

Further, since we have little time variation, we implement a difference-in-difference (DiD) estimator, comparing levels (shares) between the four combinations of treatment and eligibility groups.

Concretely, we report in Panel B of Table 2 estimates of β_{time} from

$$\begin{aligned} y_{i,t} = {} & \beta_{time} \cdot \mathbf{1}\{\text{GM}_i \text{ born} \in [1944, 1946]\} \times \mathbf{1}\{\text{GM}_i \text{ eligible}\} \\ & + \alpha_a \cdot \mathbf{1}\{\text{GM}_i \text{ born} \in [1944, 1946]\} + \gamma_a \cdot \mathbf{1}\left\{\text{GM}_i \text{ eligible}\right\} \\ & + \text{age dummies for daughter} + X_{i,t}\Gamma + \eta_t + \epsilon_{i,t}, \quad t = 1996, 1999 \end{aligned} \tag{3}$$

where $X_{i,t}$ contains characteristics such as the occupation of the grandmother and whether grandmother and daughter live in the same municipality, as well as the number of siblings. η_t is an indicator for the 1999 wave.

Our outcomes are indicators reflecting whether or not a daughter replied that she always has access to grandparents' help for taking care of the child (Column 1 in Panel B) or for household help (Column 2 in Panel B). We see that daughters whose mothers had access to TR are 11 percentage points (p-value 0.052) more likely to report that they always have access to help with childcare, and 11 percentage points (p-value 0.075) more likely to report having always access to help in the household. These effects correspond to a more than 20 percent increase and are marginally significant. Hence, we find supporting evidence that retired grandmothers provide substantial time transfer to their daughters and that this may

[10]Since the grandmothers born in 1950 were not 50 by the time of the survey, we exclude them from this analysis.

be an important mechanism through which grandmothers' retirement decision impacts the daughters' fertility control choices and also realized fertility.

Table 2: Mechanisms: Intergenerational Transfers.

Panel A: Financial outcomes (administrative register data)

	Grandmother employed (1)	Grandmother log disp. income (2)	Daughter liquid wealth (3)
β_a	-0.170***	-0.025***	107.683
	(0.006)	(0.006)	(1067.497)
Obs.	3,094,736	3,055,579	4,271,490
baseline mean	0.817	11.740	30292.490

Panel B: Intergenerational time transfers (1995-cohort survey)

	Access to childcare (1)	Access to household help (2)
β_{time}	0.109*	0.105*
	(0.056)	(0.059)
Obs.	2,412	2,398
baseline mean	0.509	0.461

Notes: The table reports estimation results related to the mechanisms through which the TR reform likely affected daughters of affected grandmothers. Panel A columns (1) and (2) show triple-DiD estimates using an indicator for grandmother employed and the log disposable income of the grandmother, respectively, as outcome variables in eq. (2), again excluding age dummies for daughters. Column (3) shows the effect on the liquid wealth of daughters from eq. (2). Panel B shows estimates resulting from eq. (3) using the linked 1995-cohort survey. We denote a daughter having "access to informal childcare" in column (1) if she reports that she can always count on their own parents to take care of their child in case of sudden sickness or work. In column (2), we denote a daughter having "access to household help" if she reports that she can always count on their own parents to help with cooking, cleaning, or other household chores.

6 Conclusion

In this paper, we show that relaxing time constraints reshapes fertility decisions. Exploiting a large Danish retirement reform, we find that easing time constraints raises fertility while reducing both abortions and contraceptive use. These results indicate that time constraints affect not only whether individuals have children, but also whether pregnancies are prevented, terminated, or carried to term, shifting behavior toward childbirth.

Our findings have important implications for policy and research. Even in a setting with universal abortion access and highly subsidized childcare, easing time constraints reduces abortion rates, highlighting that policies affecting household constraints influence not only fertility levels but also how pregnancies are resolved. More broadly, our results suggest that economic models of fertility should incorporate decisions over contraception, abortion, and childbirth jointly, rather than treating fertility as a single outcome, particularly in environments with uncertainty and constraints.

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Supplementary Material

A Supplementary Tables and Figures

Descriptive Statistics. Table A.1 reports descriptive statistics for the control and treatment cohorts when the grandmother is aged 47. A total of 82,831 grandmothers belong to the treatment cohort and 100,119 to the control cohort. Overall, 11 percent of grandmothers meet the UI eligibility criterion. The majority (83 percent) are employed, while 9 percent are retired, primarily through disability pension schemes. There are virtually no differences between the treatment and control groups across these characteristics.

The sample of daughters comprises 120,237 individuals in the treatment group and 143,011 in the control group. The daughters are, on average, 21 years old, with those in the treatment group being less than six months older than those in the control group. On average, daughters have 0.28 children: 81 percent are childless, and the mean number of abortions is 0.12, with 90 percent having had none. Daughters in the treatment group have marginally more children and are slightly less likely to be childless than those in the control group (0.29 versus 0.27), although these differences are negligible. The only notable difference between the groups concerns contraceptive use. In the control group, 51 percent use oral contraceptives, compared with 45 percent in the treatment group. Along all other dimensions, the groups are highly comparable: 36 percent have a partner, 69 percent live in the same municipality as their mother, and 40 percent are enrolled in education.

Estimation results. In Table A.2, we show the estimation results from estimation of equation 2, which are also illustrated in Figure 3. Due to data limitations, the sample period is restricted to $1995 - 2001$ for the morning-after pill and to $1995 - 2009$ for the Pill. The results indicate that having a grandmother eligible for TR significantly reduces the number of abortions by 0.012, corresponding to a 7 percent decrease. At the same time, the fraction of daughters who have never had an abortion increases by 0.014, or approximately 1.6 percent. Turning to contraceptive use, we observe declines in the use of both the morning-after pill and planned oral contraceptives; however, only the reduction in morning-after pill use is statistically significant. Notably, the effect on morning-after pill use is particularly large, with a reduction of nearly 19 percent. Finally, these changes in abortion and contraceptive behavior translate into higher fertility. The number of children increases by 0.02, corresponding to a 3.5 percent increase.

Table A.1: Descriptive Statistics, at age 47 of the grandmother

	(1) Full Sample	(2) Control (1947-50)	(3) Treat (1944-46)
Panel A: Grandmothers			
Share treatment cohort	0.54	0.00	1.00
	(0.50)	(0.00)	(0.00)
Eligible (UE-criterion)	0.11	0.12	0.11
	(0.32)	(0.32)	(0.31)
Grandmother employed	0.83	0.83	0.82
	(0.38)	(0.37)	(0.38)
Grandmother retired	0.09	0.09	0.09
	(0.28)	(0.28)	(0.28)
Number of observations	*182,950*	*100,119*	*82,831*
Panel B: Daughters			
Age	20.95	20.80	21.14
	(5.08)	(5.17)	(4.97)
Number of children	0.28	0.27	0.29
	(0.64)	(0.63)	(0.65)
Childless	0.81	0.82	0.80
	(0.39)	(0.38)	(0.40)
Number of Abortions	0.12	0.13	0.12
	(0.42)	(0.43)	(0.41)
Never abortion	0.90	0.90	0.90
	(0.30)	(0.30)	(0.30)
Oral Contraceptive Pill	0.48	0.51	0.45
	(0.50)	(0.50)	(0.50)
Morning-after Pill	0.02	0.02	0.02
	(0.15)	(0.15)	(0.15)
Share age 15-20	0.31	0.31	0.31
	(0.46)	(0.46)	(0.46)
Same municipality as GM	0.69	0.70	0.69
	(0.46)	(0.46)	(0.46)
Partnered	0.36	0.35	0.37
	(0.48)	(0.48)	(0.48)
In education	0.39	0.40	0.39
	(0.49)	(0.49)	(0.49)
Number of observations	*263,248*	*143,011*	*120,237*

Notes: Averages taken at age 47 of the grandmother, except for contraceptive pill (planned and morning-after) where averages are taken at age 52 of the mother, the age at which we have data on prescriptions for all cohorts. Standard deviations in brackets.

Table A.2: Estimated effect on abortion, contraceptive behavior, and fertility.

	(1) # abortions	(2) Never abortion	(3) Morning-after pill	(4) Pill	(5) # children	(6) Childless
Expansion	-0.012*	0.014***	-0.005**	-0.011	0.020**	-0.011**
	(0.006)	(0.004)	(0.002)	(0.009)	(0.010)	(0.005)
No obs	4,291,364	4,291,364	1,134,144	2,507,502	4,291,364	4,291,364
baseline mean	0.163	0.877	0.027	0.494	0.570	0.672

Notes: The table shows fertility outcomes for the daughters. The table displays regression coefficients from eq. (2), showing the difference (in differences) between daughters of eligible and not-eligible grandmothers. The dependent variable refers to the daughters and is number of abortion (column (1)), an indicator for never having an abortion (column (2)), an indicator for a purchases of the morning-after pill (column (3)) and an indicator for purchases of the pill (column (4)), number of children (column (5)) and an indicator for being childless (column (6)). The baseline mean is calculated for the control cohorts across all data years used, i.e., for those born 1947-1950 who were not eligible with respect to the UI-criterion. Standard errors are clustered at the grandmother level. *** p < 0.01, ** p < 0.05, * p < 0.10.

Robustness Analysis. We conduct three sets of robustness and sensitivity checks. First, we examine whether the restriction of treatment and control birth cohorts affects the estimation results (see Table A.3). In Panel A, we extend the treatment group to include grandmothers born in 1943. This cohort is only partially treated, as individuals could enter the scheme at age 51 (see Figure 1). Including this cohort does not alter the results. In Panel B, we instead restrict the control group by excluding the 1950 birth cohort. This adjustment has only minor effects on the estimates, although the effect on the number of children becomes statistically insignificant. In Panels C and D, we further restrict the sample: Panel C includes cohorts born between 1945 and 1948, while Panel D focuses on only two cohorts born in 1946 and 1947. Across these specifications, the estimated effects decline slightly and often lose statistical significance, except for outcomes related to the morning-after pill. Overall, the results appear robust to alternative definitions of the grandmother birth cohorts.

Second, we assess the sensitivity of the results to the definition of eligibility (UI criterion). In the baseline specification, eligibility requires four years of UI membership prior to age 47 and an unemployment rate of at least 22.5 percent during that period. We relax this definition by requiring only two years of UI membership and an unemployment rate of at least 11 percent over the preceding two years. Both the estimated effect on grandmothers' retirement and the estimated effects on daughters' fertility outcomes remain largely unchanged, with only minor differences in point estimates (see Table A.4).

Finally, we test the robustness of our findings to the estimation method by applying the Callaway-Sant'Anna approach (Callaway and Sant'Anna, 2021) - due to the staggered nature of our event study- i.e., individuals turning 50 in different years. We define the event as turning age 50 for the eligible group (UI criterion). We then estimate average treatment effects for each cohort g, $ATT(g)$, where cohorts are the different grandmother-birth-cohorts. We use the robust estimator of Callaway and Sant'Anna (2021) to estimate

$$ATT(g) = \mathbb{E}\left[Y_i(g) - Y_i(\infty) \mid G_i = g\right],$$

where $Y_i(g)$ denotes the potential outcome of individuals i if they first turned 50 at time g. $Y_{it}(\infty)$ is the potential outcome of individuals who were never treated - i.e., those not eligible under the UI criterion. We control for daughter fixed effects, year fixed effects, and daughter age fixed effects.

We then use the group-specific weights of Callaway and Sant'Anna (2021) to estimate the effect of the TR reform as the difference between the weighted average effects of the treatment birth cohorts (1944-1946) minus the control cohorts (1947-1950). Note that for the contraceptive variables (pill and morning-after pill), we do not have any pre-event data for the oldest cohorts (1944-1945) because they have turned or will turn 50 in 1995, when the contraceptive data becomes available. The Callaway-Sant'Anna estimator cannot deal with such groups that only contribute to the estimation for the post-effect and thus drops them. Note that, therefore, the effects of the contraceptive outcomes are driven purely by one treatment cohort, namely 1946; therefore, we have a much smaller sample here. In Table A.5, we present the estimation results. The results are consistent with those obtained in the baseline specification.

Table A.3: Robustness: Estimated effect on abortion, contraceptive behavior, and fertility. Varying sample.

	(1) # abortions	(2) Never abortion	(3) Morning-after pill	(4) Pill	(5) # children	(6) Childless
Panel A: Expand to 1943						
Expansion	-0.013**	0.013***	-0.005**	-0.011	0.017*	-0.009**
	(0.006)	(0.003)	(0.002)	(0.009)	(0.009)	(0.004)
No obs	4,867,928	4,867,928	1,236,485	2,777,253	4,867,928	4,867,928
baseline mean	0.163	0.877	0.016	0.494	0.570	0.672
Panel B: Restrict to 1944-1949						
Expansion	-0.009	0.013***	-0.005*	-0.012	0.015	-0.009*
	(0.007)	(0.004)	(0.003)	(0.009)	(0.010)	(0.005)
No obs	3,746,837	3,746,837	1,044,555	2,173,213	3,746,837	3,746,837
baseline mean	0.165	0.875	0.016	0.489	0.575	0.669
Panel C: Restrict to 1945-1948						
Expansion	-0.004	0.009**	-0.006**	-0.006	0.012	-0.006
	(0.008)	(0.005)	(0.003)	(0.010)	(0.012)	(0.006)
No obs	2,566,342	2,566,342	779,727	1,516,747	2,566,342	2,566,342
baseline mean	0.169	0.872	0.016	0.484	0.578	0.667
Panel D: Restrict to 1946-1947						
Expansion	-0.010	0.008	-0.008**	-0.003	0.011	-0.006
	(0.011)	(0.007)	(0.003)	(0.011)	(0.017)	(0.008)
No obs	1,309,334	1,309,334	432,916	791,888	1,309,334	1,309,334
baseline mean	0.171	0.871	0.016	0.476	0.582	0.664

Notes: The table shows fertility outcomes for the daughters. The table displays regression coefficients from eq. (2), showing the difference (in differences) between daughters of eligible and not-eligible grandmothers. The dependent variable refers to the daughters and is number of abortion (column (1)), an indicator for never having an abortion (column (2)), an indicator for a purchases of the morning-after pill (column (3)) and an indicator for purchases of the pill (column (4)), number of children (column (5)) and an indicator for being childless (column (6)). Each Panel varies the estimation sample: Panel A expands the sample by adding grandmothers born in 1943. In Panels B-D, the set of birth cohorts considered is successively restricted, reducing the sample gradually. The baseline mean is calculated for the control cohorts across all data years used, i.e., for the treatment birth cohorts in each sample version who were not eligible with respect to the UI-criterion. Standard errors are clustered at the grandmother level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.4: Estimated effect on abortion, contraceptive behavior, and fertility. Relaxing the UI-eligibility criterion

	(1) # abortions	(2) Never abortion	(3) Morning-after pill	(4) Pill	(5) # children	(6) Childless
Expansion	-0.011*	0.011***	-0.006***	-0.008	0.015*	-0.008*
	(0.006)	(0.003)	(0.002)	(0.008)	(0.009)	(0.004)
No obs	4,325,143	4,325,143	1,143,189	2,527,502	4,325,143	4,325,143
baseline mean	0.162	0.877	0.016	0.494	0.566	0.674

Notes: The table shows fertility outcomes for the daughters. The table displays regression coefficients from eq. (2), showing the difference (in differences) between daughters of eligible and not-eligible grandmothers. The dependent variable refers to the daughters and is number of abortion (column (1)), an indicator for never having an abortion (column (2)), an indicator for a purchases of the morning-after pill (column (3)) and an indicator for purchases of the pill (column (4)), number of children (column (5)) and an indicator for being childless (column (6)). The baseline mean is calculated for the control cohorts across all data years used, i.e., for those born 1947-1950 who were not eligible with respect to the UI-criterion. The definition of eligibility is adapted here to be less strict by allowing less AKASSE membership (2 years) and less unemployment, at least 11 percent over the preceding two years. The first stage result for mothers' retirement is 0.160 (0.006), so slightly smaller than the main first stage.

Table A.5: Estimated effect on abortion, contraceptive behavior, and fertility. Using Callaway-Sant'Ana estimator

	(1) # abortions	(2) Never abortion	(3) Morning-after pill	(4) Pill	(5) # children	(6) Childless
Expansion	-0.011***	0.007**	-0.003	0.001	0.012*	-0.015***
	(0.004)	(0.003)	(0.002)	(0.007)	(0.008)	(0.004)
No obs	4,400,671	4,400,671	1,136,638	2,476,445	4,400,671	4,400,671
baseline mean	0.163	0.877	0.016	0.494	0.570	0.672

Notes: The table shows fertility outcomes for the daughters. The table displays regression coefficients from eq. (2), showing the difference (in differences) between daughters of eligible and not-eligible grandmothers. The dependent variable refers to the daughters and is number of abortion (column (1)), an indicator for never having an abortion (column (2)), an indicator for a purchases of the morning-after pill (column (3)) and an indicator for purchases of the pill (column (4)), number of children (column (5)) and an indicator for being childless (column (6)). The baseline mean is calculated for the control cohorts across all data years used, i.e., for those born 1947-1950 who were not eligible with respect to the UI-criterion. Instead of a simple triple Difference-in-Differences, this table uses the estimator by Callaway and Sant'Anna (2021). The first stage result for mothers' retirement is 0.199 (0.006), thus slightly stronger than our main approach.

B Data appendix

Register Data. Our main sample consists of grandmothers born between 1944 and 1950 and their daughters. We restrict daughter observations to ages $15 - 43$ and use annual data spanning the period $1985 - 2009$. The analysis draws on several Danish administrative registers, including the Abortion Register, the Prescription Drug Register, the Income Register, the Labor Market Register, and the Wealth Register. For our main variables on fertility, our sample consists of about 4.3 million daughter-year observations; for the contraceptive variables, it is smaller due to data availability. Prescription data is only available from 1995, and the morning-after pill became available over-the-counter in 2002, which is why we restrict to the years 1995-2001 for analyses with this outcome. On the grandmother level, we have about 3 million grandmother-year observations.

Variable Definitions:

Abortion. The Abortion Register contains information on all abortions performed within the Danish public healthcare system since the legalization of abortion in 1973. We construct two measures: (i) the total number of abortions an individual has had since 1973,[11] and (ii) an indicator variable equal to one if the individual has never had an abortion since 1973.

Contraception. We use data from the Prescription Drug Register, which covers all purchases of prescription medications in Denmark from 1995 onward. Between 1995 and 2001, emergency contraception (the morning-after pill) required a prescription; for this period, we define an indicator equal to one if the individual purchased emergency contraception within the year. Oral contraceptives required a prescription throughout the entire sample period, and we construct an annual indicator for the purchase of oral contraceptives.

Fertility. Using the Fertility Register, we identify all births to the daughters in our sample. From this information, we construct two measures of fertility: (i) the total number of children and (ii) an indicator for being childless.

Location. Using information on the municipality of residence by January 1 of the grandmother and daughter, we construct an indicator for the two living in the same municipality.

Partner. We construct an indicator for whether the daughter has a partner or not, and we use the following definition of couples: (i) married or registered couple, (ii) having at least one child together and living on the same address, (iii) two adults (above 16) of opposite sex, living on the same address, not closely related, with an age difference below 15 years and where no other adults are living in at the same address.[12]

[11] This is equivalent to abortions after age 14 as the oldest daughters are 14 years old in 1973, since the daughters in our sample are born in the period 1959-1994.

[12] This is the definition of couple Statistics Denmark uses, see https://www.dst.dk/da/TilSalg/data-til-forskning/generelt-om-data/dokumentation-af-data.

Wealth. Wealth is measured as bank deposits (cash holding in bankbooks and saving accounts), bonds (market value of bonds held in custody) and mortgage deeds (market value of mortgage deeds held in custody).

Retirement. Based on the Income Register, we define an indicator for TR retirement equal to one if the individual receives benefits from the TR scheme. We also construct a broader retirement measure that includes participation in any public retirement program relevant for the age group, including disability pension, the TR scheme, and the early retirement pay (ERP) scheme.

Income. Using the Income Register, we define labor income as the sum of wage earnings and income from self-employment.

Employment. We define grandmothers to be employed if they have positive labor income and are not retired.

TR eligibility. Eligibility for TR depends on membership in an Unemployment Insurance (UI) fund and the degree of unemployment. In the main specification, we require that the grandmother has been a member of a UI fund for at least four years prior to the reference year (age 47) and has accumulated at least 10.8 months of unemployment between ages 44 and 47.[13]

1995-Cohort Survey Data. Danish Longitudinal Survey of Children (*Børneforløbsundersøgelsen* in Danish) is a longitudinal survey of children born in Denmark to Danish mothers between 15 September and 31 October 1995 and their families. The study follows the cohort up to age 18 and consists of seven waves of data collection. In this paper, we use the first two waves. Wave 1 was conducted in 1996, when the children were approximately $4-5$ months old, i.e., around the time when maternity leave typically ends. The maternal response rate in this wave was about 90 percent. Wave 2 was collected in 1999, when the children were approximately $3\frac{1}{2}$ years old, corresponding to the age at which most children attend kindergarten; the response rate in this wave was 88 percent (see Ottesen (2011)).

The survey data are linked to our main sample. Out of the ca 5,400 total survey respondents, ca 1,500 of them are also in our data as their mothers were born between 1944 and 1949. Note that we exclude those with mothers born in 1950, as those mothers did not reach age 50 by the time of the second survey. However, not all women answered all questions, and we exclude those who answered "do not know", so our effective sample size ranges around 2,400 observations over the two waves.

The survey includes a wide range of questions covering both child development and fam-

[13] This is equivalent to being on average 22.5 percent unemployed during those 4 years.

ily conditions.[14] In the analyses, we focus on the following two questions that are asked in both wave 1 and 2:

- Q. 102/Q. 52: If a situation arises where you need help with childcare, for example due to illness or unexpected work, who can you count on receiving help from...? your parents?
  Answer options: Always, Often, Sometimes, Rarely/Never, Not applicable (do not have), Don't know.

  (In Danish: *Spm. 102/Spm. 52: Hvis der opstår en situation, hvor du/I har brug for hjælp til at få passet barnet, fx sygdom eller pludseligt arbejde, kan du/I så regne med at få hjælp fra.*[15]
  Dine forældre?:
  Svar: Altid, Ofte, Af og til, Sjældent/aldrig, Har ikke pågældende, Ved ikke)

- Q. 101/Q. 54: If you need help with practical matters such as cooking, cleaning, laundry, or other household chores, can you count on receiving help from...? Your parents?
  Answer options: Always, Often, Sometimes, Rarely/Never, Not applicable (do not have), Don't know.

  (In Danish: *Spm. 101/Spm. 54: Hvis du/I har brug for hjælp til praktiske problemer som fx madlavning, rengøring, vask eller andre huslige gøremål, kan du/I så regne med at få hjælp fra?*
  Dine forældre?
  Svar: Altid, Ofte, Af og til, Sjældent/aldrig, Har ikke pågældende, Ved ikke)

The variables used in Table 2 are constructed in the following way: We construct indicators equal to one if respondents report *always* receiving help with either child care or household tasks, and zero otherwise. For our analysis, we pool data from both waves.

[14] For the documentation and questionnaire of the survey see https://www.vive.dk/da/temaer/forloebsundersoegelser/ and https://digidata.rigsarkivet.dk/aflevering/37983.

[15] The question is formulated slightly different in wave 1 (Spm. 102: Hvis du/l har brug for hjælp til at passe barnet, kan du/l så regne med hjælp fra: