The Effect of More Efficient Education on Family Formation: Evidence from the G8 Reform in Germany *

Luzia Bruckamp

LSE

January 2025

Abstract

Falling fertility rates in high-income countries have gone hand in hand with the postponement of fertility to later ages. One hypothesized driver of postponement is increased education. An open policy-relevant question is whether shortening the duration of education could reverse some of the postponement we have seen so far and lower age at first birth. Since there is potentially a causal relationship between postponement and completed fertility, more efficient education might act as a pro-natal policy. Using novel data from a German education reform that shortened the length of academic-track secondary school, I examine the effects of shorter education on family formation. While the reform successfully lowered age at highest degree completion, I find no effects on age at first cohabitation, age at first marriage, and age at first birth. This result highlights a potential asymmetric effect where increases in education ratchet up the age at first birth but decreases in the length of schooling are not able to significantly decrease age at first birth again, suggesting an important role for social norms.

*I thank Tim Besley and Kevin Kuruc for helpful comments on earlier versions of this draft. I gratefully acknowledge PhD funding from James Houghton and Goldman Sachs through the Goldman Sachs Gives - James Houghton PhD Scholarship. LSE Department of Economics, Houghton Street, London WC2A 2AE (e-mail: l.bruckamp@lse.ac.uk).

1 Introduction

Fertility rates have fallen dramatically across developed economies, with most high-income countries now experiencing below-replacement fertility. A key feature of this demographic transition has been the increased postponement of childbearing, with women having their first births at progressively later ages. The shift to later parenthood not only affects period fertility rates but may also impact completed fertility through both biological and social channels. Since fecundity decreases with age, particularly for women, delaying childbearing increases the risk of involuntary childlessness and reduces the probability of reaching a desired family size [Geruso et al., 2023]. Additionally, as individuals spend more time without children, they may adjust their fertility preferences downward [Beaujouan, 2023].

This trend of fertility postponement is influenced by a complex set of social and economic factors, with education likely playing a central role [Ní Bhrolcháin and Beaujouan, 2012]. As young adults spend more years in education and face a lengthening transition to financial stability, they tend to delay family formation. This delay is further reinforced by an increase in perceived income prerequisites for having children, as parents aim to achieve certain career and financial milestones before starting a family [van Wijk and Billari, 2024].

In this paper, I examine whether shortening the duration of education could help address fertility postponement and thereby counteract falling birth rates in high-income countries. I exploit a unique natural experiment in Germany - the G8 reform - which reduced the length of academic-track secondary school (Gymnasium) from 9 to 8 years while maintaining the same curriculum. Using data from three panel studies, I analyze how this compression of education affects the timing of key life events including completion of highest degree, first cohabitation, first marriage, and first birth.

The hypothesized mechanism behind this is that we expect people to follow relatively fixed life trajectories, especially at the top of the education distribution. After secondary school, there is university education, then marriage, then childbirth. Of course, social norms have changed over time and it is for example much more common to have children without being married. However, most people

still follow this default pathway and it is very uncommon for university students to become parents. If the education component of this life trajectory becomes shorter, we would expect all the other milestones to move forward as well, thereby lowering the age at first marriage and at first birth.

The G8 reform provides an ideal setting to study this potential mechanism. First, it affected the highest-ability students who typically experience the most fertility postponement. Second, the staggered implementation across German federal states combined with school entry cutoffs creates quasi-random assignment to treatment. Third, the reform is relatively recent, affecting birth cohorts in the late 1980s and in the 1990s. This means that we can observe family formation for birth cohorts that have arrived in a regime of low fertility and high postponement.

My empirical approach involves assigning respondents to the treatment and control group based on their birth month and year, as well as the federal state where they went to school. I conduct a descriptive analysis of age-specific rates for key life events using the last three G9 cohorts and the first three G8 cohorts. I then focus on the last G9 cohort and the first G8 cohort to statistically test for differences in the mean age at highest degree, first cohabitation, first marriage, and first birth. To address mechanical age differences between cohorts, I employ careful censoring of observations and conduct an analysis using simulations based on population-level birth statistics.

I find that while the reform successfully reduced the age at highest degree completion by about 6 months, it had no significant effects on the timing of family formation. Neither age at first cohabitation, age at first marriage, nor age at first birth show meaningful differences between the reform cohorts and their predecessors. This null result holds for both men and women.

The findings suggest an important asymmetry in how education policy affects fertility timing. While increases in education have historically pushed up the age at first birth, decreases in educational duration do not appear capable of reversing this trend. I argue that this asymmetry likely stems from sticky social norms about the "right" age to have children, which have evolved alongside educational expansion but resist downward adjustment when education is compressed.

2 Related literature

The existing literature on education and fertility has primarily focused on increases in schooling. These studies have produced mixed evidence. Aaronson et al. [2014] develop a quantity-quality model that allows for an extensive margin. They find that black women in the American South who obtained more schooling due to a school construction program, delayed childbearing. In Germany, Cygan-Rehm and Maeder [2013] found that extending compulsory schooling from 8 to 9 years reduced completed fertility and increased childlessness, which they attribute to Germany's high opportunity costs of childrearing. However, Fort et al. [2016] document substantial heterogeneity across Europe, finding negative effects of education on fertility in England but not in Continental Europe. Geruso and Royer [2018] and Cummins [2022] analyse an increase in the compulsory schooling age in England in 1972. While the former find a negative effect of the extended schooling on teen fertility, both papers find no effect on completed fertility. The results on teen fertility are consistent with an "incapacitation effect" of school which is also what Berthelon and Kruger [2011] find in the context of longer school days in Chile that end up reducing teen fertility and crime.

A recent paper by Beck et al. [2024] uses school entry cutoff and exact date of birth to study the effect of school starting age on the timing of family formation. Using a regression discontinuity design, they compare those born just before and just after the school entry cutoff. They find that being born after the cutoff increases age at first marriage by 4.7 months for women and 2.4 months for men. Similarly, age at first birth is increased by 2.9 months for women and 4.0 months for men. There are no effects on completed cohort fertility or the overall probability of having a partner. Schaffner and Siebert-Meyerhoff [2017] employ the same methodology for a sample of German women and find that there is a small increase in the age at first birth for those who are older at graduation due to being born after the cutoff.

This paper makes several important contributions to the existing literature on education and fertility. The G8 reform provides a unique opportunity to examine how shortening secondary education affects fertility timing. Unlike previous studies that analyzed increases in schooling, this reform compressed the academic track of secondary education while maintaining educational content. By studying its

effects on age at first birth, this paper contributes to our understanding of whether educational duration affects fertility timing symmetrically when shortened rather than extended, and whether shorter educational trajectories might help address fertility postponement in high-income countries.

The papers that study increases in schooling mostly focus on reforms that happened several decades ago when fertility rates were still substantially higher and overall patterns of family formation, fertility, and female labor force participation were very different. My paper uses data from a reform that affected birth cohorts in the 1980s and 1990s who are now in their 20s and 30s. We can therefore observe the effects of shortened education in a population that is experiencing the low fertility rates and high ages at first birth that we see in high-income countries these days.

Additionally, the papers that study the effect of increases in compulsory schooling estimate local effects at the bottom of the education distribution since that is where those reforms are binding. In contrast, the G8 reform that I study affected the highest-ability students. This is particularly interesting since it is at the top of the education distribution where we expect the most postponement.

Another important contribution is that the papers that use exact date of birth and school starting age can only estimate a local effect around the cutoff date for starting school. In contrast, I am able to study entire cohorts experiencing a one-year decrease in the length of schooling. This is relevant since previous research has found substantial differences in maternal characteristics across birth months [Buckles and Hungerman, 2013].

Recent evidence from Zhou et al. [2024] shows that fertility responses to policy interventions can be asymmetric. They find that anti-fertility policies have generally worked better than pro-fertility policies and build a micro-founded model that can explain this fact. My paper documents another instance of asymmetric policy effects, contributing to our understanding of why pro-natalist policies often struggle to raise fertility.

3 Setting

In the German school system, children usually start school when they are 6 years old and go to primary school for 4 years. At the end of primary school, children move on to different secondary schools based on their abilities. There are three main types of schools, Hauptschule, Realschule, and Gymnasium. The Gymnasium is for the highest-achieving students and finishing it means obtaining the Abitur, the German high school leaving certificate, and therefore being allowed to attend any university. Roughly one third of children attend a Gymnasium.

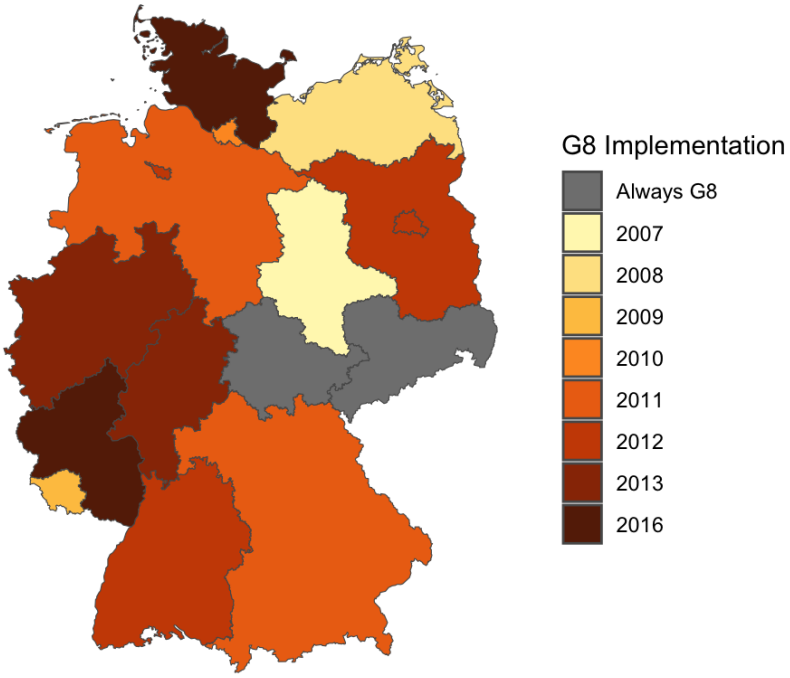
Education policy is not set centrally but managed by the 16 federal states in Germany. Before the G8 reform was implemented, the Gymnasium took 9 years to complete in 14 of the 16 federal states. Only in Saxony and Thuringia, students would finish the Gymnasium after 8 years. That meant that in most of Germany, the average student finished school at 19. This was old in comparison to most other European countries. Politicians also argued that it would be beneficial for the economy if young people started working earlier, especially given the aging population.

These arguments led to the so called G8 reform in all remaining federal states, where the Gymnasium was shortened to 8 years. The goal was to not compromise on the level of education though: The curriculum stayed the same and the teaching hours of the extra year were distributed among the other years, resulting in increased instruction hours throughout the Gymnasium.

Different federal states introduced the reform in different years. The first G8 cohorts finished school between 2007 (Saxony-Anhalt) and 2016 (Rhineland-Palatinate and Schleswig-Holstein). Figure 1 shows the year of graduation for the first G8 cohort in each federal state.

It is important to note that the reform was generally introduced in a way that it applied to children already going to school. This means that it would have been impossible for parents to strategically delay or speed up their child's entry into school in order to make sure that they end up in a G8 or G9 cohort.

Figure 1: Year of graduation of the first G8 cohort in each federal state.



After backlash from teachers, parents, and students, the reform is now being rolled back in several federal states with the first new G9 cohorts having graduated in 2021 in Lower Saxony.

Previous research shows various effects of the G8 reform on outcomes during school like grades and mental health, as well as outcomes after school like university attendance. Huebener and Marcus [2015] provide an overview of empirical research on reform effects.

The most relevant evidence on the G8 reform for this paper concerns the effect of the reform on age at various milestones. Huebener and Marcus [2017] provide evidence on age at high school graduation while Marcus and Zambre [2019] examine age at university enrollment.

Huebener and Marcus [2017] focus on outcomes during school. Using administrative data on the population of students, they find increased grade repetition and lower GPAs in the Abitur but no effect on graduation rates. Due to the in-

creased grade repetition, the reform does not lower the average age at high school graduation by a full year as intended, but only by 10 months.

Marcus and Zambre [2019] use administrative data on all university students in Germany to assess the effects of the G8 reform on university enrollment and degree progress. They find that the reform decreased university enrollment. Treated students were also more likely to delay university entry, for example by going abroad for a year. Meyer et al. [2019] and Meyer and Thomsen [2016] confirm these results and find that university enrollment decreased specifically for female students in the first year after graduation.

Marcus and Zambre [2019] also find that treated students were more likely to change their major and progressed more slowly through their university degree. All these findings point to adverse effects of the reform that dampen its impact. The authors estimate that age at university enrollment is eight and a half months lower for the G8 cohorts compared to the G9 cohorts. They do not directly estimate age at finishing university but the adverse effects that they find imply an age difference that is smaller than eight and a half months.

Taken together, the results of these studies imply a gradual dampening of the reform effect on age at each step from school to labor market entry, when it comes to university students.

4 Data

The data that I am using come from three different panel studies. The first one is the German Socio-Economic Panel (SOEP), a representative survey of the German population that has been running since 1984 and samples roughly 30,000 adults every year. The second one is the Panel Analysis of Intimate Relationships and Family Dynamics (pairfam). This panel survey has 12,000 people from the cohorts 1971-1973, 1981-1983, 1991-1993, and 2001-2003. For my analysis, only the 1991-1993 cohorts are relevant. The study ended in 2022 but the participants continue to be surveyed by the new German Family Demography Panel Study (FReDA). This is the third datset that I am using. It started in 2021 and surveys a representative sample of the German population aged 18-45 as well as their (ex-)partners. For

the FReDA dataset, I am able to not only use the anchor data but also some of the partner data.

In all three studies, I am able to extract the month and year of birth of the respondent, their gender, the federal state that they grew up in, the highest school degree they obtained, and the month and year of birth of any children that they have. I restrict my sample to those who have Abitur and were therefore potentially subject to the reform. I also exclude respondents who grew up in Saxony or Thuringia where no reform happened since they already had an 8-year long Gymnasium for decades. Furthermore, I exclude respondents from Hesse and Rhineland-Palatinate where the reform was introduced gradually over several years, making it impossible to identify whether respondents were in the treated or control group.

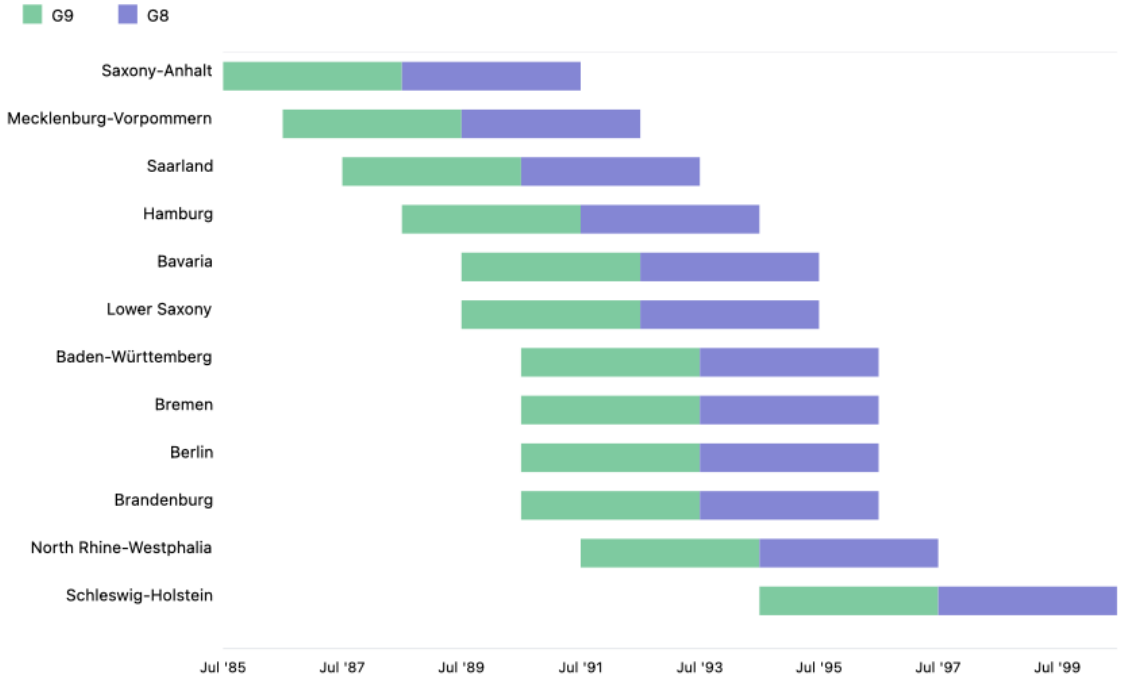
I determine whether a given respondent was part of a G8 or G9 cohort based on their birth month and year. In the period of interest, the cutoff for starting school in the remaining 12 federal states was the end of June. This means that children who turned 6 years old by the end of June had to start school that year. Children who turned 6 in July or later would start the year after.

There are ways for parents to send their child to school a year early or delay by a year but most children end up in the cohort that they were originally supposed to be in. For example, in the relevant year in Baden-Württemberg, 7% started school early. These would be children that I assign to the first G8 cohort even though they are actually in the last G9 cohort due to starting early. On the other hand, 6% started late which would be the children that I assign to the last G9 cohort but who actually end up in the first G8 cohort. In Saxony-Anhalt, the other state for which I have this information, 1% started early in the relevant year and 7% started late. As mentioned previously, there is no concern for strategic manipulation since parents could not have anticipated which cohort would be the first to switch to G8 given that the reform was generally announced and implemented when those cohorts had already started going to primary school.

For each federal state, I include those born within the 36 months before and after the cutoff. That means I have the three last cohorts that still did Gymnasium in 9

years and the three first cohorts that attended the Gymnasium for 8 years. Figure 2 shows the birth cohorts that I use in each federal state.

Figure 2: Birth cohorts 36 months before and after the policy change in 12 federal states that introduced G8 after 2000.



Combining the data from all three studies and filtering for the last three G9-cohorts as well as the first three G8-cohorts, results in a total sample of 4,521 respondents, with 2,107 respondents in the treatment group who attended secondary school for 8 years. The resulting age distribution for the full sample is visualized in Figure 13 in the appendix and exact numbers can be found in Table 2 in the appendix.

I create a subsample of the data for which I have information on the first cohabitation as well as the first marriage and call this the relationship sample. This is the case for the FReDA anchor data and for parts of the pairfam sample. The relationship sample comprises 2,887 respondents, i.e. roughly 64% of my original sample. The age distribution is visualized in Figure 14 in the appendix and exact numbers can be found in Table 3 in the appendix.

5 Results

I analyze age-specific rates of highest degree completion, first birth, first cohabitation and first marriage to investigate differences in family formation between the first three G8 cohorts and the last three G9 cohorts. This descriptive analysis is supplemented by simple t-tests comparing the last G9 cohort and the first G8 cohort. Identification comes from the fact that school entry cutoffs combined with the staggered introduction of the reform, lead to random assignment to the last G9 cohort or the first G8 cohort.

The problem with this approach is that the last G9 cohort is on average older than the first G8 cohort. That means that mechanically, their age at various life events will be older since we have more right-censoring of the G8 cohort. In order to mitigate this, I determine the highest age of G8 respondents in each federal state. If a G9 respondent experiences their degree completion, a first birth, first cohabitation or first marriage at an age that is higher than the maximum age of G8 respondents in that federal state, I discard this observation. So for example, if the oldest G8 respondent in North Rhine-Westphalia is 29, then I will determine the average age at first birth in the last G9 cohort by only counting first births that happen up until the age of 29.

The reason why I cannot run an RDD with the data is that around the cutoff, the effects of school starting age and the reform cancel each other out. I will illustrate this with the example of Saarland, where the cutoff between the last G9 cohort and first G8 cohort was July 1990. Those who were born in June 1990, started going to school in the summer/fall of 1996 and thus had a school starting age of 6 years. They were the last G9 cohort and thus graduated after 13 years in 2009. Those who were born born in July 1990 started going to school in the summer/fall of 1997 and thus had a school starting age of 7. They ended up in the first G8 cohort and therefore also graduated in 2009, after 12 years. Those born right before and right after the cutoff thus finished school at the same time at roughly the same age. If we were to estimate an RDD around the cutoff, we would thus capture the joint effects of school starting age and the G8 reform. Instead, I have to compare the entire cohorts to each other.

5.1 Age at highest degree

Before looking at family formation outcomes, I analyze the age at completion of the highest degree. I have information on the age at highest degree only for the sample of FReDA anchor respondents. We know from previous studies (Huebener and Marcus [2017] and Marcus and Zambre [2019]) that the G8 cohort is roughly 10 months younger when finishing school, roughly 8.5 months younger when starting university and that they show slightly slower study progress while being at university, for example because they change their major more often. That means we would expect a further dampening of the age gap by the time they finish their studies.

Figure 3 shows the rates of highest degree completion at each age. We can see that the overall curves are shifted to the left for the G8 group compared to the G9 group, meaning that they finish their highest degrees at younger ages. For example, those who do a vocational qualification as their highest degree would usually finish after three years which would be age 21 for the G8 group. We can see that at age 21 there is a much higher rate of degree completion for the G8 group than for the G9 group.

Figure 3: Rates of finishing the highest degree by age are shifted to the left for the G8 cohorts compared to the G9 cohorts.

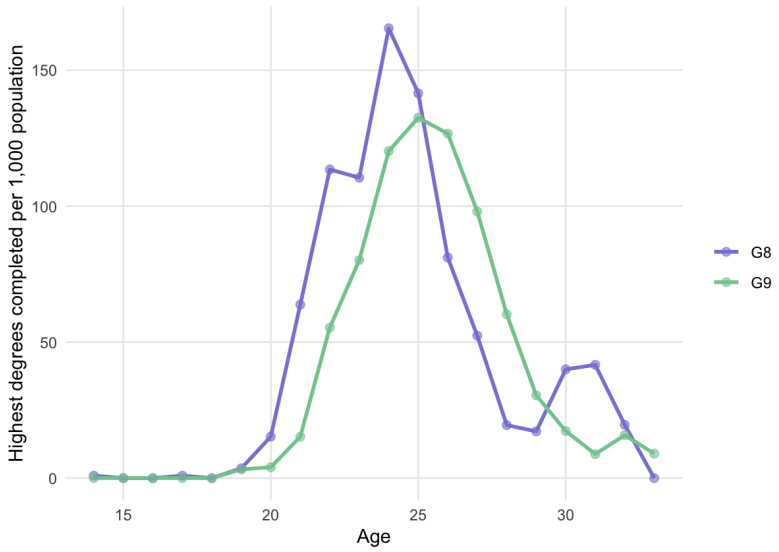
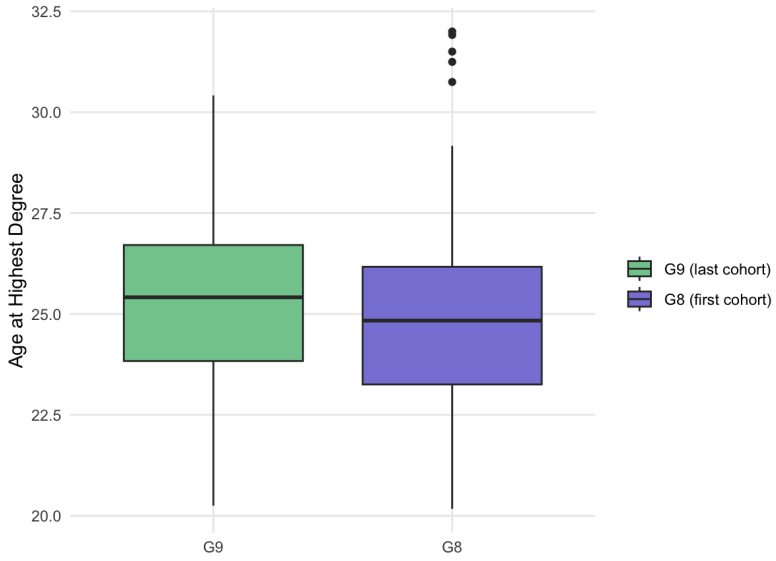


Figure 4 shows the average age at highest degree completion for the last G9 cohort and the first G8 cohort. Note that, as described above, I censor some observations from the G9 cohort to account for the fact that they are on average older than the G8 cohort. As can be seen in Table 1, the average age at highest degree for the G9 cohort is 25.3 years, while it is 24.8 years for the G8 cohort. A simple t-test reveals that this difference is significant at the 1%-level. The difference of 6 months lines up well with what we would have expected given previous results about the age at university enrollment and study progress.

Figure 4: Average age at highest degree is 6 months lower for the G8 cohort compared to the G9 cohort.



5.2 First births

My main focus is analyzing the patterns of first births in order to answer the question of whether shortening education can decrease fertility postponement. I construct age-specific fertility rates for first births given that I have the exposed population at each age and first births at each age. I then sum up the age-specific fertility rates for both groups to get a cumulative probability of having had a first birth at each age.

Figure 5 shows age-specific fertility rates for first births from age 14 to 33. There

is no leftward shift visible for the G8 cohorts compared to the curve for the G9 cohorts, contradicting our hypothesis that G8 cohorts will experience a decrease in the age at first birth. Looking at the cumulative probability of having had a first birth, Figure 6 shows that it is lower in the G8 cohorts compared to the G9 cohort.

Figure 5: Age-specific fertility rates for the G8 cohorts show no leftward shift compared to the G9 cohorts.

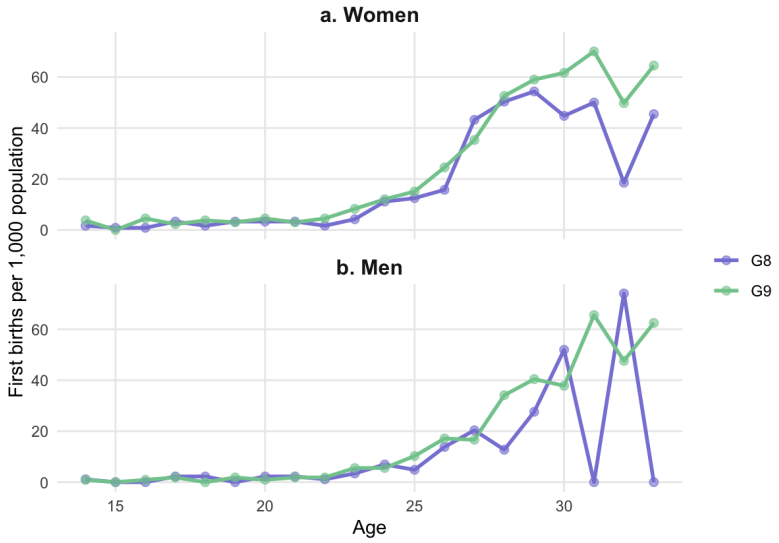
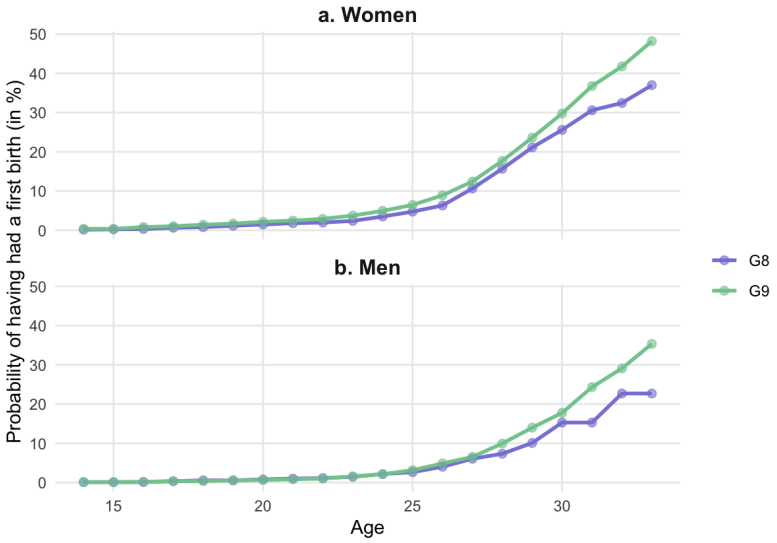
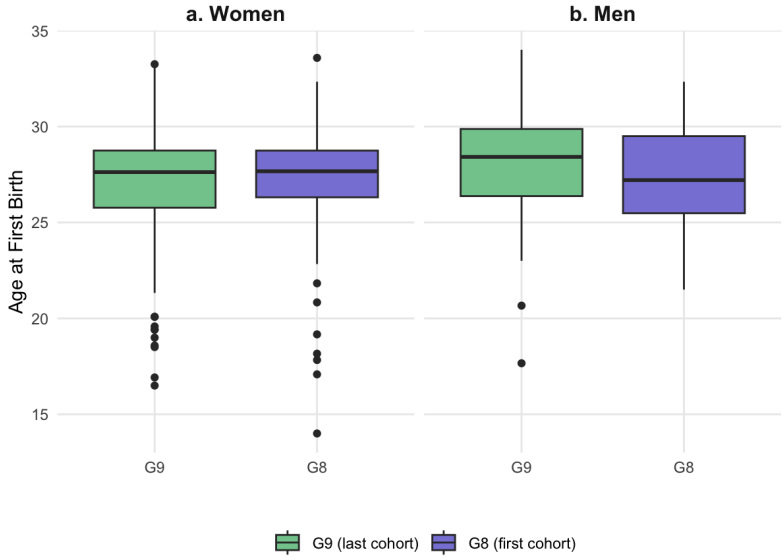


Figure 6: The probability of having had a first birth is lower for both men and women in the G8 cohorts compared to men and women in the G9 cohorts.



To test this descriptive result, I compare the last G9 cohort and the first G8 cohort with a simple t-test, censoring some of the G9 observations as explained above. The results can again be found in Table 1. There are no significant differences between the G8 and the G9 cohort, neither as a whole nor separately for men and for women. The average age at first birth for men in the last G9 cohort is 28.1 compared to 27.3 for the first G8 cohort. For women, the average age at first birth in the last G9 cohort is 26.7, compared to 27.0 for the first G8 cohort. These results are visualized in Figure 7.

Figure 7: There are no significant differences in the average age at first birth between the last G9 cohort and the first G8 cohort.

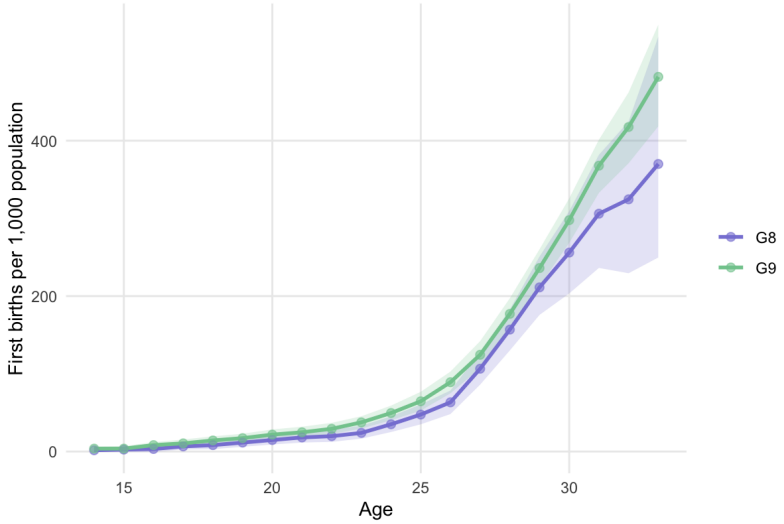


In order to robustly confirm my null result, I turn to a simulation for the women in the last three G9 cohorts and the first three G8 cohorts. I use data from the 2022 German birth statistic for women with Abitur which should be representative of the distribution that my sample is drawn from. The birth statistic gives me the cumulative probability of having had at least a first birth in different age groups. From these, I can generate plausible first birth risks at each age that match the observed cumulative probabilities. I then simulate drawing both groups from this distribution, i.e. exposing each woman in my sample to the first birth risks at each age until she either has a first birth or reaches the highest age at which I observe her.

Repeating this process 1,000 times, I can construct empirical confidence intervals for my estimates by using the 2.5th and 97.5th percentile of the generated distributions at each age. The result is visualized in Figure 8. The difference that I observe in cumulative first births at age 33 is -112 cumulative first births per 1,000 population, i.e. the G8 cohorts having that much lower cumulative fertility. Under the null hypothesis, I observe a difference of -112 or smaller 5.9% of the time. This means that we would not reject the null hypothesis and it is very unlikely that the

true difference is positive, i.e. a higher cumulative fertility for the G8 group.

Figure 8: The G8 women show lower cumulative fertility up until age 33. Shaded areas are empirical 95%-confidence intervals from a simulation.

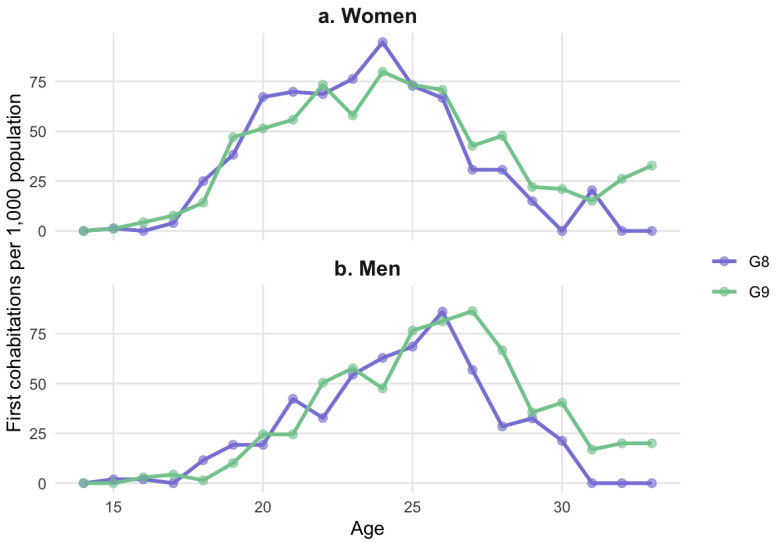


5.3 First cohabitations and first marriages

Having found no effects of the reform on age at first birth, I now turn to first cohabitations and first marriages to see whether other aspects of family formation are affected by the reform.

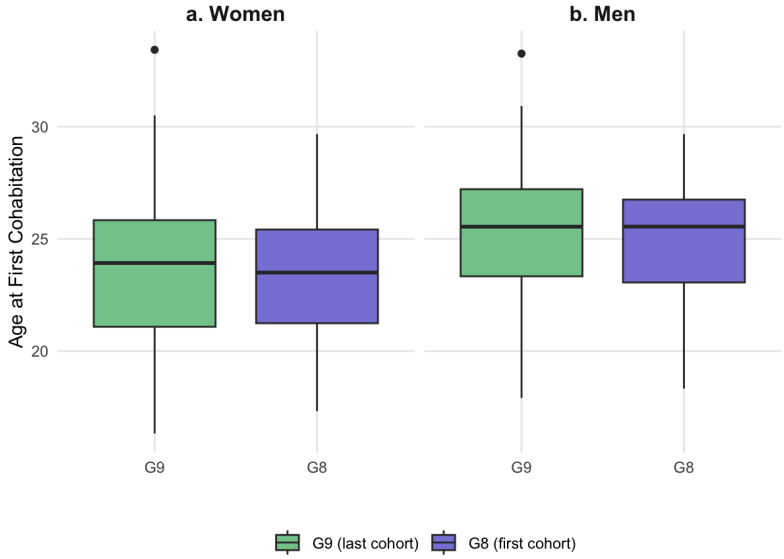
Figure 9 shows the rates of first cohabitation at each age. It seems like there is a very slight leftward shift for the G8 women compared to the G9 women.

Figure 9: There is a slight leftward shift of first cohabitations to earlier ages for the G8 women but no effect for men.



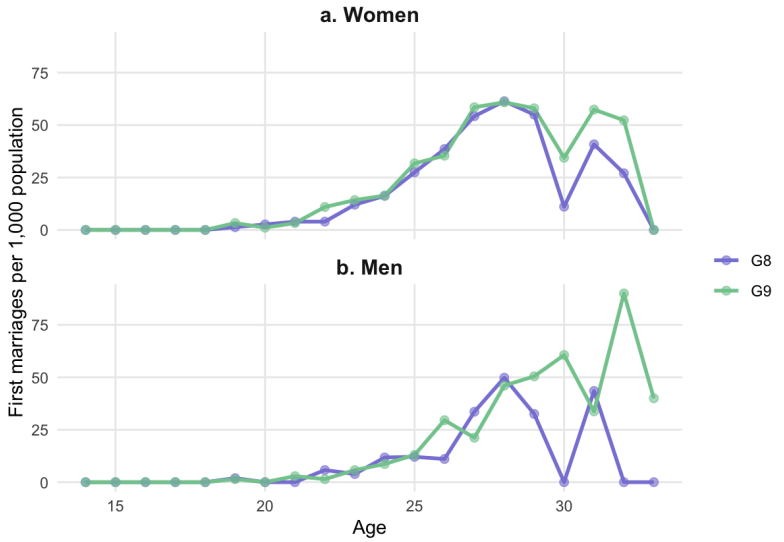
In Table 1 we can see the comparison of age at first cohabitation for the last G9 cohort (again, adequately censored to make the two groups comparable) and the first G8 cohort. We do observe slightly lower ages at first cohabitation for the G8 group but none of the differences is significant. The results are visualized in Figure 10.

Figure 10: There are no significant differences in the age at first cohabitation between the two cohorts.



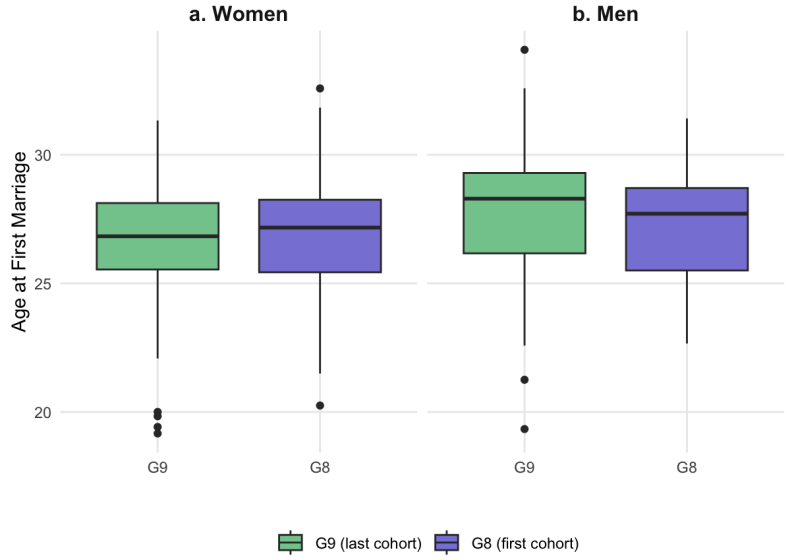
Doing the same exercise for first marriages, Figure 11 shows rates of first marriage by age. There is no leftward shift apparent for the G8 cohort.

Figure 11: No shifts in timing are apparent when comparing first marriages for G8 and G9 graduates, regardless of gender.



In Table 1 we can see the comparison of age at first marriage for the last G9 cohort (again, adequately censored to make the two groups comparable) and the first G8 cohort. We do observe slightly older ages at first marriage for the G8 women and slightly younger ages at first marriage for the G8 men but none of the differences is significant. The results are visualized in Figure 12.

Figure 12: There are no significant differences in the age at first marriage between the two cohorts.



6 Discussion

This paper examines whether shortening secondary education could help address fertility postponement by analyzing Germany's G8 reform. The results show that while the reform successfully reduced the age at highest degree completion by about 6 months, it had no significant effects on family formation timing. Neither age at first cohabitation, age at first marriage, nor age at first birth show meaningful differences between G8 and G9 cohorts. If anything, the evidence points toward slightly lower cumulative fertility in the G8 cohorts with no differences in fertility timing.

The finding that shortening secondary education had no effect on fertility timing,

Table 1: Mean Differences in Life Course Outcomes

	G9 (Last cohort)	G8 (First cohort)	Difference (G8 - G9)
Age at Highest Degree (All)	25.31	24.83	-0.48***
			(0.17)
Age at Highest Degree (Male)	25.85	25.08	-0.77***
			(0.27)
Age at Highest Degree (Female)	24.89	24.67	-0.22
			(0.22)
Age at First Birth (All)	27.14	27.01	-0.13
			(0.41)
Age at First Birth (Male)	28.06	27.25	-0.81
			(0.77)
Age at First Birth (Female)	26.73	26.95	0.22
			(0.48)
Age at First Cohab. (All)	24.21	23.83	-0.38
			(0.23)
Age at First Cohab. (Male)	25.17	24.86	-0.31
			(0.38)
Age at First Cohab. (Female)	23.62	23.39	-0.23
			(0.28)
Age at First Marriage (All)	26.95	26.97	0.02
			(0.32)
Age at First Marriage (Male)	27.72	27.11	-0.61
			(0.64)
Age at First Marriage (Female)	26.54	26.92	0.38
			(0.38)

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Results from t-tests comparing means between the last G9 cohort and the first G8 cohort in all federal states.

despite successfully reducing age at degree completion, has important implications for policy. This section explores potential mechanisms behind this null result and discusses its broader implications for addressing low fertility in high-income countries.

The asymmetric response to changes in educational duration - where increases in education have contributed to delayed fertility but decreases fail to decrease age at first birth - points to the crucial role of social norms. Over decades of educational expansion, societies have developed strong expectations about the appropriate sequence and timing of life events. These norms appear to be sticky downward, resisting adjustment even when the structural constraints that helped shape them (like education duration) are relaxed.

The gradual nature of the G8 reform's implementation may have contributed to this norm persistence. Because the reform was rolled out state by state over several years, treated students remained surrounded by peers who went to school for longer because of social ties across different federal states. This likely reinforced existing norms about the appropriate age for marriage and childbearing, even as treated individuals completed their education earlier.

An alternative explanation lies in changing standards of preparation for parenthood. Recent decades have seen the rise of "intensive parenting" norms that emphasize extensive investment in children's development. Parents today spend significantly more time with their children than previous generations and perceive higher prerequisites - both financial and otherwise - for parenthood. Under this interpretation, students may have used the time freed up by shortened education to better prepare for parenthood rather than accelerate family formation.

The results contribute to a growing body of evidence suggesting that reversing trends toward later and lower fertility may be more challenging than previously thought. While removing structural barriers like lengthy education might seem a promising avenue for pro-natal policy, my findings indicate that such interventions may have limited effectiveness.

An interesting next step would be to look at heterogeneity by federal state. If the gradual implementation did indeed contribute to the reform having no effect

on family formation, we could examine whether those who implemented the reform last, actually do see an effect. I will get access to the full marriage and birth register in Germany soon which means that I will be able to analyze the universe of first marriages and first births, giving me more statistical power and making it possible to analyze the effects separately for each federal state.

One interesting avenue to disentangle the role of education duration and social norms might be to examine fertility outcomes based on both, the federal state where someone went to school and the federal state where someone spent their adult life and had children. Some will have been subject to the reform in their federal state but then move to a federal state where their peers were not exposed, while others will not have been subject to the reform themselves but then move to a federal state where their peers were exposed. Although moving is an endogenous decision and this would not yield causal estimates, it might help shed light on the mechanisms influencing age at first birth.

References

Daniel Aaronson, Fabian Lange, and Bhashkar Mazumder. Fertility transitions along the extensive and intensive margins. *American Economic Review*, 104(11): 3701–3724, 2014.

Eva Beaujouan. Delayed fertility as a driver of fertility decline? In *The Demography of Transforming Families*, pages 41–63. Springer, 2023.

Kathryn Christine Beck, Rannveig Kaldager Hart, and Martin Flatø. School starting age, fertility, and family formation: Evidence from the school entry cutoff using exact date of birth. *Demography*, 61(6):1999–2026, 2024.

Matias E Berthelon and Diana I Kruger. Risky behavior among youth: Incapacitation effects of school on adolescent motherhood and crime in Chile. *Journal of Public Economics*, 95(1-2):41–53, 2011.

Kasey S Buckles and Daniel M Hungerman. Season of birth and later outcomes: Old questions, new answers. *Review of Economics and Statistics*, 95(3):711–724, 2013.

Neil Cummins. The causal effects of education on age at marriage and marital fertility. 2022.

Kamila Cygan-Rehm and Miriam Maeder. The effect of education on fertility: Evidence from a compulsory schooling reform. *Labour Economics*, 25:35–48, 2013.

Margherita Fort, Nicole Schneeweis, and Rudolf Winter-Ebmer. Is education always reducing fertility? Evidence from compulsory schooling reforms. *The Economic Journal*, 126(595):1823–1855, 2016.

Michael Geruso and Heather Royer. The impact of education on family formation: Quasi-experimental evidence from the UK. Technical report, National Bureau of Economic Research, 2018.

Michael Geruso, Melissa LoPalo, and Dean Spears. Age and infertility revisited. Unpublished manuscript, 2023.

Mathias Huebener and Jan Marcus. Empirische Befunde zu Auswirkungen der G8-Schulzeitverkürzung. Technical report, DIW Roundup: Politik im Fokus, 2015.

Mathias Huebener and Jan Marcus. Compressing instruction time into fewer years of schooling and the impact on student performance. *Economics of Education Review*, 58:1–14, 2017.

Jan Marcus and Vaishali Zambre. The effect of increasing education efficiency on university enrollment: Evidence from administrative data and an unusual schooling reform in Germany. *Journal of Human Resources*, 54(2):468–502, 2019.

Tobias Meyer and Stephan L Thomsen. How important is secondary school duration for postsecondary education decisions? Evidence from a natural experiment. *Journal of Human Capital*, 10(1):67–108, 2016.

Tobias Meyer, Heidrun Schneider, and Stephan L Thomsen. New evidence on the effects of the shortened school duration in the German states: An evaluation of post-secondary education decisions. *German Economic Review*, 20(4):e201–e253, 2019.

Máire Ní Bhrolcháin and Éva Beaujouan. Fertility postponement is largely due to rising educational enrolment. *Population Studies*, 66(3):311–327, 2012.

Sandra Schaffner and Andrea Siebert-Meyerhoff. *The Effect of Schooling Age on Fertility*. Number 741. Ruhr Economic Papers, 2017.

Daniël van Wijk and Francesco C Billari. Fertility postponement, economic uncertainty, and the increasing income prerequisites of parenthood. *Population and Development Review*, 2024.

Anson Zhou, Chong Pang, and Samuel Engle. Asymmetric fertility elasticities. *Available at SSRN 4912608*, 2024.

Appendix

Figure 13: Age distribution of the full sample by G8-status and gender.

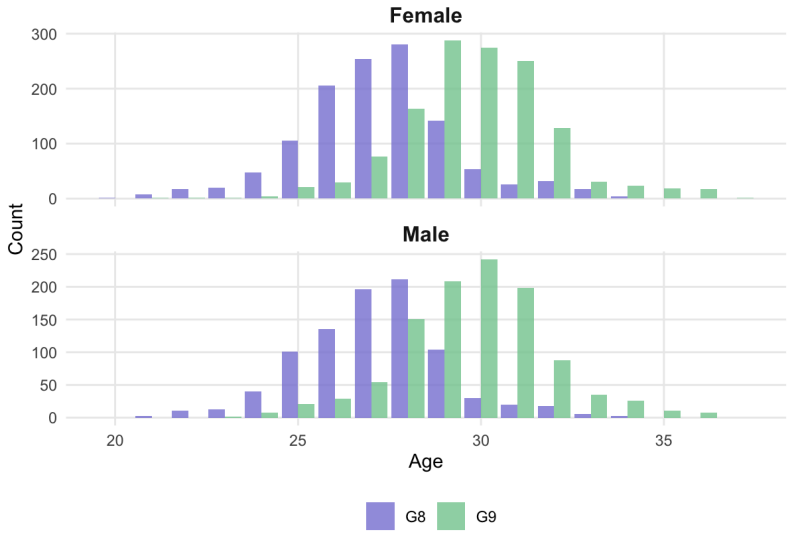


Figure 14: Age distribution of the relationship sample by G8-status and gender.

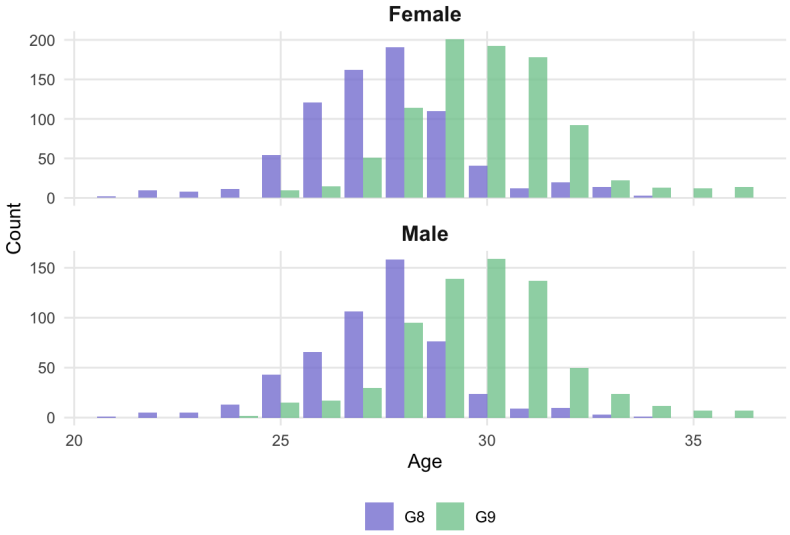


Table 2: Sample sizes by age, G8 status, and gender for the full sample.

	G8			G9		
Age	Total	Male	Female	Total	Male	Female
20	1	0	1	0	0	0
21	11	3	8	1	0	1
22	28	11	17	2	0	2
23	33	13	20	3	2	1
24	87	40	47	12	8	4
25	207	101	106	42	21	21
26	341	135	206	58	29	29
27	450	196	254	131	54	77
28	492	212	280	315	151	164
29	246	104	142	496	208	288
30	84	30	54	517	242	275
31	46	20	26	448	198	250
32	50	18	32	216	88	128
33	24	6	18	66	35	31
34	7	3	4	50	26	24
35	0	0	0	30	11	19
36	0	0	0	26	8	18
37	0	0	0	1	0	1

Table 3: Sample sizes by age, G8 status, and gender for the relationship sample.

	G8			G9		
Age	Total	Male	Female	Total	Male	Female
20	1	0	1	0	0	0
21	11	3	8	1	0	1
22	28	11	17	2	0	2
23	33	13	20	3	2	1
24	87	40	47	12	8	4
25	207	101	106	42	21	21
26	341	135	206	58	29	29
27	450	196	254	131	54	77
28	492	212	280	315	151	164
29	246	104	142	496	208	288
30	84	30	54	517	242	275
31	46	20	26	448	198	250
32	50	18	32	216	88	128
33	24	6	18	66	35	31
34	7	3	4	50	26	24
35	0	0	0	30	11	19
36	0	0	0	26	8	18
37	0	0	0	1	0	1