Effects of policy on fertility: A systematic review of (quasi)experiments

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Abstract:
This paper describes the results of a systematic review of the literature of policy effects on fertility after 1970 in Europe, USA, Canada and Australia. Empirical studies were selected through extensive systematic searches, with subsequent literature list screening. Inclusion was conditional on implementing an experimental or quasi-experimental design. 57 published papers and recent working papers were included, covering the topics of parental leave, childcare, health services, universal child transfers and welfare reforms. Childcare and universal transfers seem to have the most positive effects on fertility. Few effects were found for parental leave, but this could be linked to these reforms not lending themselves to efficient (quasi)experimental evaluation. Withdrawing cash transfers to families through welfare reforms has limited fertility effects. Subsidizing assisted reproductive technologies show some promise in increasing birth rates of women above age 35.

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Discussion Papers	comprise research papers intended for international journals or books. A preprint of a Discussion Paper may be longer and more elaborate than a standard journal article, as it may include intermediate calculations and background material etc.

Sammendrag

Denne artikkelen oppsummerer resultatene fra en systematisk gjennomgang av litteraturen om effekter av politiske tiltak på fruktbarhet etter 1970 i Europa, USA, Canada og Australia. Totalt ble 57 empiriske studier med et spesielt velegnet analysedesign valgt ut etter et omfattende systematisk søk. De utvalgte studiene dekker temaene foreldrepermisjon, barnehagetilbud- og priser, helsetjenester, overføringer og velferdsreformer. Vår kunnskapsgjennomgang viser at økt tilgjengelighet og reduserte kostnader for barnehager samt kontantoverføringer til barnefamilier har de tydeligste positive effektene på fruktbarhet. Få studier finner effekter av foreldrepermisjonsordninger, men dette kan knyttes til at reformene i mindre grad egner seg for (kvasi-)eksperimentell evaluering. Reduserte kontantoverføringer til familier gjennom velferdsreformer har begrensede effekter på fruktbarheten. Subsidiering av assistert befruktning viser tegn til økende fødselsrater for kvinner over 35 år.

1 Introduction

The decline of fertility below replacement levels has been met with concern in several advanced economies (McDonald 2006). In 2017, 83 of 201 countries in the world had fertility below replacement levels (United Nations 2018). At the same time, many of these countries allocate large budget shares to family support in different forms. In 2015, 66 percent of the European governments and almost 40 percent of Asian governments had policies to raise fertility or at least impede further decline (United Nations 2018). Within Europe, cross-country studies show that extensive public support to families correlates with higher fertility (see e.g. Gauthier and Hatzius 1997; Kalwij 2010; Wood, Neels, and Vergauwen 2016). Seminal studies assessing within-country change over time find that fertility trends often follow policy change closely (Buttner and Lutz 1990; Hoem 1990; Rønsen and Skrede 2010). However, strong and stable overall economic conditions facilitate costly policies and may themselves contribute to relatively high fertility, questioning whether (and which) family policies are the key driver.

A small but growing literature of (quasi-)experimental studies tries to isolate fertility effects of specific policies. While quasi-experimental studies aim at finding effects of single policies and bear lower risk of interpreting other societal changes as policy effects, they constitute a «lower bound» for actual policy effects because spillovers induced by the policy usually are not captured (see also Olivetti and Petrongolo 2017). Suprisingly, results from such studies have not yet been summarized in an updated and systematic review. This literature review aims to fill this gap by synthesizing studies that take an experimental or quasi-experimental approach in studying the effect of policy on fertility. Our review is limited to countries within Europe, USA, Canada and Australia. Despite institutional and cultural differences, these countries have all experienced the increase of the two-income family and falling fertility over the last half century. Hence, our systematic review complements a large literature of comparisons between these countries and across time, deepening our understanding of the interplay between public policy and fertility decisions.

2 Theoretical starting point

Raising children takes time and money, and public policies can influence fertility by affecting these resources. In its simplest form, the economic theory of the family postulates that the number of children a couple chooses to have depends on the amount of time and money they have, as well as their preferences for spending that time and money on children or other purposes (Becker 1991). Policies such as cash transfers to families, tax breaks for parents, subsidized childcare and parental leave directly affect parents' time and budget constrain through increasing family income or reducing

the direct or indirect (opportunity) cost of children. But also, policies not directly targeting families affect family resources and the cost of children, e.g. health care subsidies and housing[1]. If children are a "normal good" (i.e. a good for which consumption increases in income), increased income or decreased costs will translate into larger family sizes. However, several mechanisms make the expected relationship between resources and fertility more complicated.

First, having more resources could make parents wish or feel obliged to invest more in each child, e.g. provide better housing or schooling. This would in turn increase the cost of raising a child and could reduce the demand for children. Such a quality-quantity trade-off (and the preference for quality above quantity) can lead to (counterintuitive) negative income effects (Becker 1991).

The expected relationship between resources and fertility is further complicated by the fact that (at least one parent in) most families earn the bulk of their income in the labour market, and that several family benefits such as tax breaks for parents and most parental leave benefits depend on active employment. With increasing wages and stronger attachment to the labour market, the losses from taking time off work to care for children (the opportunity cost) increase, too. This *substitution* effect complicates a precise understanding of the fertility effect of employment related benefits. Unconditional cash transfers do not invoke a substitution effect. In contrast, tax breaks for parents and several parental leave benefits typically strengthen parents (i.e. mothers) labour attachment and could invoke the substitution effect. Their expected effect on fertility is hence more theoretically ambiguous.

Policies may also influence fertility by redistributing the time cost of childbearing between the parents. Time costs have been disproportionally taken by mothers, and if this has dampened fertility, policies aiming to shift the costs to fathers may have pro-natalist effects (e.g. Goldscheider, Bernhardt, and Lappegård 2015). However, such effects will emerge only if father's increased cost does not negatively impact fertility more than the positive effects from mothers reduced burden.

In addition, one should expect substantial variation in policy responses in different population groups (Hakim 2003). A simple example is that reduced kindergarten fees relieve the family budget and

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[1] House prices might affect fertility in two different directions. First, housing is a major cost associated with family increases. High housing prices might supress fertility through increasing the costs of having a(nother) child for those who would need more living space. At the same time, for homeowners an increase in house prices implies an increase in wealth. This could have positive effects on their fertility. But housing prices do also reflect the general prosperity of an area which could have effects on fertility independent of own wealth or the costs of living space. There are several ways through which policies affect and regulate the real estate market. However, in all studies which came across our search, variations in the cost of housing come from variations in real estate market prices over time and between areas. These are mostly not resulting of policies and hence outside the scope of our review.

reduce the price of future children, but not for families with a strong preference for parental care over formal care. Similarly, cash transfers conditional on not using formal care constitute an income/price effect for families positive to home care but should not directly influence families with a firm preference for formal care. Parental leave benefits reduce substitution costs in the first period of childrearing, if they compensate income losses from taking time off work to care for children. However, the policy is less relevant for ineligible families where the main carer already is out of paid work, or for parents who prefer to return to work quickly regardless of compensation. Individuals have incomplete information about the costs and benefits of (further) childbearing (Goldthorpe 1998). Parents will tend to have more information than the childless, potentially leading to different fertility responses to policies.

Importantly, fertility is also influenced by norms, fecundity and regulation costs (Crimmins 1985). Politically influencing norms and preferences regarding fertility choice is typically seen as both difficult and, in liberal democracies, largely undue (Schultz 2015). Hence, policies that affect fertility will typically work through affecting the time and money available to parents. In this literature review, we include relevant policies regardless of their aim, be it fertility increase, welfare-to-work-initiatives or simply cutbacks driven by budgets deficits. We note that policies may have an explicit pro-natalist (or anti-natalist) intent, and that these intentions may have effects in their own right. For instance, a welfare reform designed to reduce nonmarital childbearing sends a strong signal that this is unwanted behaviour, while a "baby bonus" emphasizes childbearing as wanted by society.

Of the policies we consider, some types of health services, such as health services for children or perinatal care, work through income and price effects and are theoretically akin to transfers. Other types of health services directly influence regulation costs. More specifically, when contraception and abortion is cheap and accessible, the cost of preventing unwanted pregnancies falls, and so should fertility. Our starting point is, however, that fertility is a private choice that is enabled or constrained by the context provided by public policies. Policies that use restrictions of elective abortion as a means of fertility increase will not be considered. We will not consider the literature on availability of contraception specifically but consider fertility effects when the cost of contraception is changed as part of a package of changing costs of health services.

3 Evaluating effects of policies on fertility

Identifying the causal effect of policies on fertility requires research designs that overcome selection problems, i.e. selective implementation and/or uptake, and confounding unmeasured factors (omitted-variable bias). How different model specifications can lead to contradictory conclusions is demonstrated for example in Rindfuss et al. (2007), showing that a naïve estimation of the association between childcare availability and fertility provides a negative relation between these two in Norwegian municipalities. Conversely, a specification that accounts for the non-random distribution of childcare facilities across the country shows the expected positive effects. Public childcare likely expanded faster in areas where women's work-family conflict was most pressing, and where fertility initially was lower. If one is interested in the causal effect of providing public childcare on these women's fertility using a good counterfactual is crucial – how would *their* fertility have looked if there was no/less/more public childcare?

The studies included in this review use (more or less) formalized strategies to tackle the above-mentioned identification challenges. They use advanced panel data models, experiments, or analytical designs exploiting reforms as natural experiments to get good comparison groups. This part briefly introduces how effects of policies on fertility are measured in the included studies and discusses some general traits of the different strategies, i.e. experimental studies, regression discontinuity designs, difference-in-differences analyses and fixed effects panel regression (see also Angrist and Pischke 2009).

Randomized experiments where a benefit is randomly given to some persons (treatment group) and not to others (control group), provide the most obvious opportunity for evaluating the causal effect of that benefit. However, for practical and ethical reasons experiments are rare, and external validity may be limited if experiments create superficial settings.

Regression Discontinuity-designs (RD) use naturally occurring random variation in treatment eligibility. They are suitable when arbitrary cut-offs define who is affected by a policy change. In the included studies, most often the birthdate of a child defines whether old or new legislation applies. If the cut-off indeed is set arbitrary and if it is not possible for parents to select into treatment status (e.g. to time delivery or conception), those being just ineligible should be similar to those being just eligible and therefore constitute a good comparison group. Rigorous tests and placebo analyses usually come with credible RD studies.

Difference-in-differences estimation (DiD) builds on the same logic. Some groups or units are exposed to policy changes or expansions, while others are not. Within-group fertility changes over time are then compared between the groups to see if the trends developed substantially different among those affected by a policy. A causal interpretation is given based on the assumption of parallel trends absent the policy change. Sensitivity tests, i.e. placebo-analyses, are again much used to strengthen credibility and show the plausibility of underlying assumptions. In cases where confounding trends are identified triple-differenced designs *(DiDiD)* and trend modelling are also used.

Two-way fixed effects panel regression models (2W FE) are a generalized form of difference-in-differences estimation. By using time and unit fixed effects these models effectively control for confounding shared time shocks and time constant differences between units. They provide causal estimates if no unmeasured time-varying variables bias the results. Credible studies provide sensitivity tests.

It makes sense to reflect on how quasi-experimental study designs define treatment and control groups. Who is affected by a policy and who remains unaffected? First, some policies create persisting differences in available resources between population subgroups. For example, when higher monthly cash transfers are given to families below an income limit, treatment and control groups are easily identified, and the challenge is to prove that they are identical (or develop identically) on other characteristics. Other reforms are universal and create only temporary differences (around the implementation period) between treated and untreated families. For example, in regression discontinuity designs extensions of (universal) parental leave are frequently evaluated based on eligibility differences imposed by reform implementation dates and one child's birthdate to define treatment and control groups. In most cases, parents in the control and treatment group both would receive longer leave for the next child. Hence, the incentives for continued childbearing are identical.

In such a case another analytical distinction is useful to bear in mind. Policies can reduce existing costs of children already conceived (current child effect) and/or lower the anticipated cost of children yet to be born (future child effects). Some studies of the fertility effects of policies distinguish between these current and future child effects (Lalive and Zweimuller 2009; Raute 2019), also referred to as income and price effects when the reform affects the monetary cost of childbearing. Whether such a distinction is possible depends on the nature and time-horizon of the policy reform and the analytical design used to evaluate the effect. If applicable, it is expected that temporary differences between treatment and control groups in experiences with current children (induced for example by the

implementation date of a parental leave reform) will have less influence on fertility choices than persistent differences between two groups that apply also to the next child.

Typically, policy effects on fertility outcomes are measured both in the short- (e.g. timing of first births or spacing to the next birth), as well as in the long-run (e.g. number of children several years after a reform or completed fertility). Especially when reforms only induce short-term differences between comparison groups, timing effects are more easily detected than quantum effects. Having children earlier might also raise the total number of children, because more fecund years remain for subsequent births. Alternatively, families may reach their target number of children quicker, giving a subsequent fertility drop.

The estimated effect of a reform cannot simply be translated to represent the fertility effect of the benefit itself, and vice versa. First, a comparison of fertility between treatment and control groups seldomly recognizes social interaction effects, which may impact policy take up as well as fertility behaviour also beyond the directly affected population. Social interaction effects have for example been shown in take-up of parental leave both for mothers (Welteke and Wrohlich 2019) and fathers (Dahl, Løken, and Mogstad 2014). Such interdependencies may affect fertility outcomes of the control group, and comparing fertility responses between treated and 'untreated' parents would underestimate the policy effect (see Olivetti and Petrongolo 2017). Furthermore, policy effects may be nonlinear, e.g. the first weeks of parental leave or a certain threshold for public childcare availability might be most important. If so, the lack of effect of subsequent extensions will give little information of the policy's total importance. Finally, in several evaluation designs the reform effect on fertility must be interpreted as average effect of the benefit on individuals who use the benefit only because of the policy reform (compliers). Individuals with strong preferences for having parental leave or childcare (always takers) might in many cases be able to find opportunities also in the absence of universal policies and their gains must be expected to be higher (as discussed in the previous chapter).

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4 Methods

This chapter briefly describes the search and selection process, and how narrative synthesis is used to synthesize the results. Details can be found in the protocol (Fauske, Hart and Bergsvik 2020). The project is also pre-registered at PROSPERO (Hart, Bergsvik, and Fauske 2019).

4.1 The process of search and sorting

The *bibliographic database search* was carried out using relevant social and medical science databases (Epistemonikos, Social services abstracts, Cochrane library, Medline, Web of science, Popline, Sociological abstracts, RePec). The original search string constrained outcomes to various measures of fertility, and, for larger databases, constrained methods to those with potential for causal inference. No constraints were set for the explanatory variable (intervention). In a next step, the original search string was modified and extended with key words for two types of interventions, family policy and housing. Together, these searches generated 17 228 unique hits.

Empirical studies were included if they fulfilled the *selection criteria* regarding participants, intervention, comparison, outcomes and study design (PICOS) (Liberati et al. 2009). Our criteria are described in Table 1, with further details in the protocol. In addition, recent review articles were included for literature list screening if they reviewed articles that matched our PICOS criteria well.

A PRISMA diagram (Liberati et al. 2009; Moher et al. 2009) documenting the *screening process* is found in Figure 1. Titles and abstracts from the 17 228 articles found through the systematic search were screened for relevance and method by two researchers, using the web application Rayyan (Ouzzani et al. 2016). When studies were included for full text reading, reference lists were screened for relevant articles, that again were read in full text by two researchers. A total of 13 review articles was also screened (Balbo, Billari, and Mills 2013; Blank 2002; Gauthier 2007; Hantrais 1997; Lichter and Jayakody 2002; Lopoo and Raissian 2012; Mills et al. 2011; Neyer and Andersson 2008; Olivetti and Petrongolo 2017; Pirog and Ziol-Guest 2006; Tach and Edin 2017; Thévenon and Gauthier 2011; Thévenon and Luci 2012). In sum, 332 articles were read in full text by two researchers, of which 57 constitute the final sample.

Two researchers were involved in evaluating the risk of bias in the studies included, with a third to resolve disagreement. *Bias assessment* was done by evaluating the extent to which assignment was (conditionally) random (quasi-random, quasi-experimental), and by evaluating tests for conditional randomness (see Angrist and Pischke 2009). Results were considered more credible and given greater
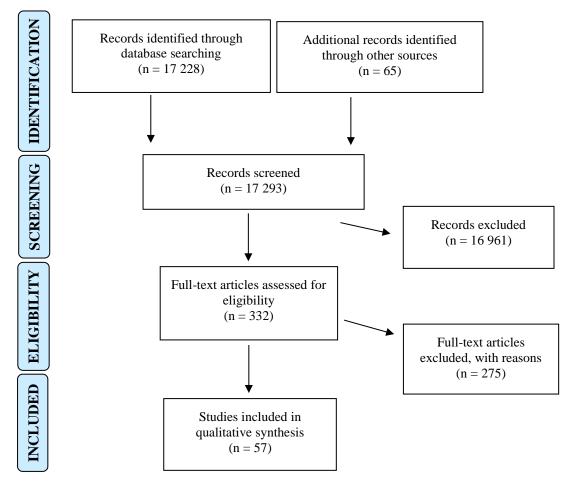
weight in the narrative synthesis if robustness checks were done for fertility outcomes specifically and linked to the subgroup/outcome where a significant effect (if any) was found.

Table 1: PICOS for inclusion and exclusion.

CRITERIA	INCLUSION	EXCLUSION
PARTICIPANTS (POPULATION)	1. Populations of nations fully located in Europe (excluding e.g. Turkey and Russia), Northern America (Canada and USA) and Australia. 2. Women or men of childbearing age during the intervention.	1. Teenage pregnancies. 2. Romania, due to a particularly coercive pro-natalist regime under Ceausescu that may generally limit external validity.
INTERVENTIONS	1. Intervention is a policy, implemented at the national, regional or local level. 2. Intervention happened after 1970. 3. The intervention affects the fertility choices of the population.	1. The intervention directly limits participants free choice by restricting access to contraception or abortion. 2. The intervention effects on fertility are unduly complex or indirect, making the intervention an obviously inefficient means of affecting fertility.
COMPARATOR/ CONTROL	1. The introduction/revocation of a policy is compared to the absence/presence of the same policy. 2. Modifications of a policy are compared to the same policy in its previous form. 3. Two different policy treatments are compared.	
OUTCOMES	1. Birth rates measured at aggregate (sub-national) level. 2. Birth probabilities measured at individual level. 3. Period ("timing") measures. 4. Cohort ("quantum") measures.	1. Outcome is measured at country level.
STUDY DESIGN	1. Field experiments 2. Quasi-experiments: difference-in-differences, regression discontinuity and instrumental variable design, and any combination of these. 3. Two-way fixed effects, or area fixed effects with detailed controls for period and cohort.	1. Observational studies that do not use the strategies mentioned for causal identification. 2. Fixed effects are measured at a higher level than treatment.

Note: for further details, see protocol (Fauske et al., 2020).

Figure 1: PRISMA diagram

IDENTIFICATION

Records identified through
database searching
(n = 17 228)

Additional records identified
through other sources
(n = 65)

SCREENING

Records screened
(n = 17 293)

Records excluded
(n = 16 961)

ELIGIBILITY

Full-text articles assessed for
eligibility
(n = 332)

Full-text articles
excluded, with reasons
(n = 275)

INCLUDED

Studies included in
qualitative synthesis
(n = 57)

From: Moher, Liberati, Tetzlaff, Altman, The PRISMA Group (2009).

4.2 Narrative synthesis

Our analysis of the material is a narrative synthesis guided by the four steps developed by Popay (2006, see also Ryan 2016). Chapter 5 of this paper gives a detailed description of each of the included studies in terms of both text and overview tables, structured by type of intervention. The discussion in Chapter 6 focuses on patterns in data, in terms of intervention type, evaluation design, context and subgroups. We also critically assess the completeness of evidence, and variation in this across type of intervention, as well as our applied methods for evaluation of bias (i.e. the validity of the identification strategies).

5 Description of patterns by type of intervention

5.1 Parental leave

Parental leave gives parents (mothers) the right to take time off from work in relation to a birth and new-born care while being granted to return to the pre-birth job afterwards. Job-protected parental leave comes unpaid, state-paid and employer-paid, and can fully or partly compensate for income losses during the absence. Long-term costs, for example in the form of a worse income development after the absence might remain. The extent of and eligibility criteria for parental leave compensations vary considerably between countries and/or states, and they often depend on mothers' employment status or earnings prior to the birth. In addition, many countries (e.g. Norway, Sweden and Spain) also reserve some weeks for fathers.

Parental leave reforms have led to plenty of policy variation over time within countries. Such reforms, or in one case the introduction of parental leave itself, are used in all 11 studies included in this review. Four studies examine effects of general parental leave expansions (in length or compensation), while four studies examine effects of introducing or expanding the paternity leave. Two studies look at fertility effects of going from means tested to earnings related maternity leave benefits. Studies are summarized in Table 2.

Parental leave was introduced in the United States in 1993 through the Family and Medical Leave Act (FMLA). FMLA provided 12 weeks unpaid job-protected leave to employees with stable employment at a covered employer over the previous 12 months. Cannonier (2014) compares fertility trends between women fulfilling the eligibility criteria and not eligible women in a difference-in-differences design and finds an increased probability of having a first and second birth, as well as earlier births among eligible women after the introduction of FMLA.

The Nordic countries were among the first to implement extensive parental leave schemes, and five studies in this review examine reform effects of these. Dahl et al. (2016) use samples of mothers giving birth around the implementation dates of six parental leave expansions in Norway between 1987-1992 in a regression discontinuity design. They only find a small effect of the 1992 reform on the number of children born to mothers 14 years after and conclude that, overall, the expansions of paid leave did little to encourage fertility.

Table 2: Studies of parental leave

AUTHORS (PUBL. YEAR)	INTERVENTION	COUNTRY (AFFECTED) IMPL.	MAIN (SECONDARY) OUTCOME	STRATIFICATION	METHOD AND RESULTS
CANNONIER (2014)	Introduction of 12 weeks unpaid job-protected leave (Family and Medical Leave Act)	USA 1993	Birth probability eligible vs. in-eligible women until 2010	Sector; Race and Ethnicity; Education	DiD; Increased 1st and 2nd birth prob.; Earlier births
ANG (2015)	Increased max. insurable earnings and income compens. from 55 to 70% (30 out of 55 weeks)	Canada (Quebec) 2006	Birth prob., age at birth (labor supply) compared to rest of Canada	Parity; Marital status; Age	DiD; Increased birth rates by 23.5%; Particularly 1st and 2nd births
LALIVE, ZWEIMULLER (2009)	Two Parental Leave reforms (flat rate benefit). 1990: 12->24 months + longer speed premium, 1996: 24->18 months + shorter speed premium	Austria 1990 & 1996	Higher order (2nd) births in short run (3 years) and long run (10 years)	Income; Occupation	RD; Positive short run and long run effects; Timing in line with incentives
CYGAN-REHM (2016)	Maternity Leave benefits from means tested to earnings related (+ grace period changes)	Germany (West) 2007	Higher order births within 12/21/24/33/36/45/48/57 months	Employment; Old benefit eligibility; Earnings	DiD; Timing in line with incentives; Neg. persisting effects if low-income; Weak temporary eff. if reform winner
RAUTE (2019)	Maternity Leave benefits from means tested to earnings related (+ grace period changes)	Germany 2007	First and higher order births within 5 years high earning vs low earning women	Age; Parity	DiD; Highly educated more likely to have 1st and 2nd child
DAHL, LØKEN, MOGSTAD, SAL-VANES (2016)	Six Parental Leave extensions (total increase 17 weeks, from 18 to 35)	Norway 1987-1992	Several; Number of children born to a mother 14 years after reform	-	RD; Small effect only in 1992; No general effect
LIU, SKANS (2010)	Parental Leave extension (12 to 15 months)	Sweden 1988/89	Children's school performance at age 16 (Timing and number of future siblings + several)	Education	DiD; No general effect; Small increase in prob. of another child within 18 months among highly educ. mothers
COOLS, FIVA, KIRKEBØEN (2015)	Introduction of 4-week father's quota (compared to 4-week expansion without reserving share for father)	Norway 1993	Several; Parent's number of children 14 years after reform and spacing	Education	DiD; No effect on fertility
DUVANDER, JO-HANSSON, LAPPEGÅRD (2016) *	Introduction of 4-week father's quota	Norway 1993 Sweden 1995	Having another child within 4 and 10 years after reform	Parity; Income; Education	DiD; No general effect; Small effect on 3rd births in Sweden if father low income
FARRE, GONZA-LEZ (2018) *	Introduction of 2 weeks paid paternity leave	Spain 2007	Birth spacing and probability of another child within 6 years	Age	RD, DiD; Longer spacing; Neg. effects, driven by mothers > 30
HART, ANDERSEN, DRANGE (2019) *	Extension of father's quota from 6 to 10 weeks	Norway 2009	Subsequent fertility (within 1-5 years) and union stability	Child sex; Parity; Union type; Education; Age;	RD; No effect on fertility

*Working papers

Focusing on a Swedish parental leave reform from 1989, Liu and Skans (2010) investigate effects of prolonged parental leave on the timing and number of future children. The reform retroactively prolonged the leave period for parents with a birth in 1988/89 from 12 to 15 months. Using a difference-in-differences approach they find a small increase (0.24 percentage points for an additional month of leave entitlement) in the probability of having an additional child within 18 months of the last, which appears to be driven by highly educated mothers. No significant effect is found for the total number of children.

In 1993 Norway introduced a four week «father's quota» in the parental leave scheme. Although fathers also previously were entitled to parental leave, from then on, a share of the parental leave period was reserved for them. Importantly, the 1993 reform extended the total parental leave length. To be eligible for the father's weeks both parents had to fulfil the eligibility criteria for parental leave benefits. Eligibility requires employment in 6 of the last 10 months and income above a certain ceiling. Cools, Fiva, and Kirkeboen (2015) use a difference-in-differences approach to compare the effects of this extension to the 1992 parental leave extension, which came without reserving a share for fathers. They find no effects of introducing the father's quota on parents' child spacing or total number of children 14 years after the reform. Using a slightly different design also Duvander, Lappegård, and Johansson (2016) find no fertility effects of the introduction of paternity leave in Norway. They do, however, find slightly higher third birth risks for couples with low-income fathers after the introduction of paternity leave in Sweden in 1995.

In 2009 the share of parental leave reserved for fathers was extended from 6 to 10 weeks in Norway. Fertility effects of this extension are studied by Hart, Andersen and Drange (2019). Results from their regression discontinuity analysis show no effects on progressions to further children within 5 years.

Spain introduced two weeks of paid paternity leave in 2007. Farre and Gonzalez (2018) examine fertility effects of paternity leave in Spain using both regression discontinuity and difference-in-differences strategies, finding that the probability of having another child within 6 years was lower and the spacing was longer among eligible couples. Results are mostly driven by mothers over 30.

Using a difference-in-differences design Cygan-Rehm (2016) examines effects of a German maternity leave reform on West German mothers' subsequent fertility within 12/ 21/ 24/ 33/ 36/ 45/ 48/ 57 months. The 2007 reform made maternity-leave benefits earnings related instead of means tested and changed the length of the grace period, securing eligibility for benefits when having a next child

within a short time after the focal child (also called 'speed premium'). Cygan-Rehm (2016) finds that the reform significantly affected the timing of higher-order births in line with heterogeneous economic incentives given by the reform. Negative and persistent effects on the probability of having another child emerged for the lowest-income mothers. In contrast, for mothers who were 'reform winners' relatively weak and temporary positive effects on higher-order births are found.

The same German reform is used by Raute (2019) to compare fertility responses of high earning women, defined as those most significantly treated by the reform, to fertility responses of low earning women in a difference-in-differences analysis. This study is (together with Ang 2015 and Cannonier 2014) one of the few parental leave studies included in this review that examine effects also on first births, finding that after the reform the highly educated were more likely to have a first and second child.

In 2006 the Quebec Parental Insurance Program (QPIP) increased the generosity of parental leave benefits in Quebec through increasing the maximum insurable earnings and the income replacement rate from 55 to 70 percent for 30 out of 55 weeks of the leave period. Using a difference-in-differences strategy Ang (2015) finds that this program increased birth rates by 23.5 percent compared to other Canadian provinces. Effects were particularly strong for first and second parity.

In Austria parental leave comes with a flat rate benefit. A reform in 1990 increased the leave period from 12 to 24 months and prolonged the 'speed premium' for the next child. In 1996 the speed premium as well as the leave period were shortened again to 18 months of parental leave. Lalive and Zweimuller (2009) study effects of these reforms on higher order births using a regression discontinuity design finding that extending parental leave with one year gave about 12 additional children per 100 women. Following the reduction in 1996 they find compressed spacing between first and second births but no effect on the number of second births within three years.

Parental leave summary
In line with the diversity and complexity of parental leave policies, the corresponding fertility effects are highly dependent on the population under scrutiny, the extent of the studied reforms and consequently the differences that reforms create between treatment and control groups (as discussed in chapter 3). While half of the studies identify various timing effects after parental leave reforms, the effects on completed fertility are more ambiguous. No general effects of the parental or paternity leave extensions are found in the Nordic countries (Cools et al. 2015; Dahl et al. 2016; Duvander et al. 2016; Hart et al. 2019; Liu and Skans 2010), and in Spain the introduction of paternity leave even had

negative effects (Farre and Gonzalez 2018). Reforms that altered benefits substantially show more apparent fertility effects (e.g. Ang 2015; Lalive and Zweimuller 2009). Further, studies comparing fertility effects between eligible versus not eligible groups (in the long run) find positive effects on first and second births among eligible women (Raute 2019; Cannonier 2014; Ang 2015). As expected, highly educated women respond better to earnings-related parental leave benefits (Cygan-Rehm 2016; Liu and Skans 2010; Raute 2019).

5.2 Childcare

Access to childcare can reduce the conflict of work and family responsibilities for parents (Presser and Baldwin 1980). Hence, childcare availability, affordability and acceptance are strongly linked to the opportunity costs of childrearing. Childcare can be offered by relatives, bought in the private market or provided publicly. The extent to which these different options are used and available varies considerably between countries, and access to publicly provided childcare, especially for the youngest children, has expanded relatively recent and could in theory influence fertility.

In this review seven studies were included for childcare, summarized in Table 3. Four studies use variation in the availability of local childcare centres over time between municipalities/ counties,[2] while two studies focus on reforms changing the costs of publicly provided childcare. One study uses pension reforms to examine how reduced availability of grandparental care impacts the fertility of the offspring of the generation affected by delayed retirement.

Rindfuss et al. examine the effect of childcare availability on first birth timing (2007) and completed fertility (2010) for the cohorts of mothers born in Norway 1957-1962. Both studies employ two-way fixed effects and use variation in the percentage of pre-school-age children in childcare centres within municipalities between the years 1973 and 1998. For first birth timing they find that increased childcare availability relates to an earlier transition to motherhood as well as higher probabilities of becoming a mother at every age for the age groups 15-19, 20-24, 25-29, 30-35. Rindfuss et al. (2010) extend the focus to total number of children born to women by age 35. They find an increase of slightly more than 0.1 in the average number of children born for each 10 percentage points increase in childcare availability. The increase is significant and positive for all parities, albeit the largest absolute difference is found for transitions to second births, and the largest relative difference for third births.

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[2] Studies relying on a combination of time and *region* fixed effects to identify the effect of childcare on fertility are excluded (e.g. Baizán 2009; Kravdal 1996) . Region fixed effects are considered too broad to fully capture the endogeneity of variation in childcare center placements at the municipality level (as shown in Rindfuss et al. 2007).

Table 3: Studies of childcare

AUTHORS (PUBL. YEAR)	INTERVENTION	COUNTRY (AFFECTED) IMPL.	MAIN (SECONDARY) OUTCOME	STRATIFICATION	METHOD AND RESULTS
RINDFUSS, GUILKEY, MORGAN, KRAVDAL, GUZZO (2007)	Increase in % pre-school-age children in childcare centers	Norway 1973-1998	First birth timing	Age	Two-way fixed effects; Earlier transition to motherhood, and higher probability of becoming mother at every age
RINDFUSS, GUILKEY, MORGAN, KRAVDAL (2010)	Increase in % pre-school-age children in childcare centers	Norway 1973-1998	Total number of children born until age 35	Parity	Two-way fixed effects; Positive effect for all parities, strongest for 2nd and 3rd births
WOOD, NEELS (2019)	Increase in childcare places for 0-3-year olds	Belgium 2002-2005	Probability of having a child among dual-earner couples	Parity	Municipality fixed effects; Positive effect on birth hazard for all parities
BAUERNSCHUSTER, HENER, RAINER (2016)	Expansion of public childcare slots for children under age 3	Germany (West) 2005-2008	Births per 1000 women aged 15-44; Age-specific birth rates (health of newborn)	Age; Marital status; Parity (among married)	Generalized DiD; Positive effect on birth rates, driven by married, strongest for 2nd and 3rd births
GATHMANN, SASS (2018)	"Home care subsidy" reform increased price of choosing public childcare compared to home care	Germany (Thuringia) 2006	Childcare choices (having another child, fertility)	Parity; Family status; Education; Income; Citizenship	DiD; Discourages 1st births; No general effect on subsequent births; Small pos. effects if 2+ children; Stronger if single, low-income, foreign;
MORK, SJOGREN, SVALERYD (2013)	Childcare reform standardized childcare fees and imposed price cap (1998 announcement, 2002 implementation)	Sweden 2002	Child births per 1000 women	Municipality; Household type (children + income); Voting patterns	DiD; Early positive effect on 1st births, particularly if low-income; 2nd births postponed; Higher order births positive price effect, neg. income effect
BATTISTIN, DE NADAI, PADULA (2015) *	Pension reforms delayed retirement = reduces availability of grandparental care	Italy 1992-2001	Fertility of the offspring	Age; Family tie strength	RD; Negative effects on offspring's fertility; Varies by tie strength; Formal childcare can attenuate effect

*Working papers

Wood and Neels (2019) estimate the effect of local childcare coverage in Belgium on the probability of having a child between 2002 to 2005 for the population of dual-earner couples in 2001.[3] The study uses municipality fixed effects and variation in the number of childcare places over the population aged 0-3 from 2002 to 2005. Changes in childcare coverage within a municipality are positively associated with birth hazards. Because the study does not include time fixed effects, common time trends might bias the estimates.

Bauernschuster, Hener, and Rainer (2016) study the effect of local childcare coverage in West German counties on birth rates among women aged 15-44. Using public childcare slots over the population of children under the age of three from 1998 to 2010 they study the fertility effect of several reforms (2005-2008) that led to a large-scale staggered expansion of public childcare for those children. First, in a difference-in-differences framework births per 1000 women are compared between counties with above-median and below-median childcare increases between 2002 and 2009. Then, a continuous measure provides effects using the full variation in childcare availability. Results show that the provision of public childcare had positive effects on fertility. A 10-percentage point increase led to an increase in birth rates of 2.8 percent. Effects are driven by married women and are strongest for second and third births.

Using a difference-in-differences framework Gathmann and Sass (2018) examine effects of the 2006 «home care subsidy»-reform in the East German state of Thuringia. The reform effectively raised the price of choosing public childcare compared to home care. It transferred at least 150 Euros monthly to those not sending their 2-year-old child to public childcare. Fertility responses in Thuringia were compared to fertility developments in other East German states for two samples: first for families with a 2-year-old-child and second for all women aged 18-45. Among families with 2-year-old-children, those with two or more children showed small positive fertility responses to the reform. These families were 'reform winners' because the subsidy was increasing with number of children. Further, fertility effects were stronger for single, low-income, and foreign parents. The effect of the reform on fertility among all 18-45-year-old Thuringian women also varied by the number of previous children, finding that the home care subsidy discouraged first births and had negligible general effects on those with children.

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[3] The dual-earner criteria probably samples a broader and more representative population for first birth probabilities than for second or third births. Because of this restriction, subsample results comparing findings by parity are not commented.

Mörk, Sjögren, and Svaleryd (2013) focus on the fertility effect of a Swedish childcare reform announced in 1998, implementing a user fee cap in 2002. The reform standardized childcare fees across municipalities and imposed a maximum fee cap, which for most families reduced childcare costs. However, new charges per child were dependent on household income and age and number of children. Thus, costs were reduced more for some families than others. Using a difference-in-differences design Mörk et al. (2013) compare before and after reform fertility at the household type and municipality level over the years 1996-2003. Among married couples an early positive effect on first births is observed. Their fertility increased by 9.8 percent, primarily driven by low-income households. Second births were postponed, and higher order births increased with 14.5 percent, but these last effects are only marginally significant.

Finally, Battistin, De Nadai, and Padula (2015) use several Italian pension reforms between 1992 and 2001 that delayed retirement ages to estimate the effect of grandparental availability on their offspring's fertility. Results from the regression discontinuity analyses show that reduced availability of grandparents had negative effects on fertility, especially in families where family ties were stronger. Family ties are measured by an index using several variables about both partners' relationship with the family of origin (i.e. distance, support, contact). Formal childcare availability somewhat attenuated these effects, especially where family ties were weak.

Childcare summary

To sum up, results are in line with expectations. Increasing childcare availability has positive effects on fertility (Rindfuss et al. 2007; Wood and Neels 2019), especially on higher order births (Bauernschuster et al. 2016; Rindfuss et al. 2010). Parents are those directly experiencing the benefits of available childcare, while childcare availability might not be as salient for those not yet having a child. In the same line reduced availability of grandparents has negative effects in a context where grandparental care is important (Battistin et al. 2015).

Changes in the price of childcare also affect subgroups of couples in line with theoretical expectations. Increasing the price of choosing public childcare compared to home care discouraged first births but increased fertility of those known to be more prone to choose home care, i.e. single, low-income, or foreign-born parents and those with many children (Gathmann and Sass 2018). Contrary, lowering and standardizing the prices of public childcare had positive effects on fertility, particularly on first births, and interestingly already after the announcement of the reform (Mörk et al. 2013).

Findings on parity specific responses to changes in childcare availability and prices diverge to some degree. While childcare availability has stronger impact on the fertility of those who already are parents (Bauernschuster et al. 2016; Rindfuss et al. 2010), reforms on the price of public childcare affect parents only marginally. Instead, reform effects emerge for first births, and one could speculate whether the diverging results can be explained by 'announcement effects' of childcare price reforms on those who are not yet parents, while actual availability (without announcement) has stronger effects on those experiencing the gains.

5.3 Health services

Perinatal care and health services for the new child constitute a large proportion of immediate costs of having a child. In extensive welfare states, this cost is carried collectively rather than individually, and will hence not influence fertility choices directly. The cost of health services may impact fertility through three main mechanisms. First, among parents, reduced cost of health care for children already born is a transfer, potentially generating an income effect. Second, reduced costs of prenatal and perinatal care, as well as health services for children, lower the price of the next child. For subfecund couples, reduced costs of reproductive technologies will have a similar price effect. Thirdly, and in contrast, reducing the cost of contraception and abortion reduces regulation costs, potentially inhibiting fertility – particularly in age groups where births tend to be unplanned or unwanted. In sum, reducing the cost of health services potentially has ambiguous fertility effects.

11 studies were included for health services, all based on data from the USA. Studies are summarized in Table 4. Eight studies look at variation in the cost of all health services, stemming from an experiment with free insurance coverage (one study), changes in Medicaid (four studies), the Affordable Care Act (ACA, two studies), and a health care reform in Massachusetts (one study). Three studies look at the effect of reducing the cost of infertility treatment specifically.

Leibowitz (1990) analyses fertility effects of a health insurance experiment carried out in six US cities 1974-1979. In the experiment families were randomly assigned to different insurance schemes, including a fully covered plan (i.e. free health services) for up to five years. Free health services lower the cost of inhibiting conception, as well as the cost of pregnancy, delivery and childrearing. Birth rates were 29 per cent higher among fully covered women than in the control group, an effect that emerged after two-three years. The study cannot conclude whether completed fertility is affected, or births are simply shifted to a period where health services are cheaper (Leibowitz 1990, p. 709).

In the Unites States, Medicaid provides health insurance to women and families with low income and covers a large share of the costs of perinatal care, delivery and health services to children. In the 1980s and 1990s, the eligibility threshold for families with children (including childless but pregnant women) has been expanded multiple times, with substantial variation in timing and level across states (Deleire, Lopoo, and Simon 2011). All four studies on Medicaid effects utilize a state and year fixed effects design and a cell-based estimation strategy, where birth rates are calculated separately by race, educational attainment, and marital status. The earliest Medicaid study by Joyce, Kaestner, and Kwan (1998) finds positive effects of two Medicaid expansions on birth rates. The subsequent studies use more refined and more plausibly exogenous measures of Medicaid availability – a simulated fraction of women eligible (Deleire et al. 2011; Zavodny and Bitler 2010) or/and an expansion threshold (Groves, Hamersma, and Lopoo 2018; Zavodny and Bitler 2010). While Zavodny and Bitler (2010) find a positive effect among women with lower education, Deleire et al. (2011) find no robust effects after detailed controls for demographic characteristics. Groves et al. (2018) is the only study to analyse first, second and higher order births separately, finding significant positive effects on higher-order births, concentrated among women with high school education only.

Two studies analyse an aspect of The Affordable Care Act ("Obamacare") implemented in 2010, where dependents up to age 26 could be listed on their parent's employer insurance. This reduced the cost of contraception and birth/perinatal care among a large share of young adults. Both Abramowitz (2018) and Heim, Lurie, and Simon (2018) use a difference-in-differences design, with unaffected age groups as controls. Both find negative effects on birth rates. Abramowitz (2018) finds (non-significant) indications that increased use of hormonal contraceptives may mediate this effect, while abortion rates are unchanged. Heim et al. (2018) find indications that those not enrolled in post-secondary education drive the effects. Both studies show pre-trend tests as robustness checks.

Apostolova-Mihaylova and Yelowitz (2018) utilize a state-specific expansion of health insurance in Massachusetts in 2006, using neighbouring states as controls in a difference-in-differences design. The reform reduced the cost of all health services and has been seen as a predecessor to the ACA reform. They find an 8% reduction of fertility among unmarried women aged 20-34, where births are often unplanned. Among married women in the same age group, fertility increases by 1%, an unsurprising response to lowering the cost of births in a group where fertility intentions are high.

Table 4: Studies on health services

AUTHORS	INTERVENTION	COUNTRY (IMPL.); AFFECTED	MAIN (SECONDARY) OUTCOME	STRATIFICATION	METHOD AND RESULTS
LEIBOWITZ (1990)	3-5 years of free medical care vs. cost-sharing insurance	USA (1974-1979); Families in 6 cities	Births during experiment; (Yearly birth probability)	-	Experiment. 29% increase in births. Strongest increase after 2-3 years.
JOYCE, KAESTNER, KWAN (1998)	Expansion of Medicaid insurance for pregnancy, perinatal and child health	USA (1987-1991); low-ed. aged 19-27	Birth rates; (Abortion rates; Abortion ratio (to births))	Race	2W FE. Increase in births concentrated among whites.
DELEIRE, LOPOO, SIMON (2011)	Medicaid availability: measured as simulated fraction available.	USA (1985-1996)	Ln(quarterly birth rates)	Race; Marital status; Education	2W FE, aggregated data. No robust relationship.
ZAVODNY, BITLER (2010)	Medicaid availability: measured as simulated fraction available and expansion threshold	USA (1982-1996)	Ln(quarterly birth rates); (Abortion rates)	Race; Marital status; Education (births only)	2W FE, aggregated data. No overall effect of extensions, possible pos. effect on low educ. white women. (Restrictions of abortion funding decrease ab. & increase births.)
GROVES, HAMERSMA, LOPOO (2018)	Medicaid availability: measured as Medicaid threshold rel. to federal poverty limit.	USA (1987-1997)	Ln(quarterly birth rates)	Race; Marital status; Education; Age; Parity	2W FE, agg. data. Pos. effect on higher order births among HS educ. women across race. Extensive checks, incl. limitation to federally initiated changes to avoid self-selection.
ABRAMOWITZ (2018)	ACA insurance: Reduced cost of conception, birth and abortion	USA (2010); Young adults (20-25) with insured parents	Prob. birth in 12 months; (Contraceptive use; Trying to get pregnant; Abortions)	Age; Marital status	DiD. Decrease in births. Increase in likelihood of trying to get pregnant. No effect on abortions. Indication of effect on long-term contraceptives. Pre-trend plots and tests.
HEIM, LURIE, SIMON (2018)	ACA insurance: Reduced cost of conception, birth and abortion	USA (2010); Young adults (20-25) with insured parents	Conception resulting in live birth	Parental income; Marital status; Parity; Postsecondary enrolment	DiD with younger (untreated) as control. Modest decrease in fertility (ITT 7-11%). Robustness incl. tests for pre-trends.
APOSTOLOVA-MIHAYLOVA, YELOWITZ (2018)	Health insurance reform lowered cost of pregnancy and pregnancy prevention	USA (2006); Massachusetts	Yearly probability of birth	Age; Marital status	DiD, individ. level data. Pos. effect on married women aged 20-34 (1%); Neg. effect on unmarried (8%). Robustness checks excluding movers + states w. minor reforms, changing age brackets.
SCHMIDT (2005)	Infertility treatment: State mandate to provide insurance vs. no mandate	USA (1985-1999); 15 treatment states	Ln(first birth) rate	Age>35; Race	DiDiD, aggregated data. 32% increase among women over 35, concentrated among whites.
SCHMIDT (2007)	As above, additionally: Strong or weak mandate; IVF covered or not; Covered proportion of pop.	USA (1981-1999); 15 treatment states	Ln(first birth) rate; Ln(higher order birth rate)	Age>35; Race	DiDiD, agg. data. Pos. effect > age 35 among whites only. No eff. at higher parities; Not dependent on mandate strength; Stronger if large pop. covered; Robustness incl. state specific trends and restr. time series.
MACHADO, SANZ-DE-GALDEANO (2015)	Infertility treatment insurance	USA (1979-2001)	Completed fertility; Age at 1st birth	Race	2W FE/DiD. No effect on completed fertility, some delay of first birth.

Compared to lowering the cost of all health services, lowering the cost of infertility should have more unambiguous fertility effects. Infertility treatment lowers the cost of having children despite fecundity problems, and lowering its cost should increase birth rates among the sub-fecund, who are overrepresented at higher ages. Effectively, cheaper infertility treatment lowers the cost of fertility postponement, potentially causing age at first birth to increase. US states have discretion to allow or require that employer mandated insurance covers infertility treatment, and to specify the types of insurance schemes and infertility treatments to be included. This generates variation in the price of infertility treatment across space and time in the USA, and the three included studies utilize this variation to estimate effects of the cost of fertility treatment on fertility in variations of difference-in-differences designs.

Schmidt (2005) finds a 32% increase in first birth rates among women above age 35, concentrated among whites. Schmidt (2007) expands on this finding, showing that effects are larger when a larger population is covered, and finds no effects at higher order births. Machado and Sanz-de-Galdeano (2015) utilize the same variation to estimate effects on age at first birth as well as completed fertility, finding that cheaper fertility treatment leads to postponed first birth, with no effects on completed fertility. Machado and Sanz-de-Galdeano (2015) use a synthetic control group in addition to a standard DiD-design and offer extensive visual displays of pre-trends. Still, long-term effects on timing of births and completed fertility are inherently difficult to measure in most quasi-experimental designs, warranting some caution in the interpretation of results. The combination of a postponement effect at low ages and a positive effect above age 35 is consistent. There is some tension between a positive effect above 35, driven by couples who would otherwise have struggled to bear children, and no effect on completed fertility.[4] This conflict suggests that further research is required before strong conclusions on the effect on completed fertility can be drawn.

Health services summary
The empirical findings confirm that reducing the cost of health services has ambiguous effects on fertility. Among young adults, results from the ACA reform indicate that fertility is lower when health services are cheaper (Abramowitz 2018; Heim et al. 2018), perhaps due to more consistent contraceptive use (Heim et al. 2018). However, Apostolova-Mihaylova and Yelowitz (2018) find that a similar reform in Massachusetts increased fertility for married women up to their mid-30s. Among women above age 35, positive fertility effects emerge when infertility treatment is cheaper (Schmidt

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[4] Mechanically, this would require a negative effect below age 35, potentially because some couples postpone childbearing due to better insurance and then adapt to a child free lifestyle and remain childless.

2005, 2007), though these effects may be temporary (Machado and Sanz-de-Galdeano 2015). A general reduction in the cost of health services in all age groups, as induced by Medicaid expansions, seems to have a weak positive effect on fertility among high school educated women (Deleire et al. 2011; Zavodny and Bitler 2010), concentrated at higher parities (Groves et al. 2018). Five years of free health care has substantial positive fertility effects, yet these are likely to be temporary (Leibowitz 1990). Despite some conflicting evidence, reducing the price of infertility treatment at higher ages stands out as the most effective strategy to increase birth counts. For welfare states that already offer comprehensive free or low-cost health services, expanding access to infertility services shows some promise in stimulating birth rates.

5.4 Universal child transfers

Cash transfers to families with children raise the family income and reduce the costs of current and future children (income and price effect) and should consequently have positive effects on fertility. However, two factors might dampen these. First, parents may use additional transfers to invest more in children already born (i.e. substitute quality for quantity). The presence of such effects is illustrated by studies showing that transfers improve child health. Second, if transfers are given as tax breaks, they will also invoke a negative substitution effect, potentially lowering fertility. A large empirical literature on the effect of tax breaks on labour supply illustrate the plausibility of such a substitution effect (see e.g. Azmat and Gonzalez 2010).

This review includes eight studies on fertility effects of universal and unconditional cash transfers and tax breaks based on policy changes in European contexts (Spain, Germany, Norway) or other extensive welfare regimes (Canada), summarized in Table 5. Targeted and conditional transfers are summarized in the next section.

Four studies analyze transfer expansions specific to the Canadian province Quebec, using (parts of) the rest of Canada as controls. Milligan (2005) analyses the effect of an increase in cash transfers to families with children in 1988. The increase was particularly marked for third children, and he finds strong effects at third births in a difference-in-differences design. Limited robustness tests for pre-trends are presented. Ang (2015) analyses the same reform (among others), finding effects concentrated at first birth. Despite better micro data, robustness tests and pre-trend inspections are even more limited in the latter paper.

Parent and Wang (2007) replicate Milligan's (2005) result for immediate effects, but using better data, they find no effects on cohort fertility. Analysing Quebec-specific extensions of parental allowance in the 1970, Kim (2014) reaches similar conclusions about effects emerging in the short term but waning in the long term. Overall, the evidence from Canada points toward marked, yet transitory, effects of universal cash transfers on fertility. The many region-specific policy changes in Quebec complicate identification of the precise effect of each policy, particularly in the long term.

Two of the included studies on universal transfers are based on reforms in Spain. González (2013) analyses the effect of a one-time cash payment ("baby bonus") introduced to all Spanish residents in July 2007. The immediate implementation and sharp cut-off of this reform makes it well suited for a regression discontinuity (RD) design. She finds a statistically significant increase in conceptions following the reform, as well as a somewhat smaller significant decrease in abortion rates. Azmat and Gonzalez (2010) evaluate the effect of a 2003 reform of the Spanish income tax, aiming to increase fertility while upholding maternal labour supply. The reform introduced substantial tax breaks for households with young children, and additional deductions conditional on mothers working. Labour supply is analysed using DiD models, while the fertility models use a first difference/RD design. They find a 5% increase in fertility, combined with increased labour supply of mothers of small children. While the 2007 reform allows for stronger causal identification, the evidence taken together indicates that monetary incentives have pro-natalist effects in the Spanish context.

Riphahn and Wiynck (2017) study the effect of a German child benefit reform in 1996. The reform, and hence the identification of effects, is complicated: In general, first births got better subsidized for lower educated (lower earning) couples, while second births were better subsidized for the higher educated (higher earning). The authors use these educational differences for identification in a difference-in-differences design and find positive, but only moderately robust, effects on higher order births among higher educated couples. For first births, an unexpected negative effect emerges for lower educated couples, perhaps attributable to compositional effects with respect to age and geography. The concentration of effects among higher-earning couples is consistent with the findings of Milligan (2005) for Canada. In sum, this study (weakly) supports a positive effect of transfers on fertility.

Table 5: Studies on universal child transfers

AUTHORS	INTERVENTION	COUNTRY (YEAR); AFFECTED	MAIN (SECONDARY) OUTCOME	STRATIFI-CATION	METHOD & RESULTS
ANG (2015)	Cash transfers; incl ANC (Allowance for Newborn Children)	Canada (1980 -->); Quebec	Fertility (Female LS)	Partnered	Parallell DiD. Pos. effect on fertility, stronger for first births.
MILLIGAN (2005)	Unconditional cash transfer increasing in number of children (ANC)	Canada (1988); Quebec	Fertility rates; Probability of having child; (Cohort fertility)	Parity; Family income	DiDiD with lower parities as control. Trend inspections. Strong positive effect on third births where incentive is strongest.
PARENT, WANG (2007)	Quebec-specific expansions of family allowance programs	Canada (Mid 1970); Quebec	Completed (cohort) fertility; (Children < 6 in household)	Age; parity	DiDiD with lower parities as control. Short term pos. effect; no lasting effect.
KIM (2014)	Allowance for Newborn Children (ANC); Age-adjusted exposure to policy	Canada (1988); Quebec	Completed (cohort) fertility	Age	DiD. Main "age-adjusted" measure is endogenous; Robustness w/exogenous measure: No effect on completed fertility.
GONZALEZ (2013)	Universal baby bonus (cash)	Spain (2007); 2y-residence	Fertility (Abortion; LS; Consumption)	-	RD. Positive effect on fertility. Temp. lower LS and less purchased childcare. No eff on consumption
AZMAT, GON-ZALEZ (2010)	Tax credit and child deductions; partly conditional on maternal employment	Spain (2003)	Fertility (Female LS)	Education; Age; Parity	RD or first diff for fertility outcomes. (DiD for female LS). Plus 5% fertility.
RIPHAHN, WIYNCK (2017)	Reform reduced cost of 1st child for low-earning couples, and increased cost for high-earning couples	Germany (1996); West	Fertility	Education	DiD. High/low ed take turns being treated/ control. Positive effect on 2nd births for high ed., unexpected neg. effect on 1st births for low ed.
GALLOWAY, HART (2015) *	Regional cash transfer	Norway (1989-1990); Troms	Fertility (Female LS, Education)	Marital status	DiD. Trend modelling + placebo tests. Positive effect on 1st births among unmarried women.

*Working papers

Galloway and Hart (2015) analyse the effect of increased cash transfers and tax breaks to families with children in Norway. They exploit variation from a regional reform in a difference-in-differences design, using bordering municipalities as controls. The combination of an increased cash transfer to parents, and general tax breaks (largest for mothers with no coresidential partner), gives an increase in nonmarital first births. The result is relatively robust to specification tests and trend modelling.

Universal child transfers summary

Taken together, studies of universal monetary transfers indicate a temporary positive effect on fertility, confirmed by studies from Canada, Spain, Germany and Norway. The limited number of existing studies on completed fertility indicate that effects are transitory.

5.5 Welfare reforms

Welfare reforms analysed in the newer quasi-experimental literature are typically intended to strengthen labour market attachment among welfare recipients and reduce their reliance on cash transfers.[5] The studied welfare reforms have predominantly been implemented in the USA and the UK, both liberal welfare regimes (Esping-Andersen 1990). Of the 21 included studies summarized in Table 6, 18 are from the USA and two from the UK. Interventions include "packages" with job training and work incentives. Larger tax breaks for low-income working parents, e.g. the US Earned Income Tax Credit (EITC), could induce either an income effect or a substitution effect on fertility, while cutbacks in unconditional transfers, such as the US Aid to Families with Dependent Children (AFDC) could reduce fertility.

Two of the included studies examine effects of AFDC utilizing over time within-state variation in benefit size in two-way fixed effects designs. Hoffman and Foster (2000) find positive effects on nonmarital births, yet these are strongly sensitive to specification. Robins and Fronstin (1996) find positive effects on nonmarital fertility for non-white women without a high school degree.

Eleven studies assess the "family cap", which denies further AFDC cash assistance to (higher order) children conceived when their mother is on welfare. A change in fertility due to these reforms could result from normative pressure (as childbearing while on welfare is directly portrayed as an outcome to be avoided), increased substitution costs due to a stronger labour force attachment, or an

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[5] Studies that evaluate child support enforcement are excluded, as this in practice works more as a means to increase men's cost of a nonmarital birth (with potential negative effects on nonmarital fertility) rather than as a transfer to unmarried women (Garfinkel et al. 2003). Further, a large earlier literature on variation in welfare benefits largely relies on methods that are not strictly quasi experimental, and interested readers are referred to the succinct summary by Moffit (1998).

income/price effect from changes in cash transfers. Four studies analyse an experiment in New Jersey 1992-1997, where a sample of about 8 300 participants was randomly assigned to either treatment (new restrictions on cash transfers, plus incentivized job training) or control (no changes). Jagannathan and Camasso (2003) and Jagannathan, Camasso, and Killingsworth (2004) both conclude that the program reduced fertility, but only among blacks with limited prior welfare experience who lived in predominantly non-black neighbourhoods. Camasso (2004) finds that both cash transfer reductions and job training had independent negative effects on fertility. However, Jagannathan, Camasso, and Harvey (2010) conclude that only 2.5% of the effect of the reform package can be attributed to changes in cash transfers, indicating that changes in norms towards childbearing on welfare was an important component of the reform effect. While some general criticisms of this experiment have been raised (Dyer and Fairlie 2004), these studies give the most reliable estimates of the effect of this particular combination of reforms. Fein (2001) analyses a comparable experiment implemented in Delaware 1995-1996, and finds no significant effects on fertility.

Four studies use within-state, across time variation to study effects of family caps in 2WFE design with controls for other welfare changes. They find negative effects on nonmarital fertility among blacks (Camasso and Jagannathan 2009, 2016; Sabia 2008). Horvath-Rose, Peters, and Sabia (2008) find negative effects on nonmarital births, but *positive* effects on marital births. They conclude that the positive effects on marital fertility are too strong to be a response to family cap incentives, and rather indicate that state implementation of family caps is endogenous to fertility trends.

If states implement family caps as a response to increases in fertility, a difference-in-differences design with more careful comparisons of trends, and more plausible control groups will be a more rigoruous identification strategy. Using double- and triple-differenced designs, neither Joyce et al. (2004) nor Dyer and Fairlie (2004) find robust effects of family caps on fertility. Kearney (2004) focuses on higher-order births, as only these are directly affected by the family cap, finding no effects.

Grogger and Bronars (2001) analyse variation in welfare benefits stemming from twin births, comparing duration to the next birth following a twin birth in high-welfare relative to low-welfare states. While lower benefits for children already born slow down parity progression among blacks, variation in benefits for the potential next (marginal) child, as induced by the family cap, had no effect on fertility. In sum, these more rigorous studies do not support that capping transfers to unmarried mothers on welfare limits fertility.

Table 6: Studies on welfare

AUTHORS	INTERVENTION	COUNTRY (YEAR), AFFECTED	MAIN (SECONDARY) OUTCOME	STRATIFICATION	METHOD & RESULTS
HOFFMAN, FOSTER (2000)	Variation in Aid to Families with Dependent Children (AFDC) across time in states	USA (1968-1991); unmarried women <23 y	Nonmarital and marital birth by age 22	Marital status; Age	2W FE. Pos. effect on nonmarital births for women in early 20s. Depends on specification of FE.
ROBINS, FRONSTIN (1996)	Variation in AFDC across time in states	USA (1980-1988)	Fertility	Educ. level; Race	2W FE. Pos. eff. for < HS ed., Black and Hispanic.
JAGANNATHAN, CAMASSO (2003)	New Jersey (NJ) Family cap: package effect	USA (1992-1997); mothers on welfare in NJ	Birth of new child (abortion, contracept., sterilization)	Race; Racial density; New or old in welfare system	Experiment. Neg. effect for black women who are new in welfare system and live in non-black areas.
JAGANNATHAN, CAMASSO, KILLINGSWORTH (2004)	New Jersey (NJ) Family cap: package effect	USA (1992-1997); mothers on welfare in NJ	Birth of new child	Race; Racial density New or old in welfare system	Experiment. Neg. effect for black women who are new in welfare system and live in non-black areas
CAMASSO (2004)	NJ Family cap: nets out job training effect	USA (1992-1997); mothers on welfare in NJ	Birth of new child (abortion, contracept., sterilization)	New or old in welfare system; Race	Experiment. Neg. eff. for short term recipients; Long term eff of JOBs training
JAGANNATHAN, CAMASSO, HARVEY (2010)	NJ Family cap: instruments effect of cash transfer change	USA (1992-1997); mothers on welfare in NJ	Birth of new child	New or old in welfare system	Experiment combined with IV to test per dollar effect of family cap. Weak neg. effect for black women new in welfare system. Mon. effect explains 2.5% of total reform effect.
FEIN (2001)	ABC program: includes job training and parent training.	USA (1995-1996); Delaware	Marriage; Fertility; (marriage and fertility plans)	Age; Parity; Marital status; Schooling; Years on welfare	Experiment. No effect on fertility; Effect on fertility plans.
CAMASSO, JAGANNATHAN (2009)	Family cap implementation timing in states	USA (1980-2000); unmarried	Nonmartial birth rate; (abortion, illegitimacy, nonmarital preg. rate)	Race; Medicaid funds abortions	FE. State trends + controls for other welfare changes. Reduction if low-cost abortion available, concentrated in states with many blacks. Mediated by abortions.
CAMASSO, JAGANNATHAN (2016)	Family cap implementation timing in states	USA (1980-2010); unmarried	Nonmartial birth rate; (abortion, illegitimacy, nonmarital preg. rate)	Race; Medicaid funds abortions	(2W) FE. State trends + controls for other welfare changes. Reduction if low-cost abortion available, concentrated in states with many blacks. Mediated by pregnancies.
SABIA (2008)	Family cap implementation timing in states	USA (1984-1998)	Ln(Nonmarital birth rate); (pregnancy; abortion)	Race	2W FE. Controls for other welfare changes. Neg. effects on black nonmarital fertility; Fewer pregnancies, not more abortions.

HORVATH-ROSE, PETERS, SABIA (2008)	Family cap implementation timing in states	USA (1984-1999)	Nonmarital birth rate; (Marital birth rate)	Race; Age; Marital status	2W FE. State trends + controls for other changes. Neg. effect on nonmarital births. Strong pos. effect on marital births indicates policy endogeneity.
JOYCE, KAESTNER, KO-RENMAN, HEN-SHAW (2004)	Family cap implementation timing in states	USA (1992-1999); higher order births, low ed.	Ln(Higher-order births): For teens: all; For women 20-34: Nonmarital; (Abortion rates)	Race	DiDiD using first births as control. Inspection of pre-trends. Higher order births decline relatively, but not more when family caps are implemented.
DYER, FAIRLIE (2004)	Family cap implementation timing in states	USA (1990s); less educated single mothers	Nonmarital births	-	DiD. No robust effect. Triple diff. with marital fertility as control. Robustness incl. trend modelling + trend inspection and contr. group sensitivity.
KEARNEY (2004)	Family cap implementation timing in states	USA (1989-1998); higher order births	Birth rate	Marital status; Education; Age; Parity	DiD. Test of pre-trends and corr. between fertility level and implementation. No significant effect with state trends.
GROGGER, BRONARS (2001)	State variation in welfare: AFDC + food stamps benefit	USA (1968-1980); unwed mothers	Next birth; Marriage	Race	Twinning. Whites postpone marriage, blacks have child sooner. Only current, not marginal, benefits influence fertility. Relative imp of twins if benefits are high vs. low.
BAUGHMAN, DICKERT-CONLIN (2003)	Earned Income Tax Credit (EITC)	USA (1990-1999)	First birth rate	Race; Marital status	2W FE. Small neg. effect among whites; small pos. effects among non-whites.
BAUGHMAN, DICKERT-CONLIN (2009)	Earned Income Tax Credit (EITC)	USA (1990s)	Birth rates	Race; Educ.; Parity; Marital status	2W FE. Tests with state trends and lags. Small neg. effects for higher-order births for whites.
FRANCESCONI, VAN DER KLAAUW (2006)	Working Families' Tax Credit: tax credit for working parents + childcare expenses	UK (1999); single mothers	LF particip.; (childcare use; marriage; fertility)	Parity; Child age; Age	DiD with single women or single w. without ed. as control group. Trends modelled. Insignificant neg. effect on fertility.
BREWER, RATCLIFFE, SMITH (2012)	UK Welfare-to-work	UK (1999); low educ. households	Fertility (contraception)	Single and coupled; parity	DiD; Increase in births among coupled women with lower ed. (compared to higher ed). Reduction in contraception among low ed.
HOFMANN, HOHMEYER (2013)	Unemployment benefit reform	Germany (2003)	Pregnancy probability	Breadw. model; HH income; Parity	IV: announcement of less generous benefits as instrument for econ. uncertainty. Perfect collinearity with period, but placebos look good. Strong econ. worries reduce pregnancy prob.
GROVES, LOPOO (2018)	Subsidies to students w disabled, retired or deceased parent; cond. on not married	USA (1982-1985); < 23 y.	Ever/age at married/divorced/child	Gender	DiD. Paternal death treatment proxy. Post-reform controls (subsidy was cancelled). Balance-test on covariates. No eff. on compl. fertility; Increases age at 1st birth.

Four studies assess the effects of tax breaks for working parents at the lower end of the income distribution. In the USA, the Earned Income Tax Credit (EITC) seems to have a negative effect on white women's fertility, both on first (Baughman and Dickert-Conlin 2003) and higher order births (Baughman and Dickert-Conlin 2009). For blacks both positive effects on first births (Baughman and Dickert-Conlin 2003) and no robust effects emerge (Baughman and Dickert-Conlin 2009). Potentially, better labour market opportunities among white women invoke stronger substitution effects. A tax break similar to the EITC was implemented in the UK in 1999. Francesconi and van der Klaauw (2007) find no significant effect on lone mother's parity progression in a difference-in-differences design. Brewer, Ratcliffe, and Smith (2012) find that the reform increased fertility among coupled women, where the (partner's) income effect will dominate the (woman's) substitution effect.

Hofmann and Hohmeyer (2013) use the announcement of a 2003 welfare reform in Germany as an instrument for economic uncertainty. The reform tightened unemployment benefits and provided conditions of economic activity comparable to those seen in welfare programs in the USA and UK. They explore if fertility behaviour changed in the period between announcement and implementation, a period in which they show that perceived economic uncertainty increased. Other period changes are an obvious threat to identification but alleviated by placebo and robustness tests. Instrumented this way, economic uncertainty reduced fertility only when perceived subjectively by the woman, and only for higher-order births. The results indicate that economic reforms may influence fertility beyond their effect on resources available to the family.

Finally, Groves and Lopoo (2018) investigate fertility effects of substantial US federal aid to students with one deceased or disabled parent in a difference-in-differences design. Aid was provided up to age 22 conditional on being unmarried. An untreated, post-program group is available as the program was revoked in the mid-80s. Statistically insignificant DiD-estimates when control variables are outcomes support a valid identification strategy. Results show no effects on completed fertility, but an increase in age at first birth. The latter outcome is (endogenously) conditioned on ever having children, which could compromise causal identification. The results indicate that students respond to economic incentives when it comes to fertility timing, perhaps leaving some room for incentivizing earlier parenthood among students.

Welfare reforms summary

The discussed welfare reforms typically aim to strengthen labour supply, with reduced (nonmarital) fertility as a more or less intended side effect. While microeconomic theory predicts that fertility may be influenced, effects are in practice very limited. Stronger labour market attachment is sometimes

achieved, and sometimes translates to lower fertility through a stronger substitution effect. Withdrawing cash transfers seem to have very limited effects on fertility, and non-monetary channels seem paramount for any effects found.

The contrast between the small effects in the welfare literature and the comparatively larger effects from universal transfers (see Section 5.4) indicates that different population groups respond differently to monetary incentives, and, potentially, that the *intention* of the policy matters. Pro-natalist policies might show stronger effects when these intentions are clearly announced. On the other hand, announcing policies that increase economic uncertainty may have the opposite effect.

6 Discussion

6.1 Patterns by study type
In this review, study type refers to two distinct yet interrelated elements: The nature of the intervention, and the nature of the evaluation design.

Patterns by intervention
The studied interventions are extremely varied, but based on the extensive summaries above, some general patterns emerge. Three groups of policies tend to impact fertility positively. First, increased availability and reduced cost of childcare have positive fertility effects. Second, lowering health care costs may have some general positive effects on fertility through lowering the cost of children. Most importantly, however, reducing the cost of assisted reproduction has a positive impact on fertility in age groups where subfecundity is high. Third, universal transfers to families with children tend to increase fertility, even if they are given as tax breaks. All these three groups of policies are evaluated with credible identification strategies.
For two groups of policies, few effects on fertility are found. First, various reductions in welfare payments, predominantly in the USA and UK, seem to have very small or no effects on fertility. The design of these policies allows for rigorous evaluations of income and price effects. The absence of effects hence indicates that no effects exist.

A second group of policies that, somewhat more surprising, yield few effects, is parental leave. Long compensated parental leaves constitute very large transfers to parents, and their (yearly) value will often largely exceed the value of e.g. public childcare subsidies. The sparsity of measured effects might be linked to two characteristics of the reforms themselves. Most importantly, for reforms on

universal parental leave the difference between the control and treatment group tends to lie in temporary experiences with current child(ren) – i.e. the treatment group has had a slightly longer or better compensated leave, or a different division between mother and father. While, strictly speaking, better compensated parental leave gives income effects, this effect will typically be temporary and not affect the flow of income when another child is considered. These types of current child effects are a priori least likely to impact future fertility (see also chapter 3). In addition, must especially the paternity leave evaluations be interpreted as measuring the average effect of the benefit on individuals who use the benefit only because of the policy reform. Fathers with a desire to take leave could mostly do so also in absence of the reforms, with potentially high impact on their fertility which is not captured by the reform evaluation. This is a powerful illustration of the more general point that the estimated reform effect results from a change in treatment experienced by the compliers, and is hence expectedly smaller than the total policy effect. Second, even in countries with very long compensated parental leaves, increases (and hence quasi-experimental evaluations) typically happen in relatively small increments. An exception is the Austrian reform, which increased parental leave from 12 to 24 months and had short- and long-term effects on fertility. So, even if a year of compensated parental leave impacts fertility, effects of smaller increments may be too small to be detected even with large data sets or effects may be nonlinear and not emerge at the evaluated margin. Hence, our observation is that the available studies leave us with insufficient information to conclude.

Patterns by identification strategy

A recurrent feature in our material is that different studies analyse the same reform with different designs and conclusions, providing an excellent window to compare design and identification strategies. Prominent examples of this include changes over time in cash transfers in Quebec, recent German parental leave reforms and over time variation in "family caps" in US states.[6]

In general, "stricter" or more conservative identification strategies will be less likely to yield biased results. Our material indicates that the bias often is away from zero, with more rigorous designs more often indicating no effect. For instance, the family cap literature illustrates that the (less conservative) two-way fixed effects studies are more likely to yield significant reform effects than the (more conservative) double- or triple-differenced designs. The latter designs more efficiently investigate and

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[6] Unfortunately, one may suspect that papers that slightly modify the design of a previous study and get a very similar result are less likely to be published or even submitted. The potential of such publication bias makes it difficult to assess the extent to which reanalysis tend to alter results.

net out deviating trends across treatment and control groups, and are less likely to be biased by the endogenous implementation of reforms.

Running field experiments on policies is expensive and to some extent politically controversial, and our evidence leans heavily on quasi-experimental evaluations. The experiments we include are exclusively from the USA and examine welfare reforms and health insurance expansions. It is noteworthy that in both cases, the estimated experiment effects tend to be larger than effects of comparable policies isolated in quasi-experimental designs. Various explanations could be offered for these differences. First, experiments may entail data sources with less measurement error, which would give less bias towards zero (Mehmetoglu and Jacobsen 2016:136). Second, "announcement effects" or normative effects, may be particularly strong when field experiments are implemented. Finally, experiments tend to be time limited, and individuals in the treatment group may display strong but temporary responses to these temporary changes in the costs of childbearing.

6.2 Effect variations by outcome

The included studies differ in whether the outcome is measured at the aggregated or individual level. We do not observe systematically different results based on this distinction and note that the distinction between the data types is not very sharp as data can be aggregated by a very large number of categories, and individual level data will eventually be aggregated to group means in a regression analysis. Whether the outcome is parity-specific or for all births does matter for results, but we discuss that point under subgroup estimation (next section).

In general, it is easier to detect tempo(rary) effects than effects on completed fertility. Usually measurement error is larger for completed fertility, biasing results toward zero. Changing policies or moving in and out of policy areas/eligibility also means that reform effects will more often be "washed out" for completed fertility. A conclusion of previous reviews has been that tempo is more easily influenced than quantum (Gauthier 2007). While our review does not counter this conclusion, we would like to add that when specific reforms are considered, tempo effects are easier to identify than quantum effects. We are hence reluctant to conclude that public policy does not matter for the level of completed fertility. While the timing of births is of importance for future population structure, completed fertility is even more crucial. Clearly, this question warrants further research.

6.3 Effect variations by subgroup

In line with microeconomic theory, policies that aim at strengthening mothers' labour force participation, such as parental leave, have stronger fertility effects on the higher educated or those with a strong attachment to the labour force. Regarding cash transfers and tax breaks, the most obvious prediction would be that the largest fertility effects would be found in lowest income brackets. In this group, the relative size of a flat transfer will be larger. Empirical studies tend to, if anything, show the opposite pattern, with larger fertility effects in the highest income brackets (see Section 5.4), and generally weaker fertility effects of welfare policy changes, which specifically target low income groups.

Variations by child parity are most apparent for the effect of public childcare. Childcare availability has strongest effects on second and third births, while price reforms seem to affect first births. A potential explanation for this pattern is that those with children base their decision to have another child on their experience with existing childcare supply, while incomplete information among parents-to-be makes them more prone to react to announced reforms. If fertility effects of parental leave are found, they are concentrated at first and second births.

Studies of health services display strong subsample effects, largely consistent with expectations. Young adults are more likely to put cheaper health services toward contraception (lowering births), while lower cost of assisted reproductive treatment yield positive effects only at higher ages where subfecundity is common.

In general, studies that estimate mean population effects only (e.g. Dahl et al. 2016) are less likely to find effects than studies that also look at subsamples. Sometimes, the nature of subsample estimations is obvious from the design of the policy: The family cap yields income effects for unmarried mothers only, and the strongest effects should be expected in this group. Expectations of subsample effects can also be derived from theory, but it is noteworthy that findings do not always confirm to expectations.

A challenge with the subsample estimation, given that results often do not confirm to theoretical expectations, is that of multiple testing. The mean population effect is estimated with one statistical test. Tests by four dichotomized stratifying variables (such as the commonly used ethnic background, education, age and marital status) gives 16 statistical tests, a level at which one will often see at least one false positive result in each study with a 5 percent significance level. Given that pre-registration is extremely rare in this literature, it is difficult to know whether the choice of stratification variables is

derived from theory or post-hoc motivated after extensive testing. A stronger tradition for pre-registration would strengthen the credibility of this literature.

6.4 Effect variations by context and completeness of evidence

In general, our review has revealed a consistency in evidence across contexts. This holds especially regarding results for subgroups, e.g. positive effects of parental leave for highly educated women's fertility in the USA, Canada, Sweden and Germany. However, there is also a tendency of similar reforms being implemented in similar contexts. Very generally speaking, universal transfers, kindergarten expansions and parental leave compensations tend to take place in already relatively extensive welfare states in Europe and Canada, where they tend to positively impact fertility, while less generous welfare states such as the United States tend to implement cutback reforms. Hence, we have limited evidence on how extensions would work in rudimentary welfare states, and how cutbacks would influence fertility in extensive welfare states.

First, effects of cutbacks and extensions are probably not symmetric. While the childcare price reforms in Germany (raising prices) and Sweden (lowering prices) affected similar subgroups, they were too different to compare effect sizes. But at least one study in our material supports that limiting a policy does not have the opposite effect from extending the same policy. In Austria reducing the parental leave length from 24 to 18 months had less impact than the preceding extension from 12 to 24 months (Lalive and Zweimuller 2009). Similarly, we do not know about nonlinearity of the effects (e.g. extensive vs. intensive margins), and last, external validity remains limited even between comparable welfare contexts due to potential welfare complementarity when effects of one policy depend on others.

The federal US system, with substantial regional policy discretion, provides more opportunities for quasi-experimental evaluation than many European welfare states characterized by nationwide rights and reforms. For health services, our empirical evidence is entirely from the US, with potentially limited validity in European welfare regimes. Despite this skewed evidence, we consider the literature on health services to be informative also for the European context. Studies that look at specific health services such as assisted reproductive treatment are relevant for ongoing European discussions on the extent to which such treatments are to be publicly funded.

6.5 Methods of bias minimization

In this review, we have assessed study quality based on the criteria for a valid (quasi-)experiment established in this research literature. We have set criteria for bias minimization in the pre-registration document at PROSPERO (Hart et al. 2019) and elaborated on them in the research protocol (Fauske et al. 2020). While we are aware of more formal strategies developed by Cochrane (2011), we find these to only fit modestly well with an evaluation of the quasi-experimental literature. Compared to a classic Cochrane review, our synthesis and weighting of evidence leans more heavily on expert judgement rather than on pre-defined criteria.

On the other hand, we have more formalistic and pre-defined criteria for bias assessment than is common in literature reviews within the field of demography. We believe that this has added some structure and replicability to our review, perhaps also facilitating a subsequent debate with counterarguments to our judgements. While it would be of interest to develop more rigorous criteria for the assessment and synthesis of quasi-experimental studies, this has been outside the scope of our study.

7 Conclusion

In this review, we have summarized studies of the effect of policies on fertility, based on an extensive and systematic search of both published articles and working papers (> 17 000 screened). We have found five groups of policies that are evaluated with respect to whether they influence fertility: Parental leave, childcare, universal transfers, health services and welfare reforms. Of these, especially childcare, universal transfers and some types of health services tend to have positive effects on fertility.

Concerns about falling fertility are mostly linked to concerns about decline in future labour supply, and countries who aim to increase fertility often tend to simultaneously want to preserve or even increase maternal labour supply (Thévenon 2011). Which of the evaluated policies unite these goals? The most obvious policy is accessible and reasonably priced childcare. In contexts where childcare coverage is high, one can speculate that improvements on accessibility, such as opening hours compatible with non-standard work hours, or quality could have further positive effects. We do not have empirical studies that assess these dimensions, however.

Universal transfers also seem to increase fertility, at least when they are substantial. A challenge with large universal transfers is that they may act as a disincentive to paid work, particularly among women

with many children and lower levels of education or work experience. However, when transfers are given as tax breaks, they will to a lesser degree work as a disincentive to paid work.

Finally, a targeted intervention that may have a small but significant effect is offering subsidized assisted reproductive treatment at all ages where the success rate is of meaningful size. That such services increase birth rates if available at a low cost to couples likely to benefit from them is founded in data.

Second, what does *not* work? The welfare cutbacks seen in the last decades in the USA and UK seem to have very limited impact on fertility. Whether such policy packages in "reversed form" have the potential to *increase* fertility is dubious, and a reform package that increases fertility while reducing labour supply at the lower end of the income distribution also does not seem politically feasible. To the extent that family income is important for child health and wellbeing, such an attempt would also raise some ethical concerns.

Our largest knowledge gap seems to be on the effect of parental leave. This is not due to a lack of studies, but rather because the nature of parental leave reforms makes them difficult to evaluate. Given that long parental leaves are costly, evaluating them in an experimental design akin to the US tradition would provide important insights. It would, however, only be politically feasible to randomly allocate *additional* parental leave benefits or rights, meaning that effects would be evaluated at yet another margin.

Finally, we note that several studies point towards announcement effects of policies. Policies perceived as supportive to families that signal childbearing as valuable to society tend to have more positive fertility effects. In contrast, if increased (female) labour supply is the main policy aim, fertility effects (if any) will be heterogenous across groups, and sometimes negative in sum.

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