Düsseldorf Institute for Competition Economics

DISCUSSION PAPER

No 316

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July 2019

HEINRICH HEINE
UNIVERSITÄT DÜSSELDORF

d|u|p düsseldorf university press

IMPRINT

DICE DISCUSSION PAPER

Published by

düsseldorf university press (dup) on behalf of
Heinrich-Heine-Universität Düsseldorf, Faculty of Economics,
Düsseldorf Institute for Competition Economics (DICE), Universitätsstraße 1,
40225 Düsseldorf, Germany
www.dice.hhu.de

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ISSN 2190-9938 (online) – ISBN 978-3-86304-315-5

Fertility Effects of College Education: Evidence from the German Educational Expansion *

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Abstract

Using arguably exogenous variation in college expansions we estimate the effects of college education on female fertility. While college education reduces the probability of becoming a mother, college-educated mothers have more children than mothers without a college education. Lower child–income penalties of college-educated mothers of two relative to mothers without college up to nine years after birth suggest a stronger polarization of college graduate jobs into family-friendly and career-oriented as a potential explanation. We conclude that policies aiming at increasing female educational participation should be counteracted by policies enabling especially college graduates to have both a career and a family.

Keywords: Family planning, college education, augmented quantity–quality model
JEL Classification: *C36, I21, J13*

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*This is an updated version of a discussion paper released under the same title in October 2017. The authors are grateful to the seminar participants in Paderborn and Düsseldorf as well as to participants at the 2016 IAAE conference in Milan, the 2016 EBE Workshop in Munich, the 2016 and 2019 Essen Health Conferences, the 2016 RGS Jamboree in Dortmund, the 2017 ESPE conference in Glasgow, the 2017 EALE conference in St. Gallen, the 2018 EEA in Cologne, the 2018 German Economic Association conference in Freiburg, and the 2019 DICE Winter School in Saas-Fee. The paper greatly benefited from comments by Johannes Abeler, Sascha O. Becker, Kamila Cygan-Rehm, Martin Karlsson, Fabian Lange, Marion Leturcq, Andreas Lichter, Melanie Lührmann, Magne Mogstad, Chiara Pastore, Anna Raute, Hendrik Schmitz, Ariel Stern, and Nicolas R. Ziebarth. For excellent research assistance, we furthermore thank Anna Werbeck. On-site access to the Micro Census data was made possible by the GESIS–German Microdata Lab, Mannheim, and we would like to thank Bernhard Schimpl-Neimanns for help with the data. Financial support from the German Research Foundation (DFG, Grant number SCHM 3140/1-1) is gratefully acknowledged.

1 Introduction

Among the many changes that have affected developed societies in the past 60 years, two certainly belong to the most significant ones: the educational expansion – describing the substantial upsurge in higher education enrollment, especially that of females – and the fertility transition, characterized by declining fertility rates that have fallen below replacement rates. The resulting consequences of both these evolutions have affected many dimensions of social interaction such as the demographic change – which today constitutes an urgent concern from a policy perspective. While policies that aim at increasing education have been introduced in all parts of the world, many industrialized countries have also set up policies to boost fertility rates. Although the direct impact both kinds of policies is often comparatively well understood due to ample research, the nexus between these policies – that is, how education affects fertility – is still mostly understudied. The negative correlation between education and fertility, sometimes referred to as the "baby gap" between high- and low-educated individuals (Raute, 2019), may hint at potentially unintended consequences that education policies may have for fertility.[1] This study contributes to the understanding whether increased education causes lower fertility.

In order to give credible policy advise on whether a career–family trade-off prevents more educated women to have (more) children, the key challenge is to overcome a self-selection into college, arguably due to differential preferences.[2] Women with initial preferences for large families might be more reluctant to sort into college education as they might expect a reduced payoff period through time of child care. Women with initial career preferences are prone to study, since it fuels their labor market perspectives. To address such a selection we exploit arguably exogenous temporal and spatial variation in the access to college education through a massive build-up of colleges Germany experienced between the 1960s and the 1980s. Several higher education policies at the federal level and within the self-governing states caused the number of colleges in Germany to double and a more than five-fold increase in the number of college spots in the new and existing universities. Importantly, this development can neither be predicted through pre-expansion characteristics of the districts nor could we find evidence of coordinated policies that favor regions with particularly low or high fertility rates.

This empirical strategy, implemented by a basic instrumental variable approach, closely relates our research to two other studies. First, using US data Currie and Moretti (2003)

[1] The ambiguity between education and family policies becomes most visible when comparing industrialized with developing countries. While more education is correlated with smaller families in industrialized countries, the opposite is true in developing countries. Thus, policies that enhance the access to education may actually be a complement to family policies in developing countries. Due to the context and the margin of education we focus on the situation in industrialized countries. See Duflo et al. (2015) and the literature therein for the case in developing countries.

[2] See Barrow and Malamud (2015) and Oreopoulos and Petronijevic (2013) for general reviews of the monetary and non-monetary returns to college education and the inherent self-selection problem.

utilize variation in college access to analyze the effect of maternal education on the offspring's health. Unlike the study at hand, they do not focus on fertility outcomes but merely consider the number of children as a potential channel. A second related paper is Kamhöfer et al. (2019) who use the same variation in the German college build-up to estimate the effect of college education on cognitive abilities and health.[3] We are not aware of any study that explicitly investigates the causal link between college education and fertility in an industrialized economy. Most studies on the effect of education on fertility make use of variation in compulsory schooling laws to address the self-selection into education.[4] The mechanisms underlying the compulsory schooling effects, such as a reduction in teenage pregnancies, are fundamentally different. This renders any extrapolation to college education at least debateable.

The college margin, however, provides a presumably interesting addition to the more often considered fertility effect of secondary schooling for four reasons: First, college education is taught more extensively. The formal time to graduation in the years under review was about 4.5 years – compared to changes in compulsory schooling that, at most, account for one or two years. Second, while compulsory schooling affects individuals at the lower end of the education (and presumably skill) distribution, college affects individuals at the upper end who may react differently. Third, college education falls well into the prime reproductive age of women (and potential fathers) while the largest effects of additional years of compulsory schooling have been found on in-school and teenage pregnancies. Fourth, college education is presumably the most important margin that drives the changes in the educational composition of developed societies in the future. By launching the Higher Education Pact 2020, for instance, Germany has recently made large public funds available in order to further increase access to college education.

These important differences to perviously analyzed secondary schooling place this study in a different policy arena. While many reforms have been undertaken to improve the comparability between a career and a family life, higher education policies are usually not considered in this context. However, college education does not only boost the career itself but also the labor market opportunity costs of having children. Thus, understanding the full consequences of education policy is crucial for implementing sustainable family policies. To get a more comprehensive pattern of how college education might affect fertility, we consider three distinct measures: the total number of children a woman has throughout her fertile ages (so-called completed fertility), the probability of becoming a mother (extensive margin of fertility), and the average number of children mothers have

[3]A working paper by Tequamem and Tirivayi (2015) analyzes the fertility effects of higher education in Ethiopia and find a reduction in family size.

[4]See, for instance, Cygan-Rehm and Maeder (2013) for Germany, Black et al. (2008) for the US and Norway, Geruso and Royer (2014) for the UK, Monstad et al. (2008) for Norway, Grönqvist and Hall (2013) for Sweden, and Fort et al. (2016) for the UK and pooled Continental European countries. McCrary and Royer (2011) consider changes in the school entry age that cause variation in education.

(intensive margin). Our results indicate that college education reduces the average number of children per women by 0.31. However, decomposing the overall effect into both margins, as suggested by Aaronson et al. (2014), is crucial: we find that college education reduces the probability of becoming a mother by one-quarter, but college-educated women who do become mothers have, on average, 0.27 more children (about 13 percent) compared to their peers without college education.

To gain a better understanding of what drives these effects, we set up an augmented version Becker and Lewis (1973) quantity–quality model that not only allows to distinguish the costs of children by inputs as well as the differential margins (as in Galor, 2012, and Aaronson et al., 2014) but also by the potential college decision of the mother. This model suggests a stronger decrease in the relative rearing costs between the first and any subsequent child for college-educated mothers compared to mothers without a college education. Explorative evidence on child–income penalties supports this hypothesis: lock-in effects into more family-friendly occupations fuel the positive effect of college education on the number of children of mothers. Analyzing, on the other hand, the timing of births indicates that while college education shifts the age-of-birth distribution considerably to the right, age-related, biological fertility problems do not seem to matter.

The evidence of a college-induced increase in the career–family trade-off bears highly relevant policy implications: policies that aim at reducing the (financial) burden of having children, especially for highly educated women, seem more promising than one-size-fits-all policies that increase child allowances or maternity leave compensations by a lump-sum, independent of how high the opportunity costs are. This is in line with recent evidence from Germany: Raute (2019) analyzes the effect of a parental leave reform that replaced a lump-sum compensation scheme with a means-tested one. While more educated, high-income families receive a higher compensation after the reform and get, on average, more children; the fertility of low-income families with an unchanged compensation remains the same. This does by no means need to imply that financial incentives alone are a way of closing the baby gap, other measures that benefit college graduates, such as more flexible working hours (Goldin, 2014), do also have the potential to confine the college-induced increase in the career–family trade-off. On the other hand, the likely absence of an involuntary biological effect through infertility suggests that a compression of the time to graduation (as, for instance, part of the Bologna Process or the recent compression of academic school duration from 9 to 8 years in Germany) is less promising in fighting an education-induced amplification of the demographic change.

The remainder of the paper is as follows: Section 2 briefly presents the general trends in fertility and higher education in Germany. Section 3 provides an overview of the college expansion and exploits both the qualitative and quantitative reasons that led to this expansion. The data and the empirical strategy are presented in Section 4. The main results on quantitative fertility effects are presented in Section 5. Subsequently, Section 6 sheds light on how economic forces and the timing of birth potentially shape the detected fertility patterns before Section 7 concludes.

2 Trends in fertility and education in Germany

Using official statistics for the whole population, Figure 1 depicts the development in female college education and fertility over time in Germany. The horizontal axis states the birth cohort. The violet line gives the trend in the share of women per birth cohort who were enrolled in college at the age of 20 (referring to the vertical axis on the left-hand side). While only 5 percent of all women born in 1943 were enrolled in higher education in 1963, the number increased tenfold until the birth cohort 1972. After the baby-booming years succeeding World War II, the average number of births per women dropped from 1.8 to 1.5. The average number of children is assessed at the woman's age of 40 for the birth cohort of the horizontal axis and plotted by the orange line (referring to the vertical axis on the right-hand side).

At first sight, Figure 1 suggests that the initial reduction in fertility was a prerequisite for the boom in female college enrollment. While this may be true, a further, substantial reduction in fertility occurred just after female college enrollment rates soared the most. As preferences for smaller families grew and contraceptive pills (whose commercial launch in Germany was in 1961, just after the cohort of 1940 decided whether to enroll in college) made it easier to meet the preferred number of children and females could "more accurately anticipate their work lives" (Goldin, 2006, p.8), which made human capital investments for women more valuable. This emphasizes how close fertility and female education are interrelated. Using variation in the availability of higher education, the empirical analysis in the following sections addresses the underlying causal relationship.[5]

Although completed fertility (as assessed at age 40) is not available for more recent cohorts, the trend in female higher education participation remains on an increasing trajectory – and so does the baby gap. This suggests that the pattern of a college-induced fertility change is not confined to the past, but persists in the current political debate.

[5]For similar trends in the development of educational participation and the average age at first marriage, see an earlier version of this discussion paper, Kamhöfer and Westphal (2017).

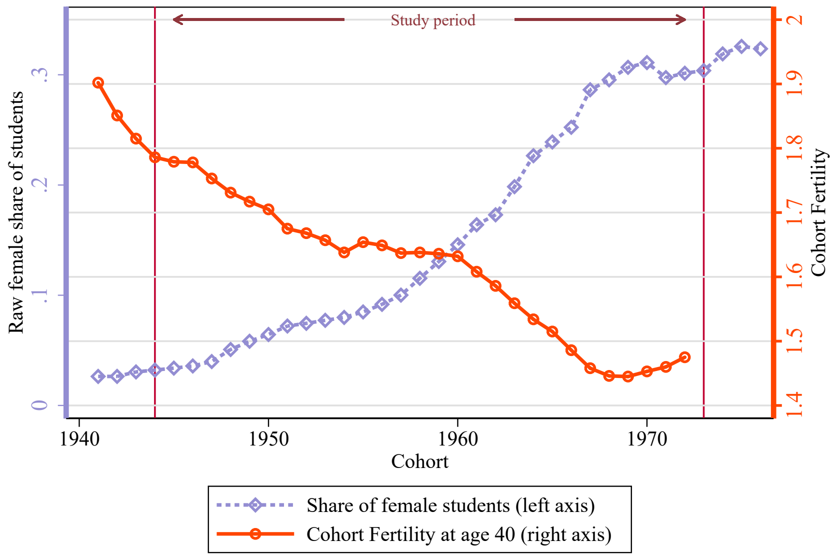


Figure 1: Trends in fertility and college enrollment by birth cohort in Germany

Notes: Own calculations using data from Max Planck Institute for Demographic Research and Vienna Institute of Demography (2014) and German Federal Statistical Office (2016). The orange line refers to the axis on the right-hand side states the average number of children per women at the age of 40 by birth cohort. The violet line illustrates the share of women of the birth cohort that are enrolled in higher education at the age of 20 and corresponds to the vertical axis on the left-hand site. To transform the number of female students in the enrollment year into the cohort share of female students, we deduct 20 years from the enrollment year and take into account that only about one-fifth of women studying in a certain year are freshmen. We divide the resulting number of female students in total by the average study length of 4.5 years to get the number per year. Finally, we divide the number of female students in a certain year by the female cohort size in this year. Note that this is only a crude adjustment. However, as we are primarily interested in the change of this share over time, we are confident of capturing most of the changes.

3 The college expansion

3.1 Background and developments

Higher education in Germany

After graduating from secondary school, adolescents in Germany either enroll in higher education or start an apprenticeship training.[6] The latter consists of part-time training-on-the-job in a firm and part-time schooling. This vocational education usually takes three years and individuals often enter the firm (or another firm in the sector) as a full-time employee afterwards. To be eligible for higher education in Germany, individuals need a university entrance degree (*Abitur*). In the years under review, only academic secondary schools (*Gymnasien*) with nine years secondary schooling (and four years elementary schooling) could award this degree. The tracking from elementary school to secondary school took (and still takes) place rather early at the age of 10. However, it is generally possible to switch secondary school tracks after any term. Moreover, students could enroll into academic schools after graduating from the other tracks (with four to five years basic track schooling or six years of intermediate track schooling) in order to receive three additional years of schooling and be awarded a university entrance degree.

[6]The general description of education in Germany and the college expansion is closely related to Kamhöfer et al. (2019) and has been adjusted for the purpose of the analysis conducted here.

In Germany, higher education is, in general, free of tuition fees and several institutions offer tertiary education – even though the distinction of the different types is not always straightforward. We limit our analysis to the larger and most established institutions: universities and technical universities. We refer to the union of these institutions interchangeably as "universities" or "colleges." We neglect two groups of higher education institutions. First, small institutions that specialize in teacher education, religious education and fine arts with no more than 1,000 students at the time under review. The second group are universities of applied science (*Fachhochschulen*). They emerged in the 1980s (see Lundgreen and Schwibbe, 2008) and are usually smaller than regular universities, specialize in one area of education, have a less theoretical curriculum, and the style of teaching is more similar to secondary schools. In the time under review, the degree awarded was also distinct.

Build-up of new colleges and the rise in higher education enrollment

While the educational system as described above did not change in the years under review, the number of academic-track secondary schools and colleges significantly increased – providing us with an arguably powerful and exogenous source variation in educational opportunities. In this subsection, we describe the supply-sided expansion in the number of colleges and their capacities in terms of student spots as this is a prerequirement for the trends in college enrollment outlined above. This so-called period of "educational expansion" (*Bildungsexpansion*) started in the 1960s and peaked in the 1970s. In the years under review, 1958–1990 (determined by the birth cohorts in our survey data), the number of districts with at least one college (only very few districts had more than one college) increased from 27 to 54 (out of 325 districts) and the total number of students increased by over 850,000 from 157,000 in 1958 to more than one million in 1990 (see Figure 2a). The number of female students in total in the colleges in the sample in Figure 2b is similar to the corresponding number in Figure 1. This indicates that our college panel captures the bulk of the higher education institutions in Germany (although we do not have any data on smaller institutions, see above). Figure A1 in Online Appendix A shows the spatial variation over time. Following the reasoning of Card (1995) and many others since then (e.g., Currie and Moretti, 2003, Carneiro et al., 2011, and Nybom, 2017), we argue that availability of higher educational opportunities in large parts of the country led to a decrease in the opportunity costs of education due to the changed distances to college. While newly opened academic schools enabled secondary school students in rural areas to receive a university entrance degree, college openings in smaller cities allowed a broader group of secondary school graduates from both rural areas and cities to take up higher education. That is, the opening of new colleges allowed individuals to commute instead of moving to a city with a college (which causes higher costs) or decreased the commuting time. As indicated in Figure 2b, women especially benefited from this development as the share of women relative to men doubled from 20 to 40 percent in the time under review.

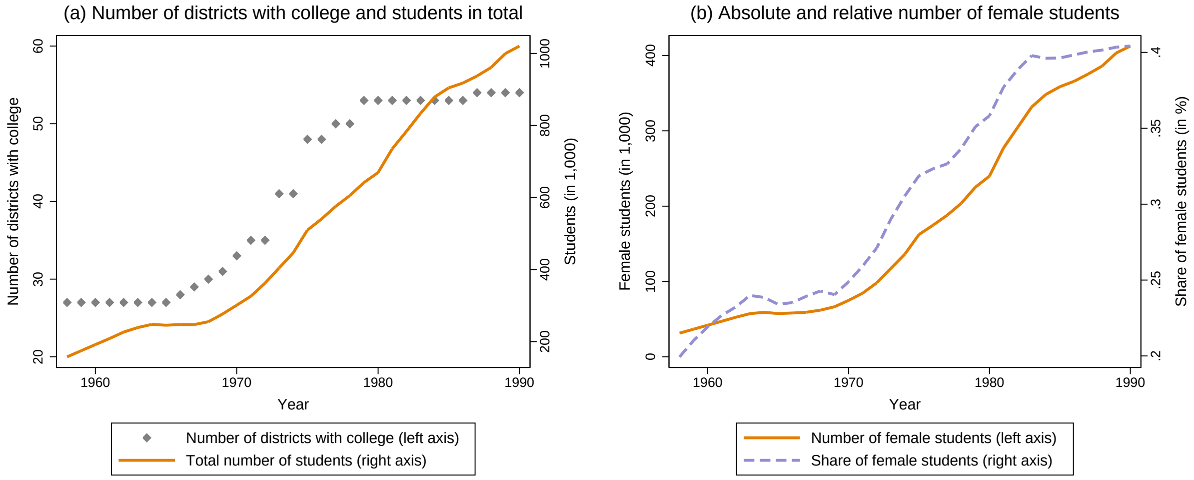


Figure 2: Colleges and students over time and by gender

Notes: Own illustration. College opening and size information are taken from the German Statistical Yearbooks 1959–1991 (German Federal Statistical Office, various issues, 1959–1991). The information on students refer to the college included in the left panel of the figure. More specialized higher education institutes that are smaller in size are disregarded as information on them are often missing.

3.2 Determinants of the college expansion

According to the analysis of Bartz (2007) of the history of higher education in Germany, mainly four factors triggered the college expansion: (i) The two world wars and the National Socialists' "anti-intellectualism" led to a low educational attainment for large parts of the population – as also argued in (Picht, 1964, p.66).[7] Therefore, large parts of society may have had an urge to catch up in terms of education. (ii) Similar to the development some decades earlier in the US described by Goldin and Katz (2009), the German industry demanded more qualified workers that were able to cope with new production technologies (see also the review of the history of the first post-war era colleges of Weisser, 2005). (iii) As argued in Jürges et al. (2011) and Picht (1964), political decision-makers saw education both as an outcome and a means in the rivalries with the communist East Germany. (iv) All these reasons also led to an increase in academic track secondary schools – as analyzed by, e.g., Kamhöfer and Schmitz (2016) and Jürges et al. (2011) – which then led to an increase in the number of individuals eligible for higher education.[8]

[7] Even today, more than 70 years later, the share of college students in Germany still does not meet OECD standards, see OECD (2015) – even so this is at least in part due to the prominent role of the apprenticeship training system in Germany. To close this gap and increase participation in higher education the German federal government and the state governments launched the Higher Education Pact 2020 (*Hochschulpakt 2020*) in 2007 and funded it with 38.5 billion Euros until 2023.

[8] Figure A2 shows the trend in academic-track secondary schooling. Two facts stand out: First, even in the expanding academic secondary schooling the share of female students rose disproportionately until women outnumbered men at academic secondary schools in 1990. Second, even in 1950 the share of women

It was partly because of these reasons that the federal government introduced the German Council of Science and Humanities (*Wissenschaftsrat*) in 1957, see Bartz (2007). In its 1960, 1966, and 1970 reports the expert council advised that college capacities should be largely increased (see Wissenschaftsrat, 1960, 1966, 1970). However, the council's authorities were (and still are) limited to making suggestions. The governments of the federal states in Germany are in charge of educational policies. The coordination between the states (which are usually ruled by several parties or coalitions of them and have elections at different points in time) mainly focuses on a standardization and mutual recognition of degrees. Figure A3 shows the number of colleges and shares of female students over time across the states. The timing of the educational expansion exhibits large differences between the states. In our analysis we use the variation in the timing between the 325 German districts (smaller administrative units, e.g., cities, that are nested in the federal states). Combining administrative data on the college expansion with survey data on individuals that face the college decision spread over more than 30 years yields a panel structure in college availability. Eventually, this allows us to control for district fixed effects (as well as district-specific time-trends), which enables us better to imitate our hypothetical experiment in mind: comparing within all districts fertility decisions of two high-school-graduation cohorts, one just before a college opens up and one after a college has been opened. This would allow us to credibly attribute any of these differences to college education, as a college opening primarily changes the educational opportunities.

In the following parts of this section we provide qualitative and quantitative evidence that this variation is exogenous with respect to individual fertility and career preferences.

Qualitative evidence

While the decentralized decision-making process makes it hard, if not impossible, to trace back the exact political reasons that led to each college opening or expansion in college size, we found evidence of the political reasoning behind some college openings. The first post-war college opening – the University of Bochum in the most-populated state of North Rhine-Westphalia in 1966 – was based on a state's parliament decision in 1961. According to Weisser (2005), the first negotiations between the city of Bochum and the state government were even partly held in secret. This offended officials of the city of Dortmund, that also hoped to get the college, but was unable to provide a construction site that fulfilled the requirements. Facing state elections, the decision to open a college in Dortmund was made only one year after the announcement to open a college in Bochum.

leveled at some 40 percent. The excess in the number of women eligible to take higher education compared to the number of women actually enrolled in colleges suggests that the academic school expansion might have been an important reason for the surge in female college participation but that it was certainly not the only one.

The decision to open six new so-called comprehensive colleges (*Gesamthochschulen*) in North Rhine-Westphalia at the beginning of the 1970s was accompanied by a more intensive public debate. After several parliamentary hearings, the suggestion of the state's minister for educational affairs to construct new colleges in areas without existing ones was agreed on, see NRW (1971b,c). Four of the six colleges were opened in industrialized cities (Duisburg, Essen, Hagen, and Wuppertal) and two colleges were opened in more rural areas (Paderborn and Siegen). The college openings in these districts were supposed to actively "promote" education ("*Bildungswerbung*") and allow a larger range of secondary school graduates to enroll in higher education, see NRW (1971a).

All in all, we neither know of any law that relates college openings to potential reasons (like population size) nor could we find a pattern in the discussions to open colleges. On the contrary, the length of the political process and time from the opening decision to the start of the teaching exhibits a lot of variation. To investigate further which factors are associated with college openings, we conduct an additional quantitative analysis.

Quantitative evidence

Our concern regarding the exogeneity of college expansion is that certain characteristics, such as average fertility, age and living arrangements plus employment structure, systematically differ between regions with a college opening through the educational expansion and a region that had not experienced a college opening. To investigate this, we combine the data on college openings presented above with administrative data from the German Micro Census in 1962 (a 1 percent sample of the whole population, see Lengerer et al., 2008). Because the Micro Census data is on a slightly broader level we observe 249 regions (in which the 325 districts are nested). While 22 of these regions already had a college before 1962 and 206 regions had no college until 1990 or later, a college was opened in 21 regions in the years under review.

Table 1 shows the 1962 means of the regional characteristics that potentially triggered a college opening. Column 1 states the mean for regions that never experienced a college opening and column 2 gives the corresponding mean for regions that experienced a college opening in the time under review. Column 3 gives the difference in means between the two. This reveals no significant difference between the regions in terms of number of children, marital status, share of females or other socioeconomic indicators such as share of migrants and unemployment rate. The share of students is lower in regions with an opening and where the employment structure differs slightly (more primary sector employment in districts with opening). This illustrates that colleges were often opened in order to foster accessibility for rather educationally alienated groups. In column 4 of Table 1, we regress an opening on all characteristics simultaneously. The stated coefficients give the difference of the factors in regions with and without a college opening while

Table 1: Balancing test of regions with and without a college opening in the time under review using administrative data

	(1)	(2)	(3)	(4)
	Regions…			
	…w/o college opening	…w/ opening 1962-1990		Predict opening using regression
Potential college determinant	Mean	Mean	Diff.	OLS
Number of kids per capita (total population)	1.497	1.437	−0.15	−0.033
	(0.522)	(0.283)	(0.121)	(0.052)
…students	0.016	0.011	-0.008^{*}	−10.723
	(0.019)	(0.011)	(0.004)	(10.653)
…divorced	0.023	0.017	−0.005	−1.00
	(0.069)	(0.006)	(0.016)	(40.185)
…widowed	0.088	0.091	0.007^{**}	20.035
	(0.015)	(0.008)	(0.003)	(20.357)
…females	0.525	0.528	0.002	−20.918
	(0.041)	(0.013)	(0.01)	(10.851)
…migrational background	0.021	0.018	−0.006	−10.698
	(0.022)	(0.017)	(0.005)	(10.545)
…unemployed	0.002	0.002	0.001^{**}	250.484
	(0.001)	(0.001)	(0.00)	(190.743)
Sectoral composition of employment				
- primary	0.029	0.046	0.023^{*}	0.39
	(0.055)	(0.053)	(0.013)	(0.497)
- secondary	0.543	0.551	0.008	0.147
	(0.088)	(0.069)	(0.02)	(0.367)
# of regions	206	21	227	227

Notes: Own calculation using German Micro Census data from 1962 (see Lengerer et al., 2008). Information on colleges are taken from the German Statistical Yearbooks 1959–1991 (German Federal Statistical Office, various issues, 1959–1991). Due to data policy restrictions Micro Census data are aggregated on regions defined through the degree of urbanization (*Gemeindegrößenklasse* indicators) and broader administrative units (*Regiergungsbezirk* level). This aggregation results in 206 regions that never experienced a college opening until 1990 or later (the mean value of the considered characteristics in these regions is given in column 1), 21 regions with a college opening between 1962 and 1990 (mean value in column 2), and 22 regions that already had a college in 1962 (data of these regions is not considered in the table). Due to a different aggregation of the Micro Census data, these numbers do not exactly correspond to those on the district level. The difference in column 3 is calculated by a simple regression of a college opening indicator on the potential characteristic and an intercept. Column 4 shows the coefficients of the characteristics in a multiple regression. The number of regions with and without a college opening differs slightly from Kamhöfer et al. (2019) as we restrict our analysis to universities that had 1,000 or more students in at least one of the years under review. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

holding the mean differences in the other characteristics constant. The regression does not find any single factor in 1962 that significantly predicts an opening in the years until 1990.

4 Data and empirical strategy

4.1 Survey data and important variables

German National Educational Panel Study

Our main data source are individual-level data from the German National Educational Panel Study (NEPS), see Blossfeld et al. (2011).[9] NEPS data map the educational trajectories of more than 60,000 individuals in total. The data set consists of a multi-cohort sequence design and samples six age groups: newborns and their parents, preschool children, fifth graders, ninth graders, college freshmen students, and adults. These age groups are referred to as Starting Cohorts and are followed over time. That is, each Starting Cohort consists of a panel structure.

For the purpose of our analysis we make use of the Adult Starting Cohort that covers individuals born between 1956 and 1986 in, so far, seven waves between 2007/2008 (wave 1) and 2014/2015 (wave 7)[10], see LIfBi (2015). Starting with about 8,500 women, the final sample includes 4,300 women who (i) were educated in West Germany, (ii) are aged 40 or older, (iii) did not became a mother in high school, and (iv) have complete information in key variables. One of those key variables is the district of residence at the time of the college decision or earlier, which we use to assign our instrument. Besides detailed information on education and fertility, including the years of childbearing, the data includes retrospective information on the respondents' labor market history and early living conditions at age 15, for instance, the number of siblings, secondary school grades, and parental education. As those factors are potentially confounding the effect of education on fertility, we consider them as control variables, see Online Appendix A, Table A1 for details.

The explanatory variable "college degree" takes the value 1 if an individual has any higher educational degree, and 0 otherwise. Dropouts are treated as all other individuals without college education. About one-fifth of the sample have a college degree, while four-fifth do not.

Dependent variables

The key dimensions along which we analyze fertility are the extensive margin (probability of becoming a mother) and the intensive margin (number of children conditional

[9]This paper uses data from the National Educational Panel Study (NEPS): Starting Cohort Adults, doi:10.5157/NEPS:SC6:7.0.0. From 2008 to 2013, NEPS data was collected as part of the Framework Program for the Promotion of Empirical Educational Research funded by the German Federal Ministry of Education and Research (BMBF). As of 2014, NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi) at the University of Bamberg in cooperation with a nationwide network.

[10]For every individual we use only the most recent observation.

on being a mother). Table 2 gives the mean values of the dependent variables by college education. From the one-fifth of college-educated women about three-quarters have at least one child. For women without a college education, the share of mothers is about ten percentage points higher. Interestingly, once a woman decides to become a mother, the average number of children is almost the same for women with and without a college education (if anything, college-educated mothers have slightly more children). In other words, the main difference in the descriptives between college-educated and non-college-educated women is on the extensive rather than the intensive fertility margin.

Table 2: Descriptive statistics of dependent variables

	(1)	(2)	(3)	(4)
	College stats			
	all women	with college	w/o college	share w/ college
Motherhood				
all women (num. obs.)	4,188	921	3,267	22.0
mothers (num. obs.)	3,217	613	2,604	19.6
non-mothers (num. obs.)	805	239	566	29.7
share of mothers (in %)	80.0	72.0	82.2	
Number of children				
all women (incl. 0 kids)	1.64	1.51	1.67	
mothers (i.e., kids$\geq$1)	2.05	2.09	2.04	

Notes: Own calculations based on NEPS–Adult Starting Cohort data.

Instrument

The processes of the college expansion discussed in Section 3 provide, on the one hand, a powerful shift in the availability of higher education for many individuals. On the other hand, the multi-faceted college expansion that took place over several decades is hard to boil down into one or a few still powerful instruments.[11] This is especially the case as we observe college openings. In the initial years, for instance, colleges are often too small to affect individual college decisions. Therefore and in this case, using a scalar for the distance to the closest college (as suggested by Card, 1995) can even be misleading. Moreover, the inherently local nature of the IV results (see next subsection) makes it desirable to have an instrument that affects as many individuals as possible and therefore also captures, for instance, the expansion in the capacities of the already existing colleges.

[11] Westphal et al. (2019) use the same source of variation in an IV setting but assess the most powerful instruments of many potential indicators using machine learning techniques.

To achieve such a powerful instrument, we follow Kamhöfer et al. (2019) and create an index that weights the non-linear effect of the college distance with the relative number of students in the 325 West-German districts:

$$Z_{it} = \sum_{j}^{325} K(dist_{ij}) \times \left(\frac{\#students_{jt}}{\#inhabitants_{jt}} \right). \tag{1}$$

This college availability index Z_{it}, that is, the instrument, basically includes the total number of college spots (measured by the number of students) per inhabitant in district j (out of the 325 districts), individual i faces in year t weighted by the distance between i's home district and district j. Weighting the number of students by the population of the district takes into account that districts with the same number of inhabitants might have colleges of a different size. This local availability is then weighted by the Gaussian kernel distance $K(dist_j)$ between the centroid of the home district and the centroid of district j. The kernel gives a lot of weight to close colleges and a very small weight to distant ones. Since individuals can choose between many districts with colleges, we calculate the sum of all district-specific college availabilities within the kernel bandwidth. Using a bandwidth of 250km, this basically amounts to $K(dist_j) = \phi(dist_j/250)$ where ϕ is the standard normal pdf. While 250km sounds like a large bandwidth, this implies that colleges in the same district receive a weight of 0.4, while the weight for colleges that are 100km away is 0.37, which is reduced to 0.24 for 250km. Colleges that are 500km away only get a very low weight of 0.05. A smaller bandwidth of, say, 100km would mean that already colleges that are 250km away receive a weight of 0.02 which implies the assumption that individuals basically do not take them into account at all. When presenting the first-stage results in Section 5.1, we also discuss alternative specifications, see also Online Appendix B.

4.2 Empirical strategy

The effect of college education on fertility
We can think about the observed total number of biological children n by college status j ($= 1$ for college graduates, and 0 else) as

$$n^j = d^j \times n_L^j \qquad \forall j \in \{0, 1\}.$$

d indicates whether a woman is a mother ($d = 1$ for mother, and 0 otherwise). n_L gives the latent number of children, independent of whether the woman has children or not. The virtue of this notation is that it decomposes observed fertility into n_L^j, which is subject to economic as well as biological forces (that we try to explore later on), and a motherhood indicator, which censors desired fertility for non-mothers. As this latent fertility is based on preferences and trade-offs, it is particularly interesting to study. The aim of our analysis is to estimate the effect of college education T (that takes on the value 1 for college, and is 0 otherwise) on three fertility measures:

- completed fertility: the number of children at age 40. Formally, the college effect on this measure is $E(n^1 - n^0)$;
- the extensive fertility margin: the probability of becoming a mother, $E(d^1 - d^0)$; and
- the intensive fertility margin: the number of children among mothers, $E(n_L^1 - n_L^0)$.

Given an appropriate instrument (see in what follows), estimating the effect of college education on the first two measures is rather straightforward as realized values are directly observable. Yet, the latent number of children, n_L^j, is only observable for $d = 1$. For non-mothers, n_L^j remains a latent factor. We cannot identify $E(n_L^1 - n_L^0)$ but instead, we *can* estimate is $E(n^1|d^1 = 1) - E(n^0|d^0 = 1)$. That is, the estimated child differential among mothers. As this effect conditions on being a mother, any non-zero effect we find on the extensive margin may render the estimated effect on the intensive margin non-causal.

In other words, if college education causes some women to remain childless, the intensive margin suffers from a selection problem. However, employing the approach used in our companion paper (Westphal et al., 2019)[12], we are confident to at least identify the sign of $E(n_L^1 - n_L^0)$. The sincerity of the bias comes down to the question "How many children would childless women with college education have without college education if they would be mothers?" and the size of the extensive margin effect. While we do not know the exact answer to the question, by definition childless women with college education would need to have at least one child in order to become a mother in absence of college education. In Online Appendix C we use this piece of information together with the estimated effects for the extensive and the intensive margin to bound the true intensive margin effect.

In the next section, we present the estimates for the three fertility measures. Thereafter, we take a closer look at what drives the estimated fertility margins. In particular, we study economic and biological forces. To account for this, we can unfold the latent number of children by college status j into $n_L^j = n^{*j} + \varepsilon^j$, $j \in \{0, 1\}$. Here, n^{*j} reflects desired fertility (the economic driving force) and ε^j comprises unmet fertility desires, we dub "biological effects."[13] As our intensive margin – $E[(n^{*1} - n^{*0}) + (\varepsilon^1 - \varepsilon^0)]$ – is driven by both factors simultaneously, we hope to shed light on which factor prevails by unraveling the intensive margin effect along fertile ages. This may be informative, as we expect the biological component $(\varepsilon^1 - \varepsilon^0)$ to be correlated with age at birth.

[12]In the companion paper we derive bounds for the effect of college education on wages under college-induced selection into employment. The techniques suggested there can also be used to bound the intensive margin effect under college-induced selection into childlessness.

[13]These dimensions are sometimes distinguished through the terms fertility (the actual number of offspring) and fecundity (the physiological ability to bear offspring).

2SLS approach

The most natural starting point to assess the parameters defined above is an ordinary least square (OLS) estimation where we regress the fertility measure under review, Y_{irt}, for individual i who graduated from high school in district r and year t on the binary college indicator T_{irt} and a vector of control variables $\boldsymbol{X}'_{irt}$:

$$Y_{irt} = \beta_0 + \beta_1 T_{irt} + \boldsymbol{X}'_{irt}\boldsymbol{\beta}_2 + u_{irt}. \tag{2}$$

In order to separate the general trend in college education from the reverse trend in fertility (as depicted in Figure 1), the vector of confounders, $\boldsymbol{X}'_{irt}$, includes among the rich set of pre-college controls introduced earlier (and listed in Online Appendix A, Table A1) also district-specific linear trends in addition to general time and district fixed effects. The district-specific trends accommodate temporal confounding factors, for instance, because of global and district-specific trends in secondary school graduation (see, e.g., Online Appendix A, Figure A2 and Westphal, 2017).

As women can chose both, their level of education and – to some degree – how many children to rear, β_1 is likely biased. The direction of the bias is a priori unclear and depends on the effect of the omitted confounders on fertility and its correlation with education. A very general, but not observable confounder may be social attitudes about who shoulders the burden of child care and the relative status of the woman at home, as discussed in Feyrer et al. (2008). If social attitudes suggest that mainly women take care of children, an increase in education – potentially triggered by better labor market opportunities and higher monetary returns to education – would discourage women to get children and encourage labor market attachment. Omitting social attitudes would then cause OLS regression to overestimate the true effect. However, if social attitudes change over time toward a stronger female empowerment (Goldin, 2006) and a more equal shouldering of child care (Feyrer et al., 2008), this would favor both education and fertility for younger cohorts and, hence, cause OLS to underestimate the true effect.[14] Other omitted factors may be women's career preferences (that are likely to upward bias OLS) or the family's wealth and emphasis on (their daughters) education beyond what is captured through the controls (that would result in a downward bias).

In order to address the selection of individuals in education and fertility along unobserved preferences, we exploit the variation in college access using the index of college

[14]Although Feyrer et al. (2008) assume technological-change-induced workforce opportunities for women to be independent of fertility trends (and, thereby, not necessarily an omitted confounder in our setting), it is easy to think of changes in labor market opportunities that are endogenous with regards to fertility. While the authors also see Germany as whole better described through the situation of unequal shouldering of child care, regional differences in (the trends toward) female empowerment make a downward-biased OLS estimate not implausible.

availability we define in Eq. 1 as an instrumental variable in a two-stage least-squares (2SLS) approach. The first stage of the 2SLS approach reads:

$$T_{irt} = \delta_0 + \delta_1 Z_{rt} + \boldsymbol{X}'_{irt}\boldsymbol{\delta}_2 + v_{irt}. \tag{3}$$

We then receive the second stage of the 2SLS approach by plugging the first-stage fitted value in for the college decision $\widehat{T}_{irt}$ in our regression of interest:

$$Y_{irt} = \beta_0 + \beta_1 \widehat{T}_{irt} + \boldsymbol{X}'_{irt}\boldsymbol{\beta}_2 + u_{irt}. \tag{4}$$

Employing this 2SLS approach using d_{irt} or n_{irt} (unconditional or conditional on mothers) as outcomes yield estimates of β_1. They are causal parameters of college education, if independence and exclusion are fulfilled. Imposing a restriction that the college expansion monotonously pushed individuals toward more college (the monotonicity restriction), β_1 is a causal effect for a specific group of women: those who would potentially go to college because of the instrument (called compliers). Because this group is typically a subset of all individuals, β_1 captures the local average treatment effect (LATE, see Imbens and Angrist, 1994) for all three outcome variables under review. In our example, the compliers are most likely those who could go to college because either a college opened up in their proximity or because existing colleges in the neighboring districts expanded. As this process potentially affected many people, one would expect the share of compliers to be rather large – a claim we underline empirically in Online Appendix B.

Independence says that conditional on $\boldsymbol{X}'_{irt}$, variation in our college accessibility measure (Z_{rt}) randomizes the otherwise endogenous decision to go to college. That is, variation in Z_{rt} does no depend either on the error term, v_i, or on general preferences about or other unobserved characteristics with respect to fertility. The balancing test in Table 1 already suggests that this seems to be a fair assumption in the setting at hand. To boost the plausibility of this assumption further, we condition on district fixed effects to effectively use only the openings of new colleges and within-district increases in college seats. Moreover, Online Appendix B reports additional results of different instrument specifications, including some that only exploit college distance and do not consider the college size. Finally, the exclusion restriction requires that any instrument-specific shift in T only affects (some of) our employed fertility measures via college graduation (i.e., the exclusion restriction).

We can think of three coinciding factors that could potentially pose a threat to one or the other identifying assumption: the availability of modern contraceptives, university hospitals, and child care availability. If women in regions with a stronger increase in college availability also had better access to modern contraceptives like the combined oral contraceptive pill – that was introduced in Germany at the beginning of the 1960s – we may falsely attribute the contraceptive effect to education. To alleviate this concern, we include district-specific trends. Moreover, Table 1 suggests that, at the aggregated level, the fertility measures are uncorrelated with the opening of a college. What is more likely is that college-educated women were more willing to use contraceptives in order to regulate fertility (see Oddens et al., 1993), which would be a channel of the effect rather than a violation of the identifying assumptions. If university hospitals opened up together with colleges and increase fertility through better maternity wards, this would threaten the exclusion restriction. The same would be true if the development of child care opportunities coincides with college openings beyond what is captured by the district-specific linear trends. However, we are not overly concerned that these factors violate the exclusion restriction. Many of the hospitals that today belong to a university, which opened within the educational expansion period (for instance, Bochum, Düsseldorf, Essen, and Ulm) existed already long before the college opening. While an increasing body of literature analyzes the child care expansion in Germany, the time span usually considered starts well-beyond the bulk of college openings and is, therefore, unlikely to confound college openings beyond what is captured through the included time trends.[15]

5 Baseline results

5.1 The effect of the college expansion on educational participation

Before looking at the college effects on fertility, we take a broader view at the first-stage relationship by analyzing the effect of the first four post-war era college openings on the probability of taking up college education in Micro Census data.[16] This is a rather broad view in that the first stage in the main analysis is based on survey data that include fertility information and also considers the size of the colleges (see Section 4.1). Still, the larger sample size in the Micro Census allows us to conduct an event study to estimate the relative change in the share of students within a 100km radius relative to the timing of the opening of these colleges (time of opening centered to 0).

The results are depicted in Figure 3 which shows a twofold takeaway. First, there is no evidence on pre-trends, indicating that the colleges were not opened in regions where already existing colleges were expanding relatively more than the colleges in regions without an opening. Second, the figure reveals a relatively sharp discontinuity: after a college

[15] Cornelissen et al. (2018) study, for example, the expansion in child care access between 1990–2003, Felfe and Lalive (2018) consider school entry cohorts 2009–2014, and Kühnle and Oberfichtner (2019) look at birth cohorts 1994–1996. Moreover, university-run child care centers are capacity-wise of minor importance. So is the supply of preschool teacher, as they do not need to have a college degree in Germany.

[16] We use the first available years of the Micro Census, 1962–1969, in which fall the opening of colleges in the cities of Bochum, Dortmund, Konstanz, and Regensburg.

was opened in $t = 0$, there was a rather large and significant increase in the relative share of students in the region even two years after the opening. Given that the colleges had just opened, this is a remarkable effect. This plot considers all students in regions within a 100km radius, thereby the increase in the number of students not only captures the somewhat mechanical effect in the region of the opening itself but it also suggests that individuals from neighboring regions were also affected by the opening, for instance, because the newly built college was within commuting distance. We take this as evidence that there was an excess demand of secondary school graduates who wanted to go to college.

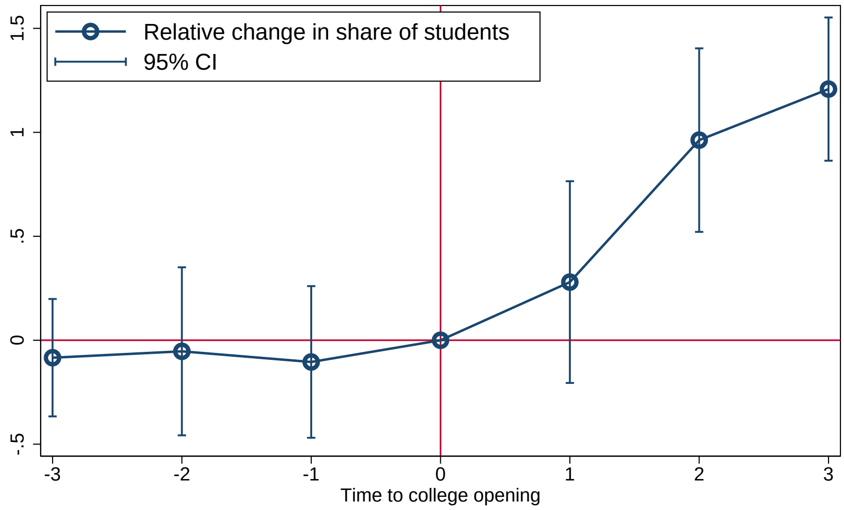


Figure 3: Relative change in the share of students in counties within 100km of college opening between 1962 to 1969

Notes: Own representation based German Micro Census data from 1962-1969 (see Lengerer et al., 2008) and German Statistical Yearbooks (see German Federal Statistical Office, various issues, 1959–1991). The figure depicts the coefficients β_τ from the following "event-study" regression where β_0 is set to zero:

$$\begin{aligned} \ln(\#\text{students}_{bt}) &= \alpha_t + \sum_{\tau \in \{-7,-1\}} \beta_\tau \mathbb{1}\left[\max(t - t_b^{\text{opening}}, -3) = \max(\tau, -3)\right] \\ &\quad + \sum_{\tau \in \{1,7\}} \beta_\tau \mathbb{1}\left[\min(t - t_b^{\text{opening}}, 3) = \min(\tau, 3)\right] + \gamma_b + \epsilon_{bt}, \end{aligned}$$

where $\ln(\#\text{students}_{bt})$ is the log number of students in region b and year t (1962-1969). α_t are year fixed effects. t_b^{opening} equals the the year in which a college opened in region b. To control for differences in levels between these regions, region fixed effects γ_b are included. Regions include all regions within a 100km radius surrounding the centroid of the region where the new colleges are located. The reason for the choice of this radius is that we want to go beyond a somewhat mechanical effect which emerges by the influx of students in the region of the opening. A sufficiently large radius partials out this effect for two reasons. First, it captures the bulk of the catchment area of a college and therefore only a minority of students do not come from the area defined by the radius. Second, within each region that exhibited an opening of a college (Bochum, Dortmund, Konstanz, Regensburg) there are already well-established existing colleges (Münster, Cologne, Freiburg or Nuremberg). Hence, there had been possibilities to enroll into a college in the defined area also in the absence of a college opening in period 0.

Online Appendix B gives the first-stage results for the survey data in Table B1, discusses alternative instrument specification, and interprets the results. Table B2 presents separate first-stage estimations for subsamples of the overall population (along father's education, year of birth, and urbanization of the home district) in order to investigate who complies with instrument changes. We interpret the results of the subgroup analysis as evidence that the complying population, although modestly selected, is not confined to any specific population.

5.2 The effect of college education on fertility

Starting with overall completed fertility, shown in panel A in column 1 of Table 3, the OLS association between college education and the number of children is -0.1. In other words, given controls, female college graduates have, on average, 0.1 fewer children than women without a college education. Addressing selection that goes beyond the observable factors, the 2SLS estimate in panel B yields a reduction in the average number of children of -0.3. Given an average number of 1.6 children in Table 2, this corresponds to a reduction of 18 percent – a rather sizeable effect. With 4.5 years of college education, the per-year reduction that goes along with college education is, on average, 0.02 children in the OLS model and 0.06 children in the 2SLS specification.

Table 3: Baseline regression results

	(1)	(2)	(3)
	Total Effect	Fertility margins	
	# of children for all women	Extensive: motherhood indicator	Intensive: # of children for mothers
Panel A: OLS regression			
College degree	-0.086^{*}	-0.075^{***}	0.129^{***}
	(0.052)	(0.020)	(0.051)
Panel B: Second-stage 2SLS regression			
College degree	-0.292^{***}	-0.204^{***}	0.268^{***}
	(0.148)	(0.053)	(0.131)
Number of observations	4,188	4,188	3,217

Notes: Own calculations based on NEPS–Adult Starting Cohort data. Control variables include full sets of year of birth and district fixed effects as well as state-specific trends. For the full list of control variables, see Online Appendix A, Table A1. District-level clustered standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Taking a closer look at the composition of the overall effect, we take the fertility margins as dependent variables. The OLS point estimate of college education on the extensive margin (that is, motherhood) is -0.08 (-0.02 per year of college). Put differently, women who went to college are 8 pp less likely to ever bear a child, given the controls. Addressing endogeneity, the 2SLS estimate in panel B yields a reduction in the probability of becoming a mother through college education of about 20 pp (4.5 pp per year). Again, the effect is precisely estimated and is large in size (the baseline probability is 82.2 percent for females without college).

Turning to the intensive margin in column 3 of Table 3, we see that the negative effect from the extensive margin does not propagate here. The differential in the number of children is slightly positive when it is controlled for observables. Going to the structural estimate, college-educated mothers have, on average, 0.268 children more than their peers without college education. This estimate confirms the intriguing pattern already found in the descriptive statistics: there are opposing effects of college education on both margins. College pushes some mothers into childlessness, but those who decide for children have more. Given that mothers have an average of 2.1 children, the relative effect amounts to a 12.8 percent increase in the number of children of college-educated mothers. While only statistically significant at the 10 percent level, the effect size is substantial. Although this result for the intensive margin may be taken with a grain of salt as it refers to the selected sample of women who decide to have children (and these women only become mothers in one potential college state), we are confident that it prevails also without the extensive-margin-induced selection effect. This is because in Online Appendix C, we apply a bounding approach developed in Westphal et al. (2019) to see that the positive effect on the intensive margin would only dissipate in extreme scenarios for the number of children of non-college-only mothers.

Before building the bridge to potential mechanisms that may contribute to explaining the results, the rather new margin of education considered here calls for a careful comparison of our findings with the literature on the secondary schooling effects on fertility. For Germany, the OLS estimate for the effect of an additional year of secondary schooling on the average number of children provided by Cygan-Rehm and Maeder (2013) is -0.020 – this is remarkable close to our per-year OLS estimate of -0.019 (=0.086/4.5 years). Instrumenting secondary education with compulsory years of schooling, Cygan-Rehm and Maeder (2013) find an effect ranging from -0.10 to -0.17 depending on the specification. This is more than twice as big as our pre-year effect of college education. The bigger effect may seem contradictory at first sight, given that college education is probably more relevant for later career opportunities and affects individuals in their prime reproductive ages. However, while interpreting the effect size, one has to keep in mind that the compulsory schooling reform affects individuals at the lower end of the educational distribution and – given the baby gap in education – the average number of children is higher at this margin. Accordingly, the 2SLS effect on childlessness by Cygan-Rehm and Maeder (2013), about 5 pp (compared to a baseline probability of 18 percent) exceeds our effect of college education on motherhood by about 5.7 percent (that is, (-0.204/0.800)/4.5 years=-0.057). Fort et al. (2016) find similarly large effects of compulsory schooling on the number of children and childlessness for England and pooled Continental European countries.

Moreover, our results confirm another interesting pattern found by several studies on the secondary schooling effect (e.g., Cygan-Rehm and Maeder, 2013, Fort et al., 2016 and Monstad et al., 2008): the OLS results underestimate the 2SLS effects in absolute terms. This indicates that the bias in the OLS results stems from omitted variables such as unaccounted trends in female empowerment (as documented by Goldin, 2006), family income and openness to new experiences rather than from pre-college career-only preferences or preferences for a traditional family (where more children are preferred to a mother's college education). Another explanation as to why OLS underestimates the 2SLS result might be that OLS captures the average treatment effect while the 2SLS model yields the LATE for the complying subpopulation. However, as discussed earlier and indicated in Online Appendix B, the college expansion was not limited to particular groups of individuals. Thus, the local nature of the 2SLS estimates seems rather unlikely to drive the pattern of the results presented here.

6 Potential mechanisms

To learn more about what drives the opposing signs at the extensive and intensive margins, we first take a closer look at the decision to become a mother using an augmented version of the Becker and Lewis (1973) quantity–quality (QQ) model. After this deterministic view, we consider how the college-induced shift in the age of leaving full-time education affects stochastic fertility problems, see Section 4.2. Understanding these mechanisms is crucial for zeroing in policy interventions that have the potential to improve the comparability of a career and family life.

6.1 Economic forces: labor market opportunity costs and lock-in effects

In the augmented QQ model women deciding on desired fertility (and also on child inputs and individual consumption) face the following maximization problem:

$$\max U\left(n^j, e^j, c^j\right) \qquad \text{s.t.} \quad y^j = n^j\left(\tau^{qj} + \tau^{ej} f(e^j)\right) + c^j. \tag{5}$$

As in the original QQ model, women derive utility from the number children (n), their human capital ($f(e)$ with e being parental inputs), and other consumption (c). The budget constraint is set by the household income (y) and the associated factor prices (given relative to consumption). Following Galor (2012) and Aaronson et al. (2014), we differentiate the costs of investing in the offspring's human capital (τ^e) from a per-child lump-sum for rearing the offspring (τ^q). As we are interested in the effect of the women's education, we differentiate this maximization problem by the potential college decision of the mother. More specifically, we allow each parameter to differ between college graduates and non-graduates. This is indicated by the superscript j.

Although the QQ framework does not model the two fertility margins explicitly, differentiating human capital costs from rearing costs allows disentangling the margins:

1. **Intensive margin.** Solving problem (5) gives the potential outcomes for the desired number of children: n^{*1} for college-educated mothers and n^{*0} for non-graduates. The desired number of children can be latent and whether it can be observed depends on the extensive margin.
2. **Extensive margin.** Following Aaronson et al. (2014), all potential mothers assess their value of becoming a mother, $V^j\left(\tau^{qj}, \tau^{ej}, y^j\right)$, and compare it to the value without any children, $V_0^j\left(y^j\right)$. The latter may reflect the value that comes with focusing on a career. Thus, all women implicitly make the following decision on the extensive margin:

$$d^j = \mathbb{1}\left\{V^j\left(\tau^{qj}, \tau^{ej}, y^j\right) > V_0^j\left(y^j\right)\right\}. \tag{6}$$

We believe that the relative effects of college education on the exogenous model parameters y^1/y^0, τ^{e1}/τ^{e0}, and τ^{q1}/τ^{q0} are the key to understanding our results. While preferences may be affected as well, we focus on price effects as the impact of college education on them is almost less ambiguous.[17] Given the augmented QQ model, we can distinguish college differentials in the costs parameters and how they may contribute the margins of fertility:

(i) A college gradient in rearing costs (τ^{q1}/τ^{q0}): college-induced higher earnings increase women's opportunity costs of rearing children.

(ii) A college gradient in household income (y^1/y^0): given the women's own college-induced income increase, the household income is determined by the partner's income – which might be higher as well due to assortative mating.

(iii) A college gradient in costs for parental inputs in children's education (τ^{e1}/τ^{e0}): college graduates face lower costs of investments in offspring's human capital.

These three channels (taken from Aaronson et al., 2014) do, when taken for themselves, not explain the positive intensive margin in combination with the negative extensive margin effect. Online Appendix D discusses the expected effects of these channels on the fertility margins based on previous studies and provides, if possible, empirical evidence using our data. To reconcile our finding we propose a new, additional channel that *can* explain the negative extensive and positive intensive margin effect:

[17]Focusing on price effects implicitly assumes that coinciding effects on preferences, which could go in either direction, may confound our effects in less systematic way.

(iv) College-induced lock-in effects in career-oriented and family-friendly occupations.

The idea is that within the range of jobs that college graduates usually desire and relative to jobs for non-college graduates, there is a considerable heterogeneity with regard to the compatibility between work and family life. For college graduates there are jobs that are a relatively incompatible with family life, but there are also jobs that are more compatible than those for non-college graduates. Once a college graduate decides to have any children, lock-in effects in those jobs reduce the costs for additional children. Take, for instance, the decision of a college graduate to become a teacher or higher civil-servant, instead of a consultant. Both kinds of job require college education, but once a woman has decided to become a teacher, her career consequences for a second child are much lower. On the other hand, jobs usually filled (at least in Germany) with non-college graduates, such as administrative assistants and salespersons, are less strictly separated in career-oriented and family-friendly. Now, among mothers (on the intensive margin), family-oriented jobs are more frequent for college graduates than for non-college graduates, which may render children more attractive for the former.

In the augmented QQ model such a career lock-in effect would be reflected by a college gradient in the *marginal rearing costs* of additional children, as they capture labor market opportunity costs of children. τ^q can then be written as $\tau^q(n)$. Although this is not necessary, we deem it likely that there are some "economies of scale" in the career consequences of additional children even in jobs that do not require a college education. That is, the marginal costs (and thus also the average costs) of an additional child decrease: $\frac{\partial \tau^{q0}(n)}{\partial n} < 0$. However, the stronger polarization into career-oriented and family-friendly jobs for college graduates causes the decline in the marginal costs to be bigger for college graduates: $\frac{\partial \tau^{q1}(n)}{\partial n} < \frac{\partial \tau^{q0}(n)}{\partial n}$. This does not rule out that the rearing costs for the first child are higher for college-educated women, for instance, because they forgo a higher income, $\tau^{q1}(1) > \tau^{q0}(1)$ (as suggested by the first channel). However, once a college-educated woman has decided to become a mother, the career consequences of the first child become sunk costs and the decision to have a second child is solely based on the marginal costs. This argumentation is in line with Adda et al. (2017) who suggest that some costs of children occur well before children are born, e.g., through choice of job.

In Online Appendix E we consider information on working overtime, public sector employment, and working as supervisor as proxies of the family-friendliness of jobs held by the women in our sample.[18] In the remainder of this subsection, we use the forgone income through childbirth as a sufficient statistic to measure the college differential in the

[18]Regressing public sector employment on instrumented college education separately for mothers and non-mothers, we find that college-educated mothers are (about 18 pp) more likely to work in the public sector than non-college mothers, see Table E1. The corresponding estimate for non-mothers is zero. Descriptively comparing the distribution of extra hours, college graduates have more often close to zero and above 20 hours per week, while non-graduates are more likely to work 5 to 15 extra hours.

average jobs' compatibility with family life. Figure 4 plots the child–income penalty using an event-study approach similar to, e.g., Kleven et al. (2019), but separately for women with and without college education. We assess income penalties for the second child indirectly by estimating the income penalties nine years after the birth of the first child for mothers who will at least have two children. This approach has two advantages. First, the first child likely affects the pre-birth income of the second child, thus the pre-birth income of the first child is both more relevant and valid. Second, we expect lock-in effects to be realized after the first birth. Each marker in Figure 4 states the relative income loss compared to the year before the first child was born (significant differences between college and non-college mothers are marked red). Because the panel structure of the NEPS data is not sufficiently long, we make use of the German Socio-economic Panel Study (Goebel et al., 2019).[19] Formally, we regress yearly income on indicators to the distance of birth and control variables for the calendar year and the mothers' age, see the figure note for details.

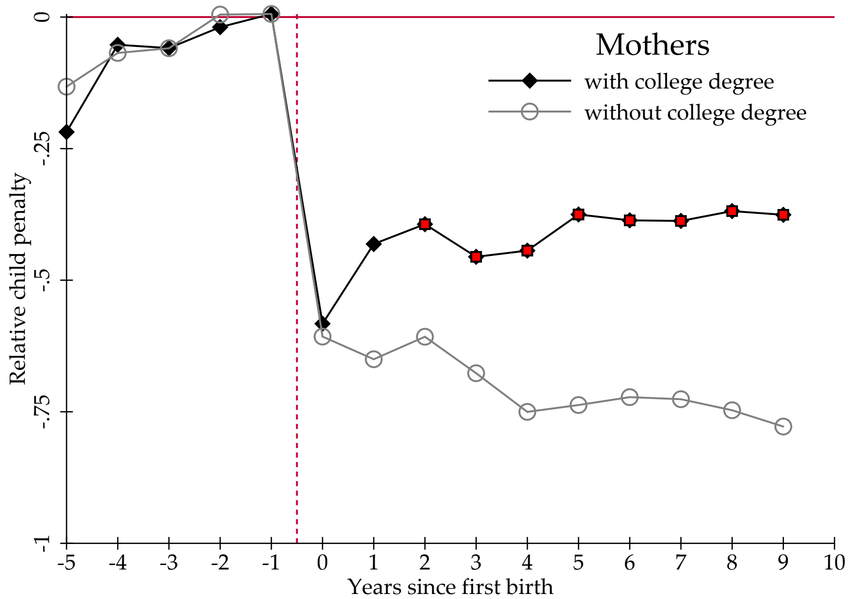


Figure 4: Suggestive evidence for decreasing average costs of child quantity

Notes: Own calculations using data from the SOEP, waves 1984–2016. Lines depict results from one nested event-study regression (using age and month fixed effects) for mothers with at least two children by college degree. The dependent variable is the log net wage as it includes also transfers such as paid parental leave. The event time is time relative to the first birth of the mother. This graph thus shows a high and even declining child penalty that non-college educated mothers incur. While initially, they have an income penalty of approximately 60 percent, it continues to decrease steadily to reach 80 percent nine years after birth. This decrease might be driven by subsequent childbirths after $t = 0$. For college-educated mothers, however, there is no decrease detectable. While being on the same relative level in the year of the childbirth, the gap to the non-college educated mothers rises continuously. Although the college-educated mothers also have a second birth (and probably even more births than non-college educated mothers in between, because the spacing of their births are more compressed), in the years following $t = 0$, their penalty decreases in relative terms w.r.t. non-college educated mothers. This may be because these mothers are more attached to the labor market (which mechanically implies a lower τ^q).

Even so there is a small upward trend in the five years prior to birth, the relative income compared to the baseline year (one year prior to first birth) does not differ by college ed-

[19]Socio-Economic Panel (SOEP), data for years 1984–2017, version 34, SOEP, 2019, doi: 10.5684/soep.v34.

ucation – even though the absolute income is higher for college-educated women. In the year the first child is born, the income for both mothers with and without college education drops by about 60 percent relative to the year before, reflecting the reduced labor market participation through maternity leave. Interestingly, only two years after birth, the relative income loss (that is, the child–income penalty) for college-graduated mothers is significantly lower than for their non-college peers. This trend continuous throughout the entire timespan we consider. Nine years after giving birth to the first child, mothers without college education earn about 75 percent less compared to their pre-birth income. For college-educated mothers the corresponding number is about 40 percent. These magnitudes are in line with Kleven et al. (2019) who find an average wage penalty of 61 percent five to ten years after childbirth, undifferentiated by college education. As we condition the sample on mothers with at least two children, the estimated wage penalties in the later years are likely to be driven by higher-order births. The college gradient in the wage penalty suggests that at least among mothers of two, college graduates face relatively lower opportunity costs at the intensive margin.

Although only of suggestive nature, we interpret the explorative analysis as evidence supporting the our hypothesized fourth channel. The income penalty and, thus, opportunity costs of additional children are relatively lower for college-educated mothers than for their peers without college education. In other words, the fourth mechanism *can* explain why college education causes some women to remain childless while college-educated mothers have more children than their non-college peers. Even though a lock-in effect is certainly not the only explanation, it is a plausible one.[20] Assuming the QQ model is characterized by log-linear preferences (for example employed in Galor, 2012) or CES preferences (used among others by Mogstad and Wiswall, 2016), Online Appendix F demonstrates analytically that the signs of the fertility margins predicted by the fourth mechanism can dominate the other three mechanisms.

6.2 Biological forces: effect heterogeneity along age

Unfolding the college effect by age

Information on the children's years of birth allows us to unfold a possible heterogeneity of college education along mothers' age. It is fair to expect that attending college shifts the age-at-birth distribution to the right – whether this shift exceeds, is equal to,

[20]Other reasons for economies of scale that favor higher-order births for college graduates besides a labor market lock-in effect include college-induced efficiency gains in rearing children (as college education increases cognitive skills, see Kamhöfer et al., 2019, which may result in better information processing). Moreover, many child care centers in Germany make the admission for children easier (e.g., by allowing to bypass a waiting list) if the child already has a sibling in the center. Together with a higher willingness among more educated mothers use child care (as suggested by Cornelissen et al., 2018), this, too, could lower the rearing costs of second children for college graduates.

or smaller than the time spend in college is however an empirical question. Analysing this age-at-birth pattern is not only an interesting exercises in is own right but potentially informative in two additional ways: First, a high, but sharply decreasing fertility rate of college graduates in their mid-30s to end-30s may hint towards age-related fertility problems (that is, infecundity). This is the case if the age-at-birth distribution is shifted thus far to the right that some women are no longer in a fertile age. Such an unintended reduction in fertility might add an important policy dimension as educational reforms aiming at compressing the time to graduation would then be more promising than monetary incentives targeting at reducing the labor market opportunity costs of children.[21] Second, the timing of birth may contributes to understanding college-induced fertility changes through elucidating the child–income penalty. Birth-related labor market disadvantages may carry more weight at some ages (and, thereby, career stages) than at others.

To detect this kind of heterogeneity, we estimate our baseline models for the extensive and the intensive fertility margins fully saturated by women's age to get age-specific effects. To this end, we reshape the data from individual level i to individual–age level ig, where g now indicates the age of the woman for each year from 20 (when college is started) to 40 (when the infertile ages are near). The second stage of the 2SLS model is then:[22]

$$d_{ig} \quad = \quad \alpha + \sum_{g=20}^{40} \eta_g \mathbb{1}(\text{age}_{ig} = g) + \sum_{g=20}^{40} \left[\gamma_g \mathbb{1}(\text{age}_{ig} = g) \times \widehat{T}_i \right] + \boldsymbol{X}_i' \boldsymbol{\beta} + u_{ig}. \qquad (7)$$

The indicator functions $\mathbb{1}(\cdot)$ return the value 1 if the observation refers to individual i at age g, and 0 for other fertile ages but g. In other words, the first sum gives a full set of age fixed effects and the second sum interacts the age fixed effects with the college indicator.[23] The interpretation of the dependent variable d_{ig} and, thereby, the interpretation of the coefficients of interest differs depending on whether fertility is measured at the extensive or the intensive margin:

- At the extensive margin, d_{ig} is a binary indicator that takes on the value 1 if woman i becomes a mother at age g (and 0 otherwise), given that she does not have a child until age $g-1$. The age fixed effects η_g give the baseline hazard rate of having the first child (given that one does not already have a child) at age g. The coefficients of interest γ_g give the effect of college education on the baseline hazard. That is, they

[21] An example of a policy compressing the time to graduation is the so-called G8 reform in Germany. This reform redistributed the constant total amount of instruction to earlier grades, reducing the time to graduation from 13 to 12 years in total, see Marcus and Zambre (2018) and the references therein. An example from higher education policy is the Bologna Process.

[22] For the sake if simplicity, the subscripts for the time and the district are now implicit. The standard errors are clustered on an individual level as shocks are likely to be time persistent.

[23] $\widehat{T}_i$ stems from Eq. 3. Although $\widehat{T}_i$ enters Eq. 7 a total of 21 times, this does not constitute a forbidden regression problem because the age indicators ensure that only one of the $\widehat{T}_i$ is "switched on" at a time.

answer the question "How does college education affect the probability of bearing the first offspring at age g, conditional on having never given birth before?"

- At the intensive margin, d_{ig} is 1 if woman i gives birth at age g (and 0 otherwise) – independent of whether woman i already has a child or not. Accordingly, η_g is the baseline rate of having any child at age g given the woman is going to have a child by the age of 40 (as the sample for the intensive margin only consists of women who become mothers). The coefficients γ_g answer the question "How does college education affect the probability of giving birth at age g for women who have at least one child by the age of 40?"

In- and post-college effects on fertility

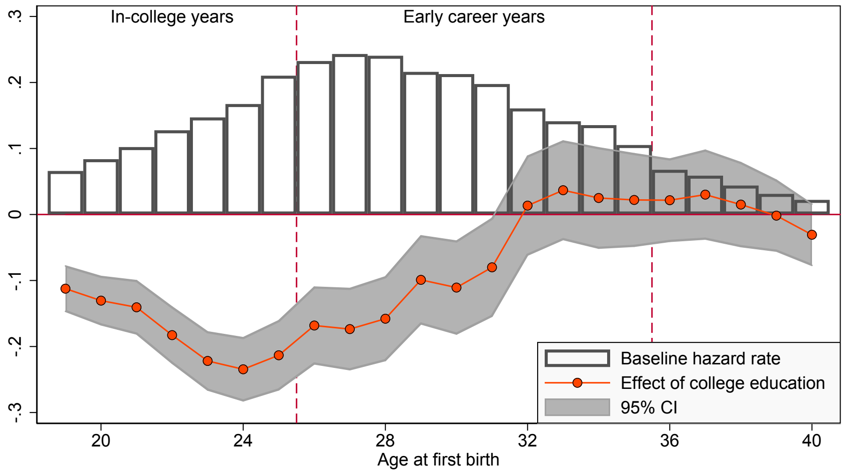
Figure 5 shows the estimation results of Eq. 7 for the extensive margin of fertility in panel (a) and intensive margin in panel (b).[24] The bars state the baseline hazard rate of becoming a mother and the baseline probability of giving birth at a certain age in panel (a) and (b), respectively.[25] The oranges lines give the effect of college education on these baseline probabilities. For the sake of interpretation, we may think of the fertile ages as three phases for which we expect distinct effects: years in college (ages 19–25), the career starting years for college graduates (ages 25–34), and ages with increased risk of infertility (ages 35 plus).

In the first phase, soon-to-be college graduates are much less likely to have a child: in both panels the orange IV estimates are well below the baseline rates. Interestingly, the negative effect of college education on fertility increases in the early-20s at about the same rate as women who do not go to college become more likely to have a child. That is, while non-graduates become more likely to start a family (after finishing their vocational education and gain in financial security), the women in college are very unlikely to do so throughout their early-20s. This dip in fertility of college graduates during the in-college years corresponds to the so-called "incarceration effect" (Black et al., 2008, p.1044), describing the lower fertility rates of adolescents while in school.

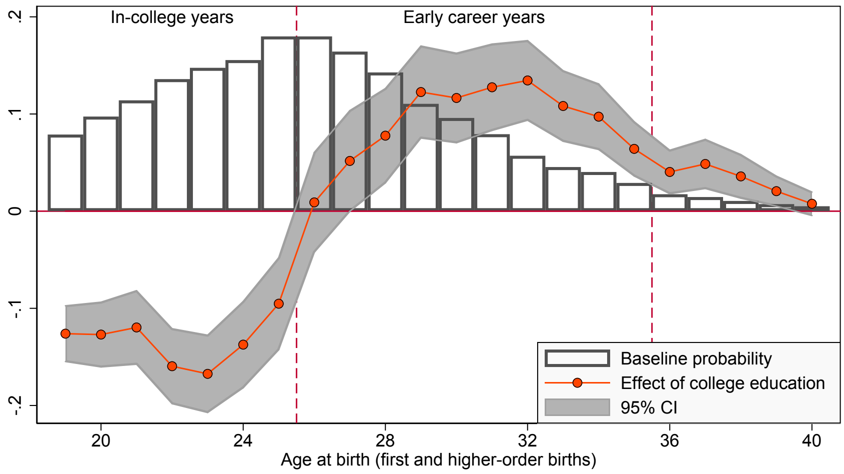
The second phase starts when college graduates leave college in their mid-20s. Here, the college effect differs across both margins: at the extensive margin college graduates are still less likely to become a mother compared to their non-college peers. In contrast, college graduates who will become a mother (intensive margin) exhibit a steep catch-up

[24]As the age-specific estimates in panel (a) after age 20 refer to the hazard of giving birth to the first child conditioning on not yet being a mother, the estimates may not be taken for the unconditional causal effect of becoming a mother at a certain age. Similarly, the estimates in panel (b) may not state the causal effects if the number and timing of children depends of the effect of college education on motherhood.

[25]Note, the baseline rates plotted in Figure 5 state the unconditional means. On the contrary, η_g in Eq. 7 are the conditional means after adjusting for college education and controls for non-college-educated women. We interpret the effect size (depicted by the orange line) relative to the unconditional mean as conventional for linear probability models.



(a) Extensive margin: effects on hazard rates of becoming mother



(b) Intensive margin: effects of bearing offspring for mothers

Figure 5: Timing of births

Notes: Both panels depict the age-specific regression coefficients from the second stage of the 2SLS model in Eq. 7 that capture the effect of college education. Panel 5a reports the effects of college education on the hazard rate of becoming a mother by age. Panel 5b depicts the respective effects on the probability of giving birth conditional on being a mother.

effect in the probability of giving birth in their mid-20s. This divergence is not only in line with the overall effects at both fertility margins, but these ages can also be assumed to be rather career-sensitive.[26] If not before, many women can be presumed to decide whether or not (and when) they want to have a child around the time of leaving college. Women who decide to have least one child may then opt for a family-friendly (but potentially in the long-run less-paying) job and start a family – a decision that eventually might favor a second child. On the other hand, some decide against a child – presumably in favor of a career – and may never have children. Lock-in effect that are realized early on in

[26]The existence of such an early-career effect can also be seen when regressing mothers' age at first birth on instrumented college education, similar to the baseline model in Table 3. As shown in column 1 of Online Appendix A, Table A3 college education increases the age of first birth by, on average, 6.5 years – exceeding the formal duration of studying by about 2 years.

the career (perhaps not only thought a lower staring salary, but also through a less steep wage trajectory) can explain both the substantial child–income penalty and the relatively lower opportunity costs for second children of college-educated mothers.[27]

The third and final phase of the fertile ages ranges from the mid-30s to end-30s. At the extensive margin, there is no significant difference in the hazard rate that college graduates and non-graduates to become mothers. This finding is more remarkable as it perhaps strikes at first glance: although involuntary fertility problems do not yet play a big role in the mid-30s, college graduates who are still childless by this time do not seem to want to catch up. In contrast, a sharply declining fertility rate toward the end of the 30s would suggest that childless college graduates do want to become a mother but that some are too old by the time they intend to start a family. We interpret this as evidence that some college graduates deliberately opt against children, presumably in favor of a career. At the intensive margin there is a slightly positive college effect in the early-30s but the effect size drops before infertility becomes a likely obstacle to additional children.

All in all, the pattern in panel (a) suggests that the lower extensive margin fertility of college-educated women in the baseline estimations seems to stem from a lower in-college fertility in combination with an early-career effect that prevents a catch-up at ages end-20s to early-30s. Moreover, the zero college effects in the mid-30s indicate that the college-induced shift of the age-at-birth distribution to the right does not push some women into involuntary childlessness. At the intensive margin in panel (b), the post-college probability of giving birth is rather pyramid-shaped. While there is a catch-up effect peaking around age 30, a fading out at second half of the 30s indicates that, here too, biological restrictions do not affect this margin.

7 Conclusion

In this paper, we analyze the nexus between education and fertility – two fundamental decisions in life that, when considered on an aggregated level, have greatly changed societies within the past 60 years. These dynamics are unlikely to be confined to the past – particularly with regard to recent policies such as the Higher Education Pact 2020 in

[27]Besides the timing of birth, the spacing of birth, i.e., the temporal distance between the first and the second child, could as well contribute to explaining the different child–income penalties. Column 2 of Table A3 indicates that college education cuts the time of birth between the first and the second child by half (the IV estimate is 1.7 years with an unconditional mean of 3.5 years). Similar to the timing plot in Figure 5, Figure A4 gives the effect of college education on spacing-in-years indicators. College education significantly increases the probability that the first and the second birth are 2 years apart and significantly decreases the probability that the spacing is 6 or more years. The costs of readjusting to work life or the time it takes to catch up on the most recent developments in the company or industry may be lower when returning form one longer leave of absence that comprises two births than for two shorter single-birth leaves.

which the German states committed to further increase access to higher education. This emphasizes the need to understand the long-term consequences of higher education that go beyond the monetary effects (Oreopoulos and Salvanes, 2011). Fertility is an especially interesting aspect in this context as higher education affects women – unlike previously studied secondary schooling – within their prime reproductive age. To analyze how education impacts individual fertility decisions in the in-college years and afterwards we make use of arguable exogenous variation in the accessibility of college education in Germany. Overall, we find that college education reduces the average number of children by 0.29. In line with previous evidence (Aaronson et al., 2014), we find that the overall quantitative fertility effects are driven by the extensive margin: the probability of becoming a mother is reduced by one-quarter. In contrast, women who decide to be a mother despite a college education, have, on average, more children.

We shed light upon the sources of these effects by addressing potential economic forces on the preferred family size and biological factors that may hinder to reach this size. We single out a novel, potentially powerful mechanism that is able to explain our margin-specific effects: labor market lock-in effects that reduce the marginal costs of additional children once college-educated women have decided to become mothers. The polarization of jobs usually taken by college graduates into rather family-friendly and more career-oriented suggests that having a career and a family is often not compatible. Although our analysis is constraint of women affected by a college expansion that took place between the 1960s and '80s, the persistence of the "baby gap" between women with and without college education indicates that the family–career trade-off is still not resolved today. As another potential explanation we consider involuntary childlessness or smaller families through age-related fertility problem. While a college-induced shift in the age-at-birth distribution towards older ages is clearly visible, fertility rates at both margins fell well before one would expect such biological considerations to matter. From a policy perspective, this is a noteworthy finding as a biological effect would restrict a woman's choice set when she maximizes her utility.

Although we find evidence that the massive college expansion and effect of college education on the probability of becoming a mother at least partly fueled the demographic transition in recent decades, the positive effect of college education on the number of children for mothers indicates that education does not per se have to decrease fertility. On the contrary, the finding that college-induced labor market opportunity costs seem to trump biological mechanisms suggests that the comparability is an important angle for family policies that need to go along with college-boosting education policies. We consider this to be an important policy implication of this study. Policies that particularly aim at triggering college-educated women into motherhood, for instance, through more flexible working hours (Goldin, 2006, 2014) or means-tested maternity leave benefits (Raute, 2019), seem promising for reducing the baby gap between women with and without a college education. Given our evidence that a biological fertility restriction does not seem to contribute much to the college effect on fertility, policies that aim at reducing the time to graduation by compressing the amount of instruction (such as the recent reform compressing academic track schooling in Germany) do not seem to close the baby gap.

While our results as well as explorative evidence indicate that a stronger polarization of jobs drive the fertility effects of college education, further research is needed to gain a better understanding of the interplay between (college) education, labor market characteristics and fertility preferences. Particularly two directions seem fruitful in this context: first, the setting we consider does not allow to disentangle a simultaneous selection into jobs and the decision to start a family from fertility preferences. Variation in technological change and the digitization of work may provide an angle to address simultaneous selection. Second, (apart from the descriptive evidence on job polarization) we need to rely on income as sufficient statistic for the family compatibility of jobs; a task-based classification of job flexibility may be better suited for this.

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Online Appendix A Additional figures and tables

Figures

Figure A1: Spatial variation of colleges across districts and over time

Notes: Own illustration based on the German Statistical Yearbooks 1959–1991 (German Federal Statistical Office, various issues, 1959–1991). The maps show all 326 West German districts (*Kreise*, spatial units of 2009) but Berlin in the years 1958 (first year in the sample), 1970, 1980, and 1990 (last year in the sample). Districts usually cover a bigger city or some administratively connected villages. If a district has at least one college, the district is depicted darker. Very few districts have more than one college. For those districts the number of students is added up in the calculations but multiple colleges are not depicted separately in the maps.

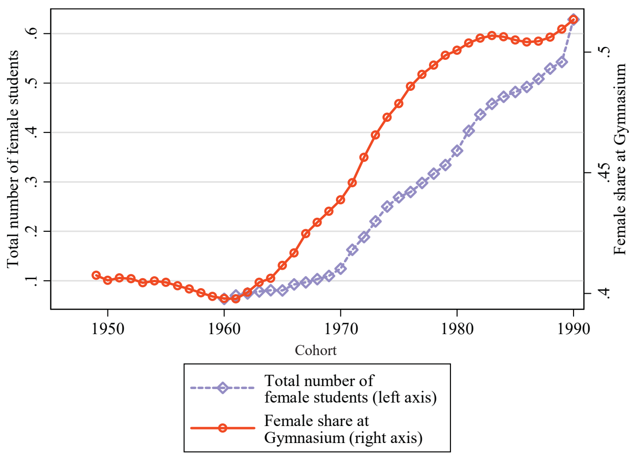

Figure A2: Trends in academic secondary school and college education for females
Notes: Own calculations using data from Köhler and Lundgreen (2014).

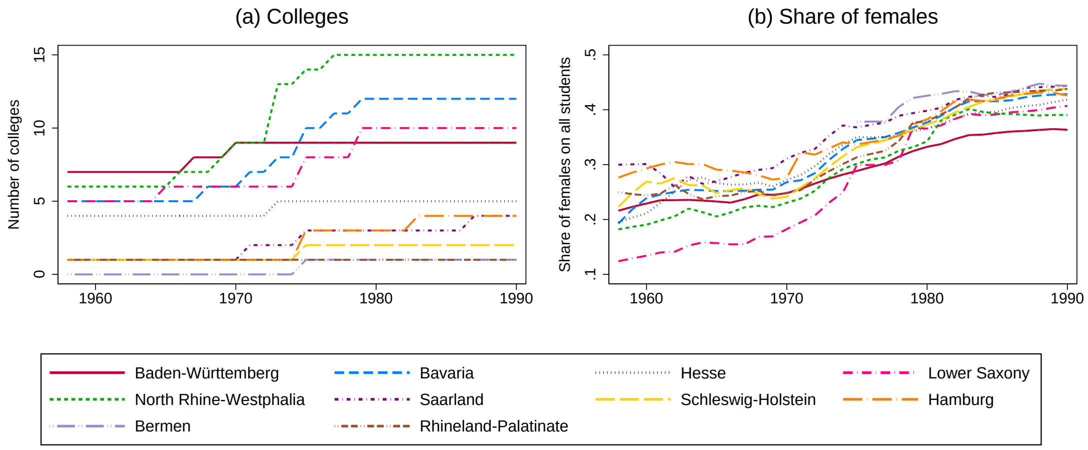

Figure A3: Trends in colleges and female students across federal states
Notes: Own calculations using data from the German Statistical Yearbooks 1959–1991 (German Federal Statistical Office, various issues, 1959–1991).

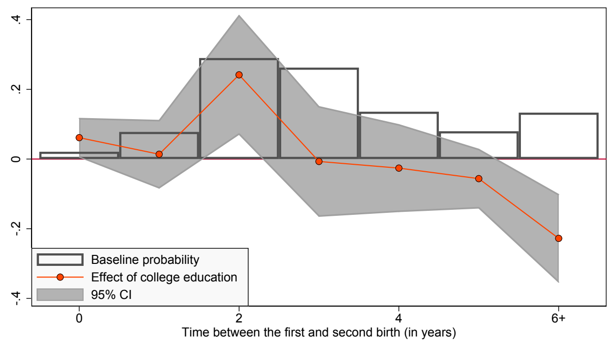

Figure A4: Effect of college education on the spacing of births
Notes: Own calculations based on NEPS–Adult Starting Cohort. The x-axis gives the distance between the birth of the first and the second child in years. The sample is restricted to mother of at least two children, 2,592 observations in total. The bars state the unconditional probability that the distance in births corresponds the number on the x-axis. Each maker gives the 2SLS effect instrumented college education from a regression where the outcome variable is each of the spacing indicators in turn. This is similar to the procedure presented in Eq. 7 in the main part of the paper. The shaded area depicts the 95 percent confidence interval for the point estimates.

Tables

Table A1: Baseline regression results

Variable	Definition	College degree yes	College degree no
General information (R: respondent, M: mother, F: father)			
Year of birth (FE)	Year of birth of R	1959	1959
Migrational background	=1 if R was born abroad	0.008	0.009
No native speaker	=1 if mother tongue is not German	0.003	0.004
Pre-college living conditions			
Siblings	Number of siblings	1.548	1.810
First born	=1 if R was the first born in the family	0.326	0.282
Age 15: single parent	=1 if R was raised by single parent	0.064	0.057
Age 15: patchwork	=1 if R was raised in a patchwork family	0.013	0.027
Age 15: orphan	=1 if R was a orphan at the age of 15	0.009	0.021
Age 15: rural district	=1 if district at R's age of 15 was rural	0.149	0.246
Age 15: M employed	=1 if M was employed at R's age of 15	0.448	0.486
Age 15: M never unemp.	=1 if M was never unemployed until R's age of 15	0.583	0.611
Age 15: F employed	=1 if F was employed at R's age of 15	0.965	0.948
Age 15: F never unemp.	=1 if F was never unemployed until R's age of 15	0.985	0.964
Pre-college health and education			
Final school grade: excellence	=1 if the overall grade of the highest school degree was excellent	0.035	0.015
Final school grade: sufficient or worse	=1 if the overall grade of the highest school degree was sufficient or worse	0.007	0.009
Repeated one grade	=1 if student needed to repeat one grade in elementary or secondary school	0.161	0.167
Repeated two or more grades	=1 if student needed to repeat two or more grades in elementary or secondary school	0.017	0.011
Parental characteristics			
M: year of birth (FE)	Year of birth of the R's M	1931	1931
M: age at birth	M's age when R was born	28.66	27.76
M: still alive	=1 if M is still alive in 2009/10	0.677	0.631
M: migrational background	=1 if M was born abroad	0.062	0.046
M: at least inter. educ.	=1 if M has at least an intermediate school degree	0.531	0.182
M: vocational training	=1 if M's highest degree is vocational training	0.703	0.896
M: college	=1 if M has a college degree	0.126	0.022
M: further job qualification	=1 if M has further job qualification	0.171	0.082
F: year of birth (FE)	Year of birth of the R's F	1928	1929
F: age at birth	F's age when R was born	31.37	30.37
F: still alive	=1 if F is still alive in 2009/10	0.477	0.441
F: migrational background	=1 if F was born abroad	0.064	0.051
F: at least inter. educ.	=1 if F has at least an intermediate school degree	0.612	0.241
F: vocational training	=1 if F's highest degree is vocational training	0.418	0.694
F: college	=1 if F has a college degree	0.317	0.083
F: further job qualification	=1 if F has further job qualification	0.264	0.223
Number of observations		921	3,267

Notes: Information taken from NEPS–Adult Starting Cohort data. In the case of binary variables, the mean gives the percentage of 1s. FE=variable values are included as fixed effects in the analysis. Mean values refer to non-missing observations. Missing information is replaced with 0 and a control variable for the transformation is include in the regression models.

Table A2: Descriptive statistics of instruments and background information

	(1)	(2)	(3)	(4)
	Statistics			
	Mean	SD	Min	Max
Instrument: College availability	0.459	0.262	0.046	1.131
Background information on college availability (implicitly included in the instrument)				
Distance to nearest college	27.580	26.184	0	172.269
At least one college in district	0.130	0.337	0	1
Colleges within 100km	5.860	3.401	0	16
College spots per inhabitant within 100km	0.034	0.019	0	0.166

Notes: Own calculations based on NEPS–Adult Starting Cohort data and German Statistical Yearbooks 1959–1991 (German Federal Statistical Office, various issues, 1959–1991). Distances are calculated as the Euclidean distance between two respective district centroids.

Table A3: Effect of college education on timing and spacing of births

	(1)	(2)
	Maternal age at 1^{st} birth (in years)	Years between 1^{st} and 2^{nd} birth
Panel A: OLS regression		
College degree	3.136^{***}	-0.399^{**}
	(0.237)	(0.145)
Panel B: Second-stage 2SLS regression		
College degree	6.460^{***}	-1.745^{***}
	(0.723)	(0.421)
Sample mean	27.3	3.5
Number of observations	3,410	2,557

Notes: Own calculations based on NEPS–Adult Starting Cohort data. Control variables include full sets of year of birth and district fixed effects as well as state-specific trends. For the full list of control variables, see Online Appendix A, Table A1. District-level clustered standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Baseline fertility rates and college effects by age

	(1)	(2)	(3)	(4)	(5)	(6)
	Extensive margin			Intensive margin		
Age	Baseline hazard		Effect	Baseline probability		Effect
	no college	college		no college	college	
17	0.024	0.002	−0.059	0.030	0.003	−0.048
18	0.045	0.002	−0.087	0.054	0.003	−0.091
19	0.067	0.006	−0.113	0.080	0.009	−0.123
20	0.084	0.015	−0.131	0.097	0.021	−0.129
21	0.102	0.019	−0.136	0.114	0.026	−0.115
22	0.128	0.030	−0.177	0.135	0.041	−0.152
23	0.147	0.047	−0.222	0.147	0.063	−0.166
24	0.167	0.061	−0.239	0.155	0.081	−0.142
25	0.210	0.070	−0.210	0.179	0.089	−0.095
26	0.233	0.109	−0.168	0.179	0.135	0.005
27	0.243	0.138	−0.178	0.164	0.164	0.042
28	0.241	0.150	−0.157	0.142	0.164	0.075
29	0.216	0.186	−0.101	0.110	0.191	0.119
30	0.213	0.201	−0.114	0.096	0.188	0.113
31	0.198	0.213	−0.082	0.079	0.177	0.126
32	0.161	0.202	0.018	0.057	0.151	0.138
33	0.141	0.168	0.045	0.045	0.110	0.112
34	0.135	0.170	0.025	0.040	0.101	0.097
35	0.105	0.153	0.020	0.029	0.084	0.064
36	0.068	0.116	0.019	0.017	0.057	0.039
37	0.059	0.102	0.026	0.014	0.047	0.046
38	0.044	0.077	0.011	0.011	0.034	0.034
39	0.031	0.060	−0.003	0.007	0.025	0.021
40	0.022	0.040	−0.029	0.005	0.016	0.008

Notes: Own calculations based on NEPS–Adult Starting Cohort data. The effects are those depicted in Figure 5 and estimated according to Eq. 7. Unlike the figure, the baseline hazard and the baseline probability are stated by college status.

Online Appendix B First-stage robustness and compliers

First-stage evidence from survey data and alternative instrument specifications
Table B1 shows that first-stage results for different instrument specification. While panel D gives our preferred specification, we start we the Card (1995) specification using a binary indicator that takes the value 1 if the college is in the home district by the time of the decision to go to college (and 0 otherwise). Controlling for district fixed effects, a college in the home district increases the probability of studying by about 15 pp in the full sample (column 1) and 18 pp in the sample of mothers (column 2). Using the distance to the nearest college in panel B, a 1 km reduction to the nearest college increases the probability to go to college by 0.3 pp. In panel C we consider to distance to all colleges weighted by a Gaussian kernel function. Using a 250 km bandwidth implies that (when holding all other college distances the same) a college opening in the home district increases the instrument by 0.4 ($= K(0)$) and the probability of studying by $0.157 \times 0.4 = 0.063$, 6.3 pp. College openings in other districts count for less (e.g., $K(200) = 0.29$ for a college opening in 200 km distance from the home district). This aggregation of all college distances has the advantage that others colleges than the nearest college are taken into account as well. In panels D to F we multiple the distance with the share of students to inhabitants. This accounts for two things: First, newly opened, but in the first years smaller colleges count less. Second, even if a surrounding district hosts a big college, this counts less when the number of potential students (captured thought the number of inhabitants) is high in this district. The specifications between panels D to F differ in the kernel bandwidth. As one might expect, the smaller the bandwidth, i.e., the more weight is put to close by colleges, the bigger the first-stage coefficient. In our preferred specification, an increase in the share of students in the home district by 5 pp results in an increased probability of studying by 4 pp (0.4 kernel weight $\times$ 0.05 increase in share of students $\times$ coefficient $2 = 0.04$).

Complying subpopulations
This first stage determines the share of individuals for which the second-stage conditions the effect on college education (that is, the compliers). By comparing the first-stage effect of increased college availability on the probability of studying across different subgroups, it is possible to gauge whether certain individuals were more likely to comply with the college expansion and, thereby, be captured by the second stage. To this end, we repeat the first-stage estimation in Table B2 along three potentially important characteristics by which we separate our data. The first subgroup is defined by the school degree of the father. This separation may be informative since it sheds light on the question of whether the educational expansion increased educational mobility. High-educated fathers are defined as having at least an intermediate track education, and hence more than the most common educational degree of that time. The shares of both subgroups are approximately balanced. However, the first stage is much stronger for women with lower-educated fathers as is evident from Table B2. Calculating the relative frequency of compliers of low-educated fathers relative to high-educated fathers ($0.63/0.37 = 1.7$, see table notes for details) indicates that a woman with a father we define as low educated is nearly twice as likely to comply with the college expansion as a woman with a high-educated father. Hence, in the example above, the college opening is supposed to increase the probability of studying by $0.06 \times 1.7 = 0.102$, 10.2 pp, for daughters of lower educated fathers.

Splitting the sample by the women's year of birth one can calculate the corresponding complier shares. The results show that the first-stage effect and, hence, also the share of compliers, is only slightly larger for women born after 1960, suggesting that our instrument has power throughout the educational expansion. This piece of evidence is moreover likely to be informative regarding the external validity of the results. As the first-stage effect does not seem to be confined to certain years in the time under review, it

Table B1: Baseline regression results

	(1) Full sample	(2) Mothers
Panel A: Binary indicator for a college in the district		
Instrument	0.160***	0.186***
	(0.044)	(0.045)
	[13.046]	[17.138]
Panel B: Distance to next college in km		
Instrument	−0.003***	−0.003***
	(0.001)	(0.001)
	[19.561]	[9.154]
Panel C: Kernel-weighted distance to all colleges (bandwidth 250 km)		
Instrument	0.161***	0.159***
	(0.010)	(0.012)
	[250.230]	[188.673]
Panel D: Kernel- and student-density-weighted distance to all colleges (baseline specification, bandwidth 250 km)		
Instrument	2.104***	1.981***
	(0.122)	(0.137)
	[296.727]	[209.790]
Panel E: Kernel- and student-density-weighted distance to all colleges (bandwidth 100 km)		
Instrument	4.676***	4.364***
	(0.300)	(0.334)
	[243.582]	[170.761]
Panel F: Kernel- and student-density-weighted distance to all colleges (bandwidth 400 km)		
Instrument	1.727***	1.634***
	(0.096)	(0.108)
	[321.149]	[228.790]
Number of observations:	4,188	3,217

Notes: Own calculations based on NEPS–Adult Starting Cohort data. Control variables include full sets of year of birth and district fixed effects as well as state-specific trends. For the full list of control variables, see Online Appendix A, Table A1. District-level clustered standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. F-statistics of the instruments in brackets.

is not implausible to conjecture that more recent policies have also had similar effects on promoting educational education.

The last dimension by which we analyze the first stage is the degree of urbanization. The first-stage coefficient is slightly higher in urban regions compared to the overall effect. Yet, as most college openings occur in cities, this urban–rural gradient of the educational expansion should not come as a surprise.[28] But in rural regions there is a substantial share of compliers that is nearly as high as the share of rural high school graduates in the overall population.

Table B2: First stage and some characteristics of complying mothers

	(1) Coefficient of the First Stage	(2) Share of the population	(3) Share of compliers	(4) Obs.
Overall first stage	2.08***	1	1	4,188
	(0.12)			
First stage by education of father[a]				
– High-educated fathers	1.63***	0.48	0.37	2,045
	(0.16)			
– Low-educated fathers	2.49***	0.52	0.63	2,243
	(0.15)			
First stage by year of birth (median separation)				
– Before 1960	1.78***	0.47	0.41	1,996
	(0.23)			
– 1960 or later	2.19***	0.53	0.59	2,292
	(0.12)			
First stage by urban-rural separation				
– Urban	2.12***	0.76	0.78	3,275
	(0.12)			
– Rural	1.89***	0.24	0.22	1,013
	(0.23)			

Notes: Own calculations based on NEPS–Adult Starting Cohort data. The shares of compliers are calculated as follows: For mutually exclusive groups (denoted by subscripts 1 and 2), the overall first stage coefficient is a weighted average of the respective subgroups if the group indicator is also interacted with the set of controls. In this case, weights are determined by the group shares ω_1 and ω_2 of the overall population. Thus, $\widehat{\delta_{\text{overall}}} = \widehat{\delta_1}\omega_1 + \widehat{\delta_2}\omega_2$. Accordingly, the shares of compliers can be determined as $\pi_j = \widehat{\delta_j}/\widehat{\delta_{\text{overall}}} \times \omega_j$, for $j \in \{1,2\}$. In this table, the group indicators are not interacted with all the controls, in order to present the same first stage result as employed for the main results. Therefore, the weighted average may not hold with equality until we normalize the weights π_j such that $\pi_1 + \pi_2 = 1$. This procedure has also been applied in Akerman et al. (2015). Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

[a] High-educated fathers are defined to have at least an intermediate track education, and hence more than the most common educational degree of that time.

[28]That regions with college openings have, on average, a larger share of primary industries – and are thereby more rural – may seem to contradict the result of Table 1 in the main text. However, the degree of urbanization used here is only based on the number of inhabitants, not on the population density.

Online Appendix C Bounding the sign of the intensive margin effect

Similar to marginal college–wage premiums under selection into employment as discussed in Westphal et al. (2019), we can bound the IME using one rather innocuous assumptions and two inequality restrictions. The one assumption that female college graduates are monotonously pushed toward childlessness because of college by the college-induced improved job opportunities. Concerning the inequality restrictions, the first is on the unobserved quantity $E(n^0 \mid d^0 = 1, d^1 = 0)$, that is, the expected number of children of non-college-educated mothers who would have opted against children if they were pushed toward a college education. By definition, this quantity is greater than 1. The second hypothesized restriction is on the sign of the intensive margin effect that we assume to be positive. Using the assumptions plus both restriction and applying the formula derived in Westphal et al. (2019) (where we conditioning on compliers implicitly), we can derive informative conclusions, which confirm that also the selection-free intensive-margin effect has to be positive:

$$\begin{aligned} E(n^1 - n^0 \mid d^0 = 1, d^1 = 1) &= \text{IME}\frac{E(d^0)}{E(d^1)} \\ &\quad - \Big(E(n^1 \mid d = 1) - \underbrace{E(n^0 \mid d^0 = 1, d^1 = 0)}_{\substack{\text{Children from untreated} \\ \text{compliers who would be} \\ \text{childless if treated}}}\Big)\frac{\overbrace{E(d^0 - d^1)}^{-\text{EME}}}{E(d^1)}. \end{aligned}$$

On the left-hand side (LHS) is the true intensive margin effect for women who have children irrespective of college education. The right-hand side is composed – with one exception – only of observed quantities. In order for our conclusion – that the true effect on the intensive margin is positive – to hold, the LHS needs to be larger than zero and we can hereby transform the equality above into the inequality below:

$$0 < \text{IME}\frac{E(d^0)}{E(d^1)} - \Big(E(n^1 \mid d = 1) - E(n^0 \mid d^0 = 1, d^1 = 0)\Big)\frac{-\text{EME}}{E(d^1)}$$

Plugging in our estimated values for the IME and EME and rearranging yields:

$$\begin{aligned} 0.267E(d^0) &> \Big(E(n^1 \mid d = 1) - E(n^0 \mid d^0 = 1, d^1 = 0)\Big)0.209 \\ E(n^1 \mid d = 1) - 0.267\frac{E(d^0)}{0.209} &< E(n^0 \mid d^0 = 1, d^1 = 0) \\ 2.279 - 0.267\frac{0.83}{0.209} &< E(n^0 \mid d^0 = 1, d^1 = 0) \\ 1.219 &< \underbrace{E(n^0 \mid d^0 = 1, d^1 = 0)}_{\text{by definition: } >1} \end{aligned}$$

Thus, as long as the unobserved quantity is larger than 1.219, we have a positive effect on the intensive margin. This is likely to be fulfilled, because, by definition, $E(n^0 \mid d^0 = 1, d^1 = 0)$ are factual mothers and, most naturally, mothers have at least one child. Thus, as long as more than every fifth of those non-college-educated mothers (who, however, would have gone to college had they been affected by the college expansion) had one additional (the second) child, we have a robust and positive effect on the intensive margin. The number of 1.219 children per mothers is low by any standard and also way lower than the already low fertility rate in Germany. Therefore, we tend to conclude that college-induced selection into childlessness does not reverse our positive effect on the intensive margin.

Online Appendix D The effect of college education on the QQ model parameters

Previous research and empirical evidence using the data at hand allow us to gauge how college education affects the three parameters of the augmented QQ model and their potential consequences for the extensive and the intensive fertility margins:

(i) The college-gradient in rearing costs: τ^{q1}/τ^{q0}

Is seems fair to assume that opportunity costs of forgone labor market earnings are likely to drive any effect of education on the costs of rearing children. A large body of literature suggests a positive college income premium, see, e.g., the literature review by Barrow and Malamud (2015) and Westphal et al. (2019) for women affected by the college expansion. If the wage potential of college-educated women is indeed higher than the one of their non-college peers, they have to forgo more income in order to rear offspring, i.e., $\tau^{q1}/\tau^{q0} > 1$. The existence such a child-income penalty is well documented, see, for instance, Kleven et al. (2019). In fact, their finding does not only suggest, that there is a severe income penalty, but this penalty seems to be higher in Germany than, e.g., in the US, UK, and Scandinavian countries. Adda et al. (2017) calculate that about three-quarters of the income penalty can be attributed to reduced labor supply, with the remainder being caused by wage effects. Looking at Denmark, Lundborg et al. (2017) empirically identify a reduction in the hourly wage (that is likely to be caused by a less-paying job closer to home). Most interestingly for us, Lundborg et al. (2017) also find evidence that the income penalty (for the first child) is higher for college-educated mothers. Moreover, Raute (2019) analyzes a parental leave reform that effectively reduces τ^{q1} while leaving τ^{q0} unchanged and finds positive fertility effects. Using NEPS data, we can find the expected positive college wage premium, see Table D1. College educated women earn, on average, about 50 percent more per hour of work.[29] Moreover, making use of the longer-running SOEP panel, we can confirm a severe child-income penalty, see Figure 4 in the main part of the paper. If the labor market opportunity costs are indeed higher for college graduates, this is likely to reduce the fertility at both margins, see that argument in Aaronson et al. (2014). Building on the augmented QQ model, Aaronson et al. (2014) provide reduced-form evidence that additional (secondary school) education reduces fertility along both margins.

(ii) The college-gradient in household income: y^1/y^0

Given the effect of college education on an individual's labor market income, it seems plausible, that the same goes for the household income, e.g., $y^1/y^0 > 1$. The difference between the household income channel and the individual income channel above is the

[29] Although fairly big, this effect is in line with Westphal et al. (2019), see the discussion therein for details.

partners' income. Assortative mating makes it likely that college-educated women have spouses that have themselves college education and earn more than spouses without college education. Column 2 and 3 of Table D1 give the coefficients of women's college education on the household income and the spouses' education. Both signs point towards the expected direction. For the purpose of our analysis it is reasonable to hold the women's contribution to the household income constant as any effects on this are captured through rearing costs. A higher household income then facilitates more children – and dismantles the QQ trade-off at least partly. How this affects the actual decisions on the fertility margins depends however on who does the child rearing. If both partners shoulder child rearing equally, (additional) children will affect the spouse's income similar to the woman's income and a higher income of the spouse increases the labor market opportunity costs. If, on the other hand, the woman takes most of the burden of child rearing, the higher household income enables more children. Evidence suggests that Germany belongs to the countries where rearing costs are unequally distributed and women shoulder, on average, a higher burden, see Feyrer et al. (2008). If so, college-gradient in household income taken alone will probably increase fertility at both margins.

(iii) The college-gradient in costs for parental inputs in children: τ^{e1}/τ^{e0}

Evidence suggests $\tau^{e1}/\tau^{e0} < 1$. That is, college-educated women face lower costs of investing in their offspring's human capital than women without college education. Björklund et al. (2006) find, for instance, a transmission of human capital from parents to their adopted children. Thus, at given rearing costs, offspring's education becomes relatively less expansive. Following Aaronson et al. (2014) the effect of this price shift is twofold: On the extensive margin, women need to have children in order to benefit from the lower human capital costs ($V^j\left(\tau^{qj}, \tau^{ej}, y^j\right)$ increases while $V_0^j\left(y^j\right)$ is unaffected in Eq. (6)) in the main part of the paper and are more likely to become a mother. On the intensive margin, the relative price reduction in offspring's human capital leads to a substitution of n with e in Eq. (5). Consequently, there are fewer but more educated children. Hence, $\tau^{e1}/\tau^{e0} < 1$ alone would predict to opposite of our finding.

Table D2 summarizes the expected signs of college education on both fertility margins through each of the QQ model parameters.

Table D1: The effect of college education on woman's wage, household income, and spouse's education

	(1) Woman's hourly wage	(2) Gross household income	(3) Spouse has college education
Panel A: OLS regression			
College degree	0.267***	0.274***	0.383***
	(0.038)	(0.025)	(0.028)
Panel B: Second-stage 2SLS regression			
College degree	0.497***	0.493***	0.725***
	(0.085)	(0.068)	(0.083)
Sample mean	2.8	8.0	0.3
Number of observations	1,497	3,848	2,683

Notes: Own calculations based on NEPS–Adult Starting Cohort data. Control variables include full sets of year of birth and district fixed effects as well as state-specific trends. For the full list of control variables, see Online Appendix A, Table A1. District-level clustered standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D2: Expected signs of the QQ mechanisms on the margins

	(1)	(2)	(3)	(4)
		Expected sign of college education on the...		
Gradient in...	QQ parameter	QQ parameter	Extensive margin	Intensive margin
rearing costs	τ^{q1}/τ^{q0}	$+$	$-$	$-$
household income	y^1/y^0	$+$	$+$	$+$
education costs	τ^{e1}/τ^{e0}	$-$	$+$	$-$
birth-order-specific rearing costs	$\frac{\partial \tau^{q1}(n)}{\partial n} / \frac{\partial \tau^{q0}(n)}{\partial n}$	$-$		$+$

Notes: Own illustration. The table summarizes expected effects of college education on the parameters of the Becker and Lewis (1973) quantity–quality (QQ) model, augmented by Galor (2012) and Aaronson et al. (2014). The parameters of interest are stated in column 1. Column 2 gives the expected effect of college education on the parameter stated on the left, while column 3 and 4 suggest how this transmits into the extensive and the intensive margin, respectively. Our expectations are based on the discussion in this subsection and the literature we refer to.

Online Appendix E Suggestive evidence on a college-gradient in job characteristics

Overtime

Figure E1 shows the distribution of overtime hours by college. Women with college education are more likely to have either a job with no to little overtime (the leftmost red bar exceeds the leftmost green bar) or a job with more than 20 extra hours (the rightmost bars). Women with college education are more likely to have a job with between 5 and 20 extra hours per week. We interpret this as suggestive evidence that the polarization of jobs into being career-oriented and family-friendly is stronger in jobs occupied by college-educated women. Column 1 in Table E1 looks at the effect of college education on average overtime. The empirical strategy here is similar to the baseline model, but we swap the outcome variable to be overtime. Although college education leads to a slight decrease in overtime but the effect size is below one hour per week and not statistically different from zero.

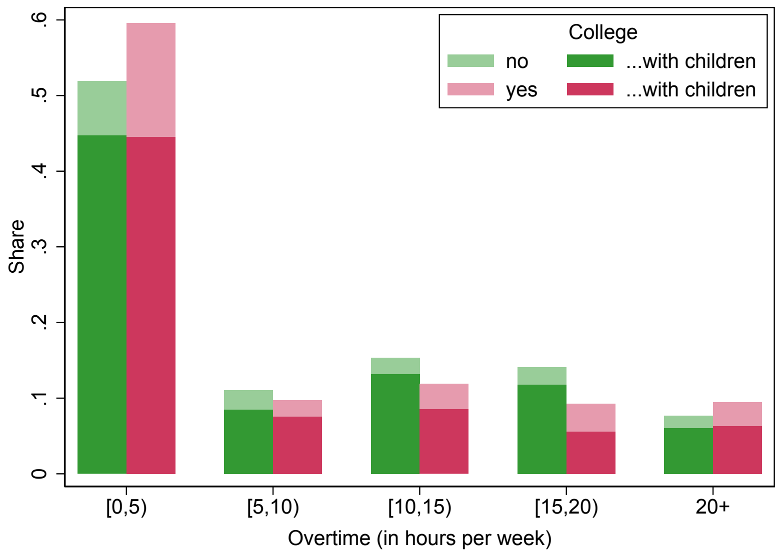

Figure E1: Distribution of overtime by college
Notes: Own illustration based on NEPS–Adult Starting Cohort data.

Figure E1 also compare the amount of extra hours between mothers with and without college education descriptively (the sample size prevents us to run separate regressions). College-educated mothers (depicted in dark) are as likely as mothers without college education to work very little or very many extra hours. They are, however, less likely to work between 5–20 extra hours per week.

Plotting the average overtime hours per week differentiated by college education and number of children in Figure E2 reflects similar lock-in effects as the child-income penalties in the main text. Given that women decide to have children, the reduction in working extra hours that goes along moving from the first to having a second child is lower for college- than for non-educated mothers. This pattern holds for total overtime in panel (a) of Figure E2 as well as conditional on positive overtime in panel (b).

Public sector employment

A prime example for being locked-in in a family-friendly job is public sector employment.

Table E1: Effect of college education on working overtime, public sector employment, and being a supervisor

	(1)	(2)	(3)	(4)
	Overtime (hours)	Public sector (1=yes)		Supervisor (1=yes)
	all	mothers	non-mothers	all
Panel A: OLS regression				
College degree	-0.392	0.219^{***}	0.130^{***}	0.049
	(1.003)	(0.026)	(0.069)	(0.029)
Panel B: Second-stage 2SLS regression				
College degree	-1.753	0.177^{***}	0.111	0.021
	(6.883)	(0.070)	(0.129)	(0.045)
Sample mean	7.6	0.2	0.2	0.6
Number of observations	1,523	3,304	805	1,397

Notes: Own calculations based on NEPS–Adult Starting Cohort data. Control variables include full sets of year of birth and district fixed effects as well as state-specific trends. For the full list of control variables, see Online Appendix A, Table A1. District-level clustered standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

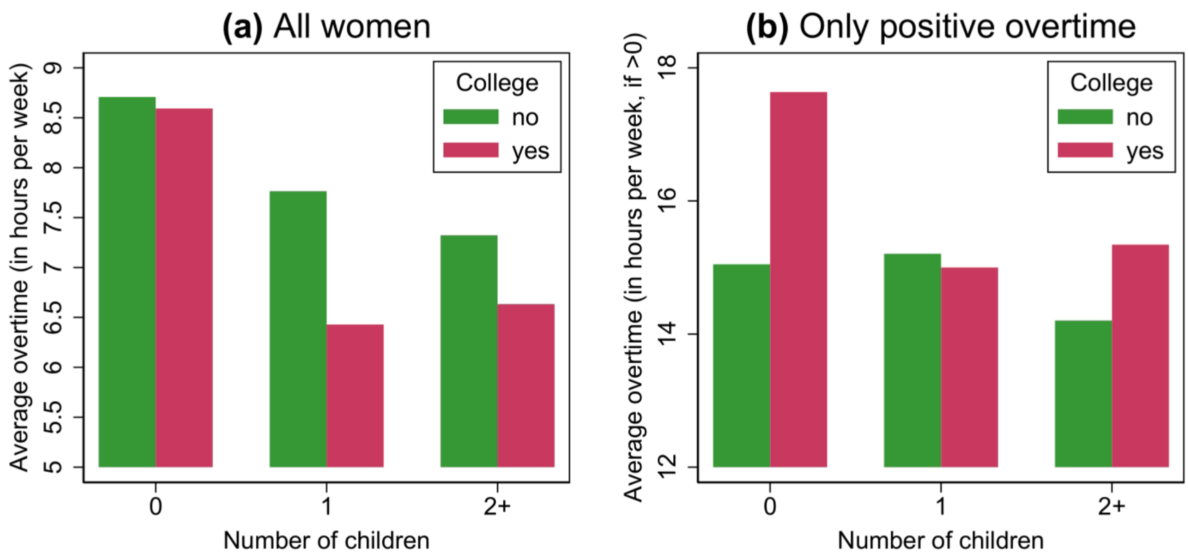

Figure E2: Distribution of overtime by number of children
Notes: Own illustration based on NEPS–Adult Starting Cohort data.

Panel (a) of Figure E3 provides descriptive evidence that public sector jobs seem indeed more family-friendly. College-educated women in private sector jobs work, on average, four more extra hours per week than their peers in public sector jobs. For women without college education the corresponding number is two hours per week. Panel (b) shows that college-educated women are not only twice as likely to work in a public sector job as women without college education, but college-educated mothers are also about 5 pp more likely to do so than college-educated non-mothers. In line with the negative extensive fertility margin, panel (c) exhibits that women without college education are more likely to become a mother than women with college education, independent of the sector of employment. However, among college graduates, women in public sector jobs are more often mothers than those in working in the private sector. Finally, conditioning on being a mother, panel (d) of Figure E3 gives the average number of children by college education and employment sector. Comparing these four groups, college-educated mothers in public sector jobs have the most children (around 2.1) – whereas college-educated mothers in private sector jobs have about 0.1 children less, on average. For mothers without college-education is difference is negative, that is, mothers working in the private sector have, on average, more children than mothers in the public sector. Although purely descriptive, all four patterns support the notion that labor market lock-in effects mediate a positive intensive margin effect of college education.

Columns 2 and 3 in Table E1 give the effects of college education on public sector employment for mothers and non-mothers. While the OLS results suggest that college graduates are more likely to work in the public sector independent whether they have children, the IV results indicate that college education significantly increases the probability of working in the public sector by about 18 pp for mothers, but not for non-mothers. Thus, the choice to become a mother is at least correlated with the choice to work in the public sector.

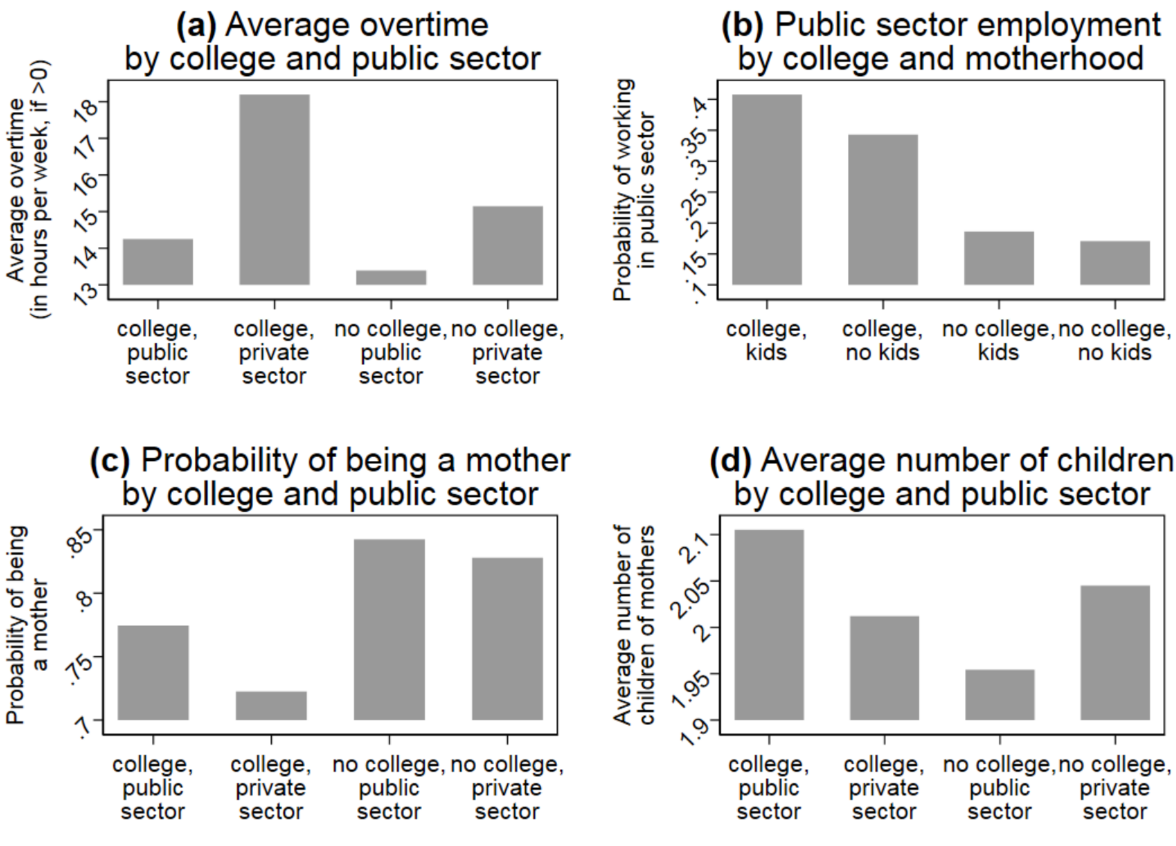

Figure E3: Public sector employment

Notes: Own illustration based on NEPS–Adult Starting Cohort data.

Working in a supervising position

Figure E4 plots the fraction of women who as supervisor. As one might expect, college graduates are about twice as likely to be a supervisor than women without college education, independent of the number of children. Unfolding the overall pattern by the number of children, the probability of being a supervisor declines for women without college education by about 0.5 pp between having no child and the first child. The second child reduces the probability compared to the first child by an additional 1 pp. For college-educated women the association in the reduction of being a supervisor between no child and the first child is 2 pp. However, once a college graduate has a child, there is no additional reduction in the probability of being a supervisor when having two children. While the sample size in Table E1 prevents us to unfold the overall effect of college education on the probability of being a supervisor by the number of children, even the conditional effect in column 4 seems to be rather humble and not different from zero.

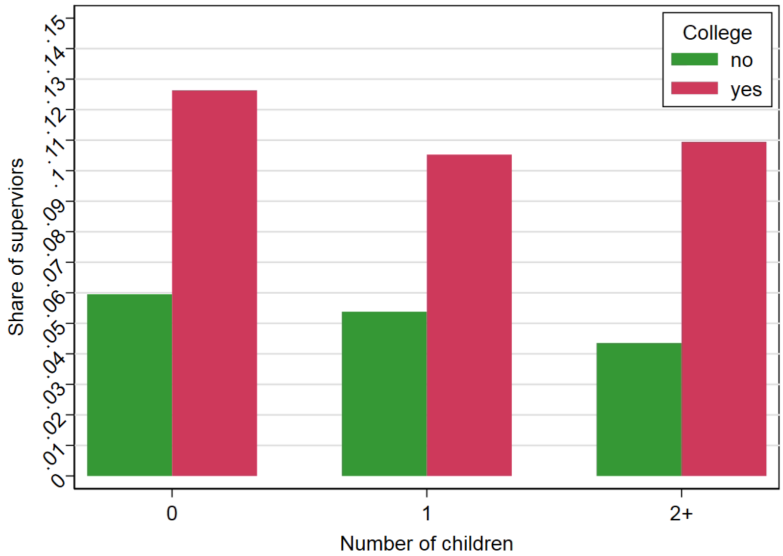

Figure E4: Probability of working in a supervising position by college and number of children

Notes: Own illustration based on NEPS–Adult Starting Cohort data.

All in all, the descriptive pattern depicted in this appendix gives support to the existence of a college-gradient in labor market lock-in effects that corresponds to the increased affordability of additional children for college-educated mothers. However, given the descriptive nature of the analyses conduced in this appendix, this evidence may be interpreted as ranging between anecdotal and suggestive, but certainly not firm.

Online Appendix F The impact of decreasing the cost for additional children along the birth order

Log-liner preferences

The QQ model with log-linear preferences (used in Galor, 2012) reads as follows:

$$\max U(n, e) \qquad \text{s.t.} \quad y = n(\tau^q + \tau^e e)$$

$$U(n, e) = \ln(n) + \beta \ln(e)$$

In constrast to Galor (2012), however, there are only two choice variables, n and e, so c is omitted. As a new feature, we allow τ^q to change for additional children on the intensive margin (beyond the firstborn) to change (since we think they may decrease due to economies of scale). To be precise, we parameterize these costs for college-educated mothers as follows:

$$\tau^{q1}(n) = \gamma^1 + \alpha \ln(n) \qquad \forall \quad n \geq 1$$

Non-college-educated mothers, in contrast, have constant marginal effects (although for our argument to hold, we only need that the economies of scale are larger for college-educated mothers):

$$\tau^{q0}(n) = \gamma^0$$

The difference between these cost structures has the following marginal effects:

$$\frac{\partial \tau^1(n) - \tau^0(n)}{\partial n} = \frac{\alpha}{n}$$

Solving this model, one can derive the optimal number of children (desired fertility) for college and non-college-educated mothers:

$$n^{1*} = \frac{\left(1/\beta - 1\right) y^1}{(1/\beta)\tau^{q1} + \alpha}$$

$$n^{0*} = \frac{\left(1/\beta - 1\right) y^0}{(1/\beta)\tau^{q0}}$$

These depend on the model parameters y^T and τ^{qT} (τ^{eT} drops out of the model because of log-linear preferences). Important for analyzing the effect college education has on the the intensive margin. Therefore, we relate both optimal responses (n^{1*} and n^{0*}) to one another:

$$\frac{n^{1*}}{n^{0*}} = \frac{y^1/y^0}{\tau^{q1}/\tau^{q0} + \alpha\beta/\tau^{q0}}$$

We have a positive effect on the intensive margin, if and only if $\frac{n^{1*}}{n^{0*}} > 1$. Inserting this inequality in the expression above and rearranging yields:

$$\begin{aligned} & 1 < \frac{y^1/y^0}{\tau^{q1}/\tau^{q0} + \alpha\beta/\tau^{q0}} \\ \Leftrightarrow \quad & \alpha\beta/\tau^{q0} < y^1/y^0 - \tau^{q1}/\tau^{q0} \\ \Leftrightarrow \quad & \alpha < \frac{\left(y^1/y^0 - \tau^{q1}/\tau^{q0}\right)\tau^{q0}}{\beta} \end{aligned}$$

This expression shows that if the marginal costs of having children (the child penalty) are decreasing in the birth order of the additional child for college-educated mothers relative to non-college-educated mothers, then a positive effect on the intensive margin may appear. If there is no assortative mating – such that $y^1/y^0 = \tau^{q1}/\tau^{q0}$ – then a positive effect on the intensive margin emerges if and only if there are decreasing marginal effects of an additional child. This result holds irrespective of the generel cost level of children (τ^{q1}/τ^{q0}).

Constant elasticity of substitution (CES) preferences
This proposition does not only hold with log-linear preferences. Using constant-elasticity-of-substitution (CES) preferences as used e.g. in Mogstad and Wiswall (2016), we can also show that economies of scale in the cost for children (that are independent of their human capital investments) lead to a negative effect on the extensive - but a positive effect on the intensive margin.

The QQ optimization problem for CES preferences generally looks as follows:

$$\max \left[\pi n^{\sigma} + (1-\pi)e^{\gamma\sigma}\right]^{(1/\sigma)\eta} c^{1-\eta} \qquad \text{s.t.} \quad y = n(\tau^{q}(n) + \tau^{e}e + c)$$

We solve this problem twice (for college and non-college educated mothers), where $\tau^{q}(n) = \gamma + \alpha \ln(n)$ for college-educated mothers and $\tau^{q}(n) = \gamma$ else. Then, we are interested in effects of varying α on $\frac{n^{*1}}{n^{*0}}$ (which is greater than one if college education has a positive effect on the intensive margin). Analytically, this yields rather complicated expressions. Yet, solving this model for different values of α yields valuable insights that are depicted in Figure F1. As for log-linear preferences, increasing the economies of scale for additional children for college-educated mothers (decreasing α) increases the number of children of college educated mothers (the blue line – its level is mapped on the left scale). This results holds irrespective of the relative costs for children of college-educated to non-college educated mothers (the red line – its level is mapped on the right scale) which can be higher for college educated mothers.

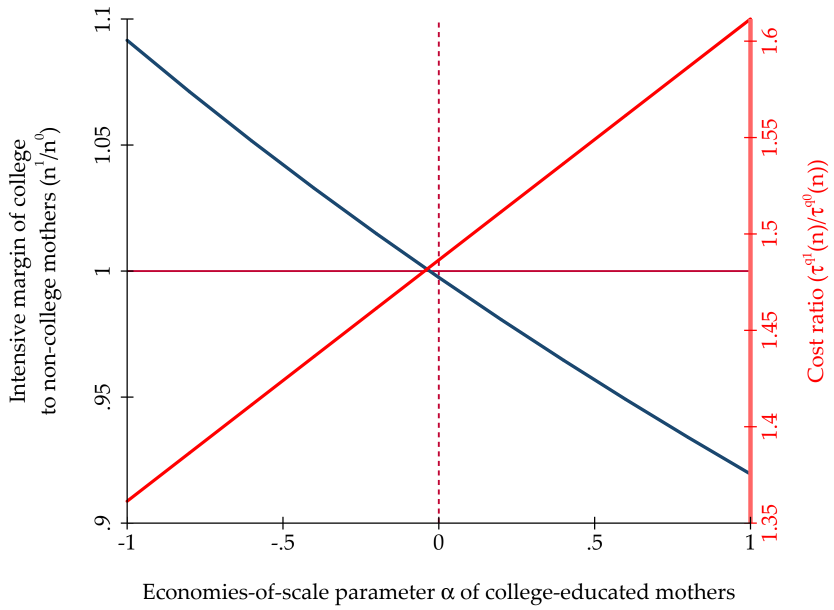

Figure F1: Intensive margin effect explained by CES preferences

Notes: This graph depicts effects of varying the relative degree of economies of scale for college-educated mothers w.r.t. to non-college-educated mothers (α) on the intensive margin (n^{*1}/n^{*0}, the left scale) and the cost ratio (τ^{q1}/τ^{q0}, the right scale) using CES preferences ($\max[\pi n^{\sigma} + (1-\pi)e^{\gamma\sigma}]^{1/\sigma\eta}c^{1-\eta} \quad$ s.t. $\; y = n(\tau^{q} + \tau^{e}e + c)$). As in Mogstad and Wiswall (2016), the parameters in this graph are set as follows: $\sigma = -9$ (CES= 0.1), $\eta = 0.3$, $\pi = 0.5$.

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ISSN 2190-9938 (online)
ISBN 978-3-86304-315-5