Relative Age for Grade and Adolescent Risky Health Behavior: Evidence on Abortions and Alcohol Poisonings

Eva Rye Johansen*

July 2020

Abstract

This paper uses Danish register data to examine the effect of relative age for grade on women's abortions and alcohol poisonings. I exploit an administrative rule that creates a discontinuous jump in relative age for grade for children born around January 1. In adolescence, being young-for-grade leads to an increase in the probability and number of abortions and an increase in the probability of an alcohol poisoning. Additional analyses suggest that an advanced educational cycle drives the increase in abortions. The results show how the educational environment shapes women's adolescent risky health behavior.

Keywords: Risky health behavior, school starting age, abortion, drinking

JEL: I12, I21, J13

*Department of Economics and Business Economics, Aarhus University, Fuglesangs Allé 4, 8210 Aarhus V, Denmark. erjohansen@econ.au.dk.

Acknowledgements: I thank John Cawley, Maria Koch Gregersen, Maria Humlum, Michael Lovenheim, Armando Meier, Helena Skyt Nielsen, Anne Birgitte Simonsen, Marianne Simonsen, Nicolas Ziebarth, participants at the 2018 DGPE conference, ESPE 2019, the 2019 Prague workshop on Gender and Family in the Labor Market, the 2018 PhD workshop at Stavanger University and seminar participants at Aarhus University and Cornell University for helpful comments. I gratefully acknowledge that the Carlsberg Foundation (grant CF17-0408) funds the data used in this study.

I. Introduction

Risky behaviors such as unprotected sex and binge drinking peak in adolescence and young adulthood. In 2015, 68.5% of abortions in the U.S. were among 15-30-year-olds (Jatlaoui et al., 2018) and, among drinkers in the U.S. and Europe, heavy episodic drinking was most common for 15-24-year-olds (WHO, 2018). However, these types of risky behavior can have severe health consequences and may have a negative impact on the educational performance and labor market outcomes (for an overview see, Cawley and Ruhm, 2011). Because of this, risky sex and heavy drinking are serious health issues for adolescents and it is a policy goal to decrease the occurrence.

This paper uses full-population, Danish register data to investigate the effect of relative age for grade on adolescent women's abortions and alcohol poisonings; two severe consequences of risky sex and heavy drinking. Differences in relative age for grade originate from the school starting age. The recent literature on school starting age finds that a later school start leads to improved behavior in adolescence when considering crime (Landersø et al., 2017), teenage childbearing (Black et al., 2011; Tan, 2017), mental health (Black et al., 2011; Dee and Sievertsen, 2018) and substance use (Argys and Rees, 2008).[1] Argys and Rees (2008) is the paper closest to mine and I build on their work by considering the effect of relative age for grade on outcomes that capture the consequences of risky sex and heavy drinking while Argys and Rees focus on the behavior (sexual activity and substance use). Furthermore, my analysis consider both short- and medium-term effects while focusing on results between grades. This allows for a deeper understanding of the interplay between the educational cycle, the educational environment and adolescent risky health behavior.

In fact, only few papers look at the effect of educational decisions and policies on risky sex and heavy drinking. Yet, children and adolescents spend a substantial amount of time in school and, thus, education may be an important part of shaping perceptions and attitudes towards such behaviors. Elsner and Isphording (2018) find that a higher achievement rank in high school leads to less risky behavior. Dincer et al. (2014) and Reynoso and Rossi (2019) study policies in Turkey and Argentina, respectively, and also find some support that length and time of day of education matters for risky sex. I contribute to this literature by investigating the effect of relative age for grade; an element of education central to all children in western countries.

[1] The early literature on school starting age focused on the effect on test scores in school (Bedard and Dhuey, 2006; Elder and Lubotsky, 2009) and long-run educational attainment and labor market outcomes (Black et al., 2011; Frederiksson and Öckert, 2014). More recently, Arnold and Depew (2018) considers long-run self-reported health.

I use Danish register data where I combine individual-level information on abortions and alcohol poisonings with information on an individual's education, school peers, births, contraceptive use, cohabitation and chlamydia. I follow the women from age 15 to 30 and consider both if a woman experiences an abortion or alcohol poisoning and the number of times. The register data avoids problems with self-reports, which are often a concern for these outcomes.

To identify a causal effect, I exploit a discontinuity in relative age for grade created by the administrative rule that children should start school in the calendar year they turn seven. I use a large sample of 89,496 women born +/- two months around the cutoff at January 1. Essentially, the rule stipulates that girls born on December 31 are 6.6 years old when starting school whereas girls born the day after, January 1 are 7.6 years old. This gives rise to a fuzzy regression discontinuity design, where I can investigate the effect of relative age for grade on abortions and alcohol poisonings (see e.g. Evans et al. (2010) and Landersø et al. (2017) for papers that use the same approach). I only observe relative age for grade in the 8^{th} grade, but this relates closely to relative age for grade at school start because very few children repeat or skip a grade. I verify this using newer and richer data (see also Landersø et al. 2017 and *forthcoming*, who use the same setup).

The results show that being young-for-grade leads to 3%-point higher probability of an abortion by age 20 (mean 8.3%). However, by age 30, the estimate is smaller and insignificant. Furthermore, being young-for-grade leads to 1.8%-point higher probability of an alcohol poisoning by age 20 (mean 3.1%). By age 30, the estimate for an alcohol poisoning is 1.6%-points, but imprecisely estimated and insignificant. On the intensive margin, the estimates show a significant increase in the number of abortions by age 25, but no effects on the intensive margin of alcohol poisonings.

To understand the results better, I use detailed supplementary data to investigate both the behavior and the channels that relative age for grade may operate through. Using information on births, contraceptive use, cohabitation and chlamydia, I find that the increase in abortions are likely due to more pregnancies, an earlier sexual debut and an earlier involvement in romantic relationships. Furthermore, results for men show that relative age for grade does not affect men's alcohol poisonings, chlamydia, cohabitation and fatherhood. Thus, the behavior and channels that affect women's outcomes does not affect men in a similar way.

Relative age for grade can operate through three channels and I use information on enrollment and completion of education as well as information on school peers to investigate these channels. First, being young-for-grade delays the educational cycle. This may affect abortions and alcohol poisonings if behavior concerning sex and drinking depends on whether you are a senior or junior in high school, and not only whether your biological age is 18 or 17 years old. Such an effect can come from the curriculum, social norms and expectations related to a grade and incapacitation. Social norms and expectations related to a grade may come from school peers' average age in a grade. Incapacitation may come from behavior concerning sex and drinking not only depending on whether you are a senior or junior, but also from being a senior or having graduated.

Second, being young-for-grade implies that a woman is younger compared to her peers. Being younger than peers could lead to more risky sex and heavy drinking because older peers exert a pressure when it comes to drinking and sex. Argys and Rees (2008) use the school starting age rule to investigate this and find that being relatively younger than the peers in a grade lead to more substance use for girls, but have no effect on sexual activity and outcomes for boys.[2] Third, young-for-graders could potentially have lower levels of skills and maturity at school start. The lower level could persist throughout the educational cycle and even grow.

I find that the educational cycle is a likely driver of the results for abortions consistent with the behavior that women form romantic relationships according to grade instead of biological age. Results for the timing of education combined with the timing of abortions and alcohol poisonings does not support an incapacitation effect, while results for the school peers' age do support that social norms and expectations from older peers could cause an effect of the educational cycle. For alcohol poisonings, the channels that drive the increase is less clear.

The results show that the educational environment shapes adolescent risky health behavior. Indeed, women of a similar biological age have different probabilities of an abortion or alcohol poisoning depending on the relative age for grade. Thus, not only biological age drives the peak in risky health behaviors in adolescence, but also an "educational age". As peers may form social norms and expectations about behavior through the educational cycle, an "educational age" does not contrast the literature on peer effects in risky health behaviors (e.g. Card and Giuliano, 2013 and Yakusheva and Fletcher, 2015).

The paper proceeds as follows: Section 2 provides a description of the data and section 3 describes the methods. Section 4 shows the results and section 5 discusses the results and concludes.

[2] Fredriksson and Öckert (2005) and Black et al. (2013) find that peers' age is an unimportant mechanism for achievement, education, labor market outcomes and teenage pregnancies.

II. Data

a. Sample and relative age for grade

This paper uses Danish register data that combines individual level information from education registers with information on abortions and alcohol poisonings from health registers for the entire Danish population. As abortions are only available for women's records, my main analysis focus on women, but Section IV.d shows results for men for the available outcomes. The main sample consists of all Danish women born in the months November through February in the years between November 1981 and February 1992. Information on the full sample of 89,496 women is available until age 25.[3] From age 26, the sample size gradually decreases by dropping the youngest cohort for each additional age.

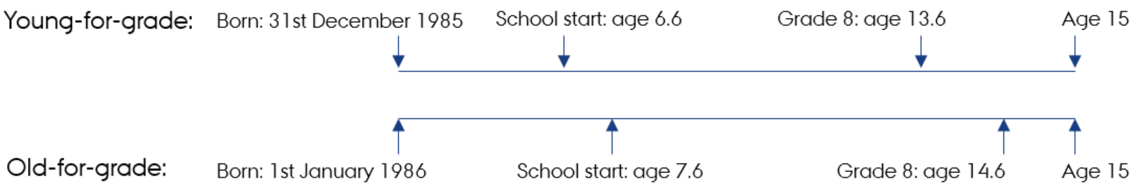
The independent variable of interest is the relative age for grade. A child is supposed to attend school from the calendar year they turn seven, when they start in the 1st grade, and until they have completed the 9th grade. Thus, a later school start leads to the same amount of schooling, but changes the timing of the educational cycle.[4] Parents take the final decision to enroll in school and they can apply to postpone enrollment one year or to enroll a year earlier. Preschool and kindergarten class teachers advise parents regarding the decision of when a child should start school. In practice, the school starting age takes on two values around New Year: 6.6 and 7.6. For my sample, the school starting age de facto becomes a question of relative age for grade and, thus, I refer to the treated and control as young-for-grade and old-for-grade, respectively. Before 1st grade, the vast majority of children attend kindergarten class, and prior to this they typically attend preschool from age three to five. A delayed school start implies an additional year in preschool or kindergarten class.

Information on the relative age for grade comes from the educational enrollment register, but unfortunately, information on the age in a specific grade is only available from the 8th grade. However, this is equivalent to age in the 1st grade because very few children repeat or skip a grade

[3] All outcomes are available until and including 2017 and women born in 1992 turn 25 years old in 2017.
[4] This is contrary to the American context, where minimum school leaving ages imply that a later school start leads to fewer years of education. Thus, results on the effect of a delayed school start are often interpreted as the effect of fewer years of education (see e.g. McCrary and Royer, 2011).

and this is unrelated to being born before or after New Year (Landersø et al., 2017 and *forthcoming)*.[5] I create a binary indicator for being young-for-grade.[6]

Figure 1. Theoretical timing of the educational cycle based on relative age for grade.



Note. Figure shows the theoretical educational cycle for someone who obeys the rule and progresses through the educational cycle without any delays. The horizontal lines show time from birth to age 15.

Figure 1 shows the theoretical educational cycle for a child who is young-for-grade versus a child who is old-for-grade where both comply with the rule. After the 9th grade, women can attend high school or a vocational track. 71% of my sample have started high school at some point by age 20. High school takes three years. After high school, many take gap years before potentially enrolling in further education. Section IV.e.i shows the actual effect of relative age for grade on enrollment as well as completion of various education levels.

Figure 1 highlights the fact that it is important whether I measure the outcomes at a fixed age or a fixed grade. For a fixed age, a different relative age for grade leads to a different amount of education at the time of measurement as well as a different timing of the educational cycle (e.g. high school). For example, at age 15, young-for-graders attend the last half year of the 9th grade and the first half year of high school, whereas old-for-graders attend the last half year of the 8th and the first half year of the 9th grade. Consequently, for each age the effect of being young-for-grade reflects the effect of attending one grade higher. For a fixed grade, a different relative age for grade

[5] I have complete information on the educational cycle from 2007 and I use this to investigate the assumption. Appendix Table A.1 shows the fraction of girls starting school in 2007 and 2008 who repeats a grade (in the case of kindergarten class also those who start late). In addition, Appendix Table A.2 shows results for the first stage across grades for the cohorts 1997 and 2000. The first stage does not differ within a cohort, but only between cohorts. This supports the approximation of using school starting age in the 8th grade. Furthermore, I take a number of steps to ensure the measure is as accurate as possible. I only include pupils attending public school, private or independent schools or boarding schools. Women, who attend other kinds of schools, are likely to have had other than ordinary schooling careers. I only use women who enroll in the 8th grade for the first time in July through September, which are the expected months. Finally, I only use women who have the expected age in the 8th grade as other ages indicate a disruption of schooling at some point.

[6] Measured around New Year. Children born on November 1st are 6.8 and children born on February 28th are 7.4 years old.

leads to a different age.[7] For example, in grade eight young-for-graders are 13.6-14.6 years old, whereas old-for-graders are 14.6-15.6 years old. Thus, young-for-graders are a year younger for each grade and the effect of relative age for grade will reflect the effect of this. In the main results, I use a fixed age and in Section IV.e.i on the mechanisms, I report results for a fixed grade.

In addition, labor market, population, birth and education registers provide information on a range of background characteristics on both the child and the parents. Appendix Table A.3 shows means and standard deviations. I center the background characteristics on the cutoff. In this way, I measure the background characteristics of the women at the same age regardless of them being born in different calendar years.

b. Outcomes

This paper considers two overall outcomes: abortions and alcohol poisonings. The outcomes capture the consequences of risky sex and heavy drinking and are the measures of adolescent risky health behavior that I can accurately observe in the data.[8] The information on abortions comes from The National Patient Register (abortions performed by hospitals) and from The National Health Insurance Service Register (abortions performed by OB/GYN's).[9] Information on alcohol poisonings comes from The National Patient Register. The register data is highly reliable and avoids problems from self-reports. Jones and Kost (2007) estimate that face-to-face interviews in the 2002 National Survey of Family Growth (NSFG) only report 47% of abortions and Tierney (2019) estimates that the Add Health dataset only reports 35% of abortions. Such underreporting is not a concern with the register data.

I define an abortion as an induced abortion.[10] A woman can terminate a pregnancy before the end of the 12th week of pregnancy without giving a reason (the Danish health law, chapter 25). Unmarried women below 18 years of age must have their parents' consent. After the 12th week of pregnancy, a

[7] As I compare individuals who are born at approximately the same time, a different age at measurement implies a different time of measurement (see Figure 1). However, I assume this to be constant between the two adjacent years.
[8] Ideally, I could supplement the analysis with information on sexual activity and drinking, but, unfortunately, this information is not available. Furthermore, I have information on all hospital contacts from 1994-2017. Drug overdoses and injuries are examples of other types of risky health behavior that are available in the register. However, these outcomes are not suitable for the analysis because there are too few drug overdoses (less than 1% of women have had a drug overdose by age 30) whereas injuries often occur before adolescence (61% of women have had an injury by age 15). Results for drug overdoses and injuries are available upon request.
[9] In 2004, OB/GYN's where allowed to perform abortions.
[10] This corresponds to ICD10 codes O04-O06 and abortions performed by OB/GYNs.

woman must apply to the abortion council and state a reason.[11] In the data, 97% of abortions occur in the first trimester.

I define alcohol poisonings as an alcohol-related hospital contact.[12] This covers hospital contacts due to both alcohol poisonings and a range of liver diseases or withdrawal symptoms, but because I only consider ages 15 through 30, 97% of the contacts are alcohol poisonings. Liver diseases and withdrawal symptoms typically occur at older ages after years of addiction. Thus, I refer to the variable as alcohol poisonings. Access to alcohol has become more restrictive throughout my sample period. An age limit of 15 years of age was introduced for buying alcohol in 1998 and this was raised to 16 years of age in 2004 (Datta Gupta and Nilsson, 2020).

I investigate results for both the extensive and the intensive margin of the outcomes. Consequently, I define the outcomes as both a dummy variable that take the value one if an event happens by a given age - this informs about the first time an event happens – and a variable for the number of times an event happens. The number of events includes the cases with zero abortions or alcohol poisonings and, thus, should be interpreted together with the results for the dummy variable.

Figure 2 shows the fraction of women, who have experienced an abortion or alcohol poisoning by a given age separately for women who are old-for-grade and young-for-grade. By age 20, around 8% of women have had an abortion, and by age 30, the fraction is 19%. There is almost no difference between young-for-graders and old-for-graders. Women who are young-for-grade are more likely to have had an alcohol poisoning until age 19, but by age 30, old-for-graders are most likely to have had an alcohol poisoning. More than 4% of women have had an alcohol poisoning by age 30. The empirical analysis investigates if there is a causal effect of being young-for-grade. Appendix Table A.4 shows the means by a given age for the entire sample.[13] In addition, Appendix Figure A.1 shows the probability of an abortion or alcohol poisoning at a given age. The figure shows that abortions and alcohol poisonings are most common in adolescence and early adulthood.

---

[11] Reasons include the woman's health and life situation, dangers to the baby, the baby will be born with a malformation or the pregnancy is the result of an assault. After 18th weeks of pregnancy, very severe reasons must be provided.

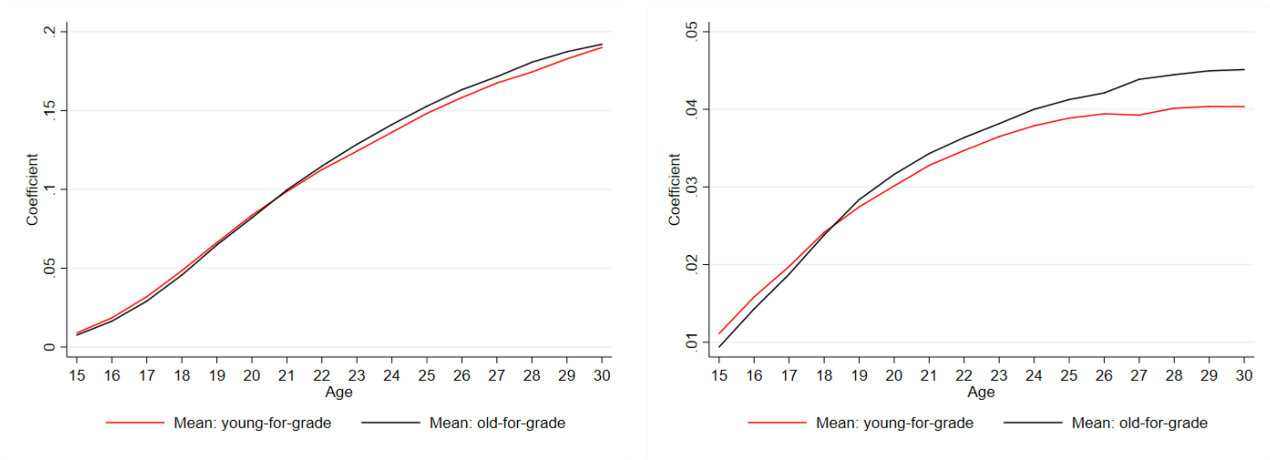
[12] ICD10 codes: All subcategories within F10, K70, T51 and G312, G621, G721, I426, E244, K292, K852, K860, Q860, P043 and O354

[13] The Finnish Institute for health and welfare (2019) report that in 2015 there were 12.4 abortions per 1,000 women aged 15-49 in Denmark. Jatlaoui et al. (2015) report that in the US there were 11.8 abortions per 1,000 women aged 15-44. Per l,000 live births there were 265.5 abortions in Denmark and 188 in the U.S.

Figure 2. Means for an abortions and an alcohol poisonings for old-for-grade and young-for-grade, respectively.

Abortion

Alcohol poisoning



Note. For the ages, 15-25 the sample consists of women born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. Old-for-grade is starting school at age 7.6; young-for-grade is starting school at 6.6.

Appendix Table A.3 shows background characteristics of the women who have had an abortion or alcohol poisoning by age 20 and 30, respectively. Women who have an abortion or alcohol poisoning by age 20 have parents who are less likely to cohabit, have a weaker labor market attachment and a shorter education compared to the full sample of women. The background characteristics are very similar for women who have experienced an abortion or alcohol poisoning by age 30 compared to by age 20.

In addition to the two main outcomes, I have information on a range of supplementary outcomes that shed light on the underlying behavior: births, contraceptive use, chlamydia treatment and cohabitation. Appendix Table A.4 shows the means for these variables. Births comes from The Fertility Register and takes the value one if a woman have conceived a child by a given age (child's birthdate - 280 days). I also combine information on births, abortions and miscarriages in a variable for a pregnancy by a given age. It is not until age 22 that more women have given birth than have had an abortion. Contraceptive use takes the value one if a woman has used hormonal contraception (pills, patches, rings, injections, implants)[14] or has had an IUD inserted (either hormonal or copper) by a given age. Information on hormonal contraception comes from The Register of Medicinal

[14] ATC codes corresponding to: all subcategories within G03AA, G03AB and G03AC as well as G02BB01.

Product Statistics that includes information on purchases of prescription drugs (e.g. date and the anatomical therapeutic chemical (ATC)). [15] I supplement this information with information from The National Health Insurance Service Register on insertion of IUD's by general practitioners and OB/GYNs. Hormonal contraception indicates both risky and non-risky behavior; risky because it does not protect against sexually transmitted diseases but non-risky because it protects against pregnancy. Contraceptive use relates closely to the sexual debut and being in a steady relationship.[16] Interestingly, contraceptive use is mainly a question of timing. By age 15, 30% of the girls have used contraception; by age 30, the number is 96%.

Cohabitation status comes from The Population Register and is available on January 1 of each year from age 17. The alternative to cohabitation is to live with parent(s), alone or with roommates. I also have information on the partner with whom the woman cohabits. By age 30, 86% of women have cohabited. Women who cohabit younger have relatively older partners. The variable for chlamydia comes from The Register for Medicinal Product Statistics and refers to women who have received treatment with the specific dose of the specific type of antibiotics that treats chlamydia.[17] This type of antibiotics can also treat other diseases, but the dose that treats chlamydia is specific to this disease.[18] Chlamydia is more common than abortions. From a prevalence of 1.9% by age 15, it increases to 29.8% by age 25 and then stagnates until age 30.

Appendix Table A.5 shows probabilities for other outcomes conditional on having had an abortion or alcohol poisoning by age 20 or 30. This speaks to the degree of overlap in the different outcomes. There is a much higher probability of experiencing the other outcomes conditional on an abortion or alcohol poisoning. This suggest that part of the outcomes reflect a common risky health behavior. However, none of the outcomes overlaps perfectly. For example, an abortion by age 20 does not translate to 100% probability of chlamydia treatment by age 20. This suggest that an abortion and

[15] For this paper, I only have information on selected ATC-codes relevant for risky health behaviors.

[16] Appendix Figure A.2 shows the relation between the variable for using contraception and the sexual debut. The figure compares the fraction who have had their sexual debut at different ages according to a survey (Knudsen, 2007) to the fraction of women who have used contraception at the same ages according to my data. The correlation is strong. A likely explanation is that as women expect to have sex and/or be in a steady relationship or shortly after, they often try the pill as contraception at least once.

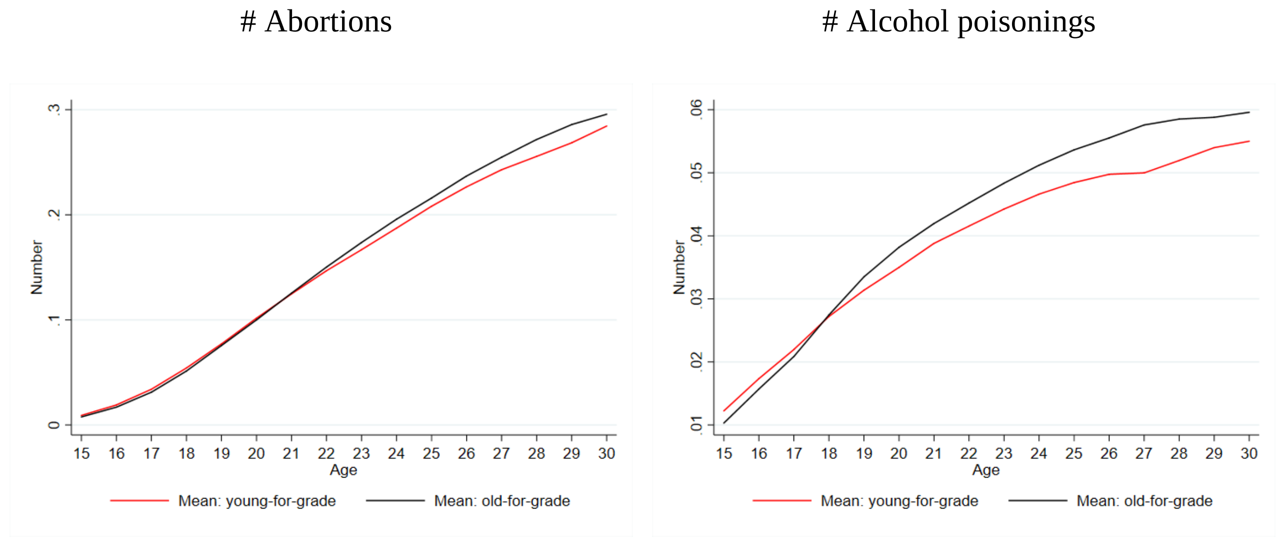
[17] 1000 mg of azithromycin taken in two pills at the same time. Other drugs can also treat chlamydia, but Statens Serum Institut reports that azithromycin is by far the most common.

[18] However, the numbers for chlamydia according to this definition overestimate the occurrence compared with official numbers (according to Statens Serum Institut (2018) there where 32,931 incidences of diagnosed chlamydia in 2017, where my numbers report 41,111). The discrepancy can be because doctors treat patients without confirmation of the diagnosis. On the other hand, other drugs can also treat chlamydia. If relative age for grade does not affect other usages of the drug, IV-regressions still reflect the effect of chlamydia.

chlamydia treatment to some extent capture different kinds of risky health behavior relating to sexual activity. Thus, it is not obvious that results should be similar for the outcomes.

Figure 3 shows the average number of times women have an abortion or alcohol poisoning by a given age. For abortions, averages for young-for-graders and old-for-graders are similar. By age 30, women have had 0.29 abortions. This is higher than the probability of an abortion (19%), which suggests that some women have had more than one abortion by age 30. By age 30, old-for-graders have had more alcohol poisonings than young-for-graders have. This is between 0.05 and 0.06, which is higher than the probability of an alcohol poisoning suggesting that some women have had more than one alcohol poisoning. Appendix Table A.6 shows the average in a table instead of a figure as well as the average number of births, chlamydia treatments and cohabiting partners.

Figure 3. Means for number of abortions and alcohol poisonings for old-for-grade and young-for-grade, respectively.



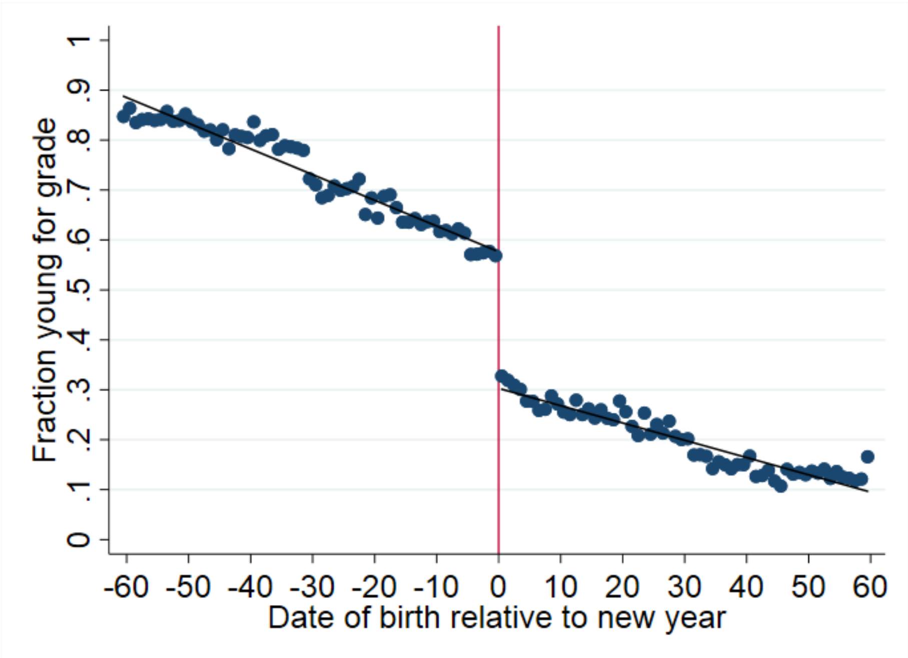
Note. For the ages, 15-25 the sample consists of women born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. Old-for-grade is starting school at age 7.6; young-for-grade is starting school at 6.6.

III. Methods

This paper investigates the effect of relative age for grade on abortions and alcohol poisonings. However, relative age for grade is endogenous because parents choose the school starting age based on factors like maturity that also affect abortions and alcohol poisonings. To circumvent this problem and estimate a plausibly causal effect of relative age for grade, I follow the literature (e.g. Evans et al., 2010 and Black et al., 2011) and instrument relative age for grade with the administrative school starting age that children should start in the first grade in the calendar year they turn seven. If parents comply with the rule girls born in November and December would be 6.6 years old when starting school whereas girls born in January and February would be 7.6 years.

Figure 4 shows the fraction of girls, who start school at age 6.6 (referred to as young-for-grade) for each birthdate from November through February. The fraction does not jump from one to zero. Indeed, some girls born in November and December postpone the school start with a year. This fraction of girls increases as I approach the cutoff and causes a downward slope. Meanwhile, some girls born in January and February start a year earlier. This fraction is decreasing with distance to the cutoff, which causes a downward slope.

Figure 4. Fraction who are young-for-grade by date of birth relative to New Year.



Note. Young-for-grade is starting school at age 6.6 instead of 7.6. The vertical line indicates New Year. The x-axis indicates days relative to New Year. The sample consists of women born from November to February born in the period Nov. 1981 – Feb. 1992.

This setting gives rise to a fuzzy regression discontinuity design. In the first stage, I estimate the following equation for woman *i*:

$$Youngforgrade_i = \alpha_1 + \beta_1 cutoff_i + +g_1(day_i) + \gamma_1 X_i + u_i \tag{1}$$

Where $youngforgrade_i$ is a dummy variable that takes the value one, if a woman starts school at age 6.6 and $cutoff_i$ is a dummy variable that takes the value one if a woman is born in January or February. $g_1(day_i)$ is a linear trend with distance to the cutoff measured in days that varies around the cutoff and X_i is background characteristics. u_i is an idiosyncratic error term.

In the second stage, I regress the following equation for woman *i*:

$$Y_i = \alpha_2 + \beta_2 youngforgrade_i + g_2(day_i) + \gamma_2 X_i + \epsilon_i \qquad (2)$$

where Y_i is the outcome, which is a measure of abortions or alcohol poisonings. $youngforgrade_i$ is relative age for grade, which equation (1) instruments. $g_2(day_i)$ and X_i is the linear trend and background characteristics from equation (1), respectively. ϵ_i is an idiosyncratic error term.

This strategy involves choosing the appropriate bandwidth and control function for distance to the cutoff. Ideally, I could use a bandwidth with observations only one month or less away from either side of the cutoff. However, only very few women experience abortions and alcohol poisonings during adolescence. Thus, restricting the bandwidth to only +/- one month leads to large standard errors and, therefore, even very large estimates relative to the mean become insignificant. Instead, I choose a bandwidth of +/- two months around the cutoff. For this bandwidth, the covariates still balance at standard significance levels. This is no longer the case for +/- three months. Appendix Table A.9 shows results for the alternative bandwidths as well as results for the MSE optimal bandwidth with robust confidence bands following Calonico et al. (2014).

Several elements can affect the outcome and relative age for grade in a continuous way around the cutoff, but it is assumed that such elements are adequately controlled for through a function of distance to the cutoff. In my primary specification, I control for distance to the cutoff in days and allow this linear trend to vary on either side of the cutoff. Appendix Table A.10 reports results for a quadratic control function. The estimates do not change much, but the standard errors increase.

It is an important assumption that individuals are similar around the cutoff. Hence, I conduct a balance test, where I regress the instrument on the background characteristics. The F-statistic for joint significance of the background characteristics is 1.38 and the p-value is 0.15. Appendix Table A.11 shows the coefficients for the individual characteristics from the joint regression as well as results for separate regressions of a characteristic on the instrument. As an additional test, I compare results both with and without control variables. In the case of differences around the cutoff, I will expect the results to change when adding control variables, but this is not the case. Results without control variables are available upon request.

It is problematic if parents manipulate whether children are born on either side of the cutoff. In Appendix Figure A.3, I inspect a histogram of births around New Year. The figure shows an irregularity, because planned C-sections are not performed on holidays such as January 1 and Christmas.[19] However, this should not affect my results because C-sections should be unrelated to the school starting decision. As a test of this, I perform Donut regressions, where I exclude +/- 1 day around New Year and +/- 7 days. Appendix Table A.12 shows the results. The estimates for abortions remain unchanged, but the estimates for alcohol poisonings become smaller and insignificant.[20]

A final assumption is monotonicity. The assumption holds if no parent defies the school starting age rule regardless of the child's birthdate. This happens if a parent always applies for an exemption to the rule – e.g. if a parent has their child enrolled before the rule states when the child is born in January or February and later than the rule states when the child is born in December or November (Landersø et al., 2017). This is not likely, and, thus, the assumption likely holds.

IV. Results

a. First stage

Table 1 shows the regression results from the first stage. All regressions include controls for birth year centered on the cutoff (as opposed to calendar year) and distance to the cutoff. The table shows results both with and without controls for child and parent background characteristics. Being born January 1 or later leads to 27%-point lower probability of being young-for-grade. The estimate is highly significant. The control variables do not change the result.

[19] Contrary to the U.S., there is no large tax benefits of having children on either side of January 1 (for the effects in the U.S., see Dickert-Conlin and Chandra (1999)).

[20] The gestational length could correct for this but, unfortunately, this is only available in weeks and therefore cannot be used as an alternative instrument.

Table 1. Results for the first stage.

	Outcome: Young-for-grade = 1	
Born in January or February = 1	-0.27***	-0.27***
	(0.01)	(0.01)
Observations	89,496	89,496
R-squared	0.32	0.34
Controls	NO	YES
Distance to cutoff	YES	YES

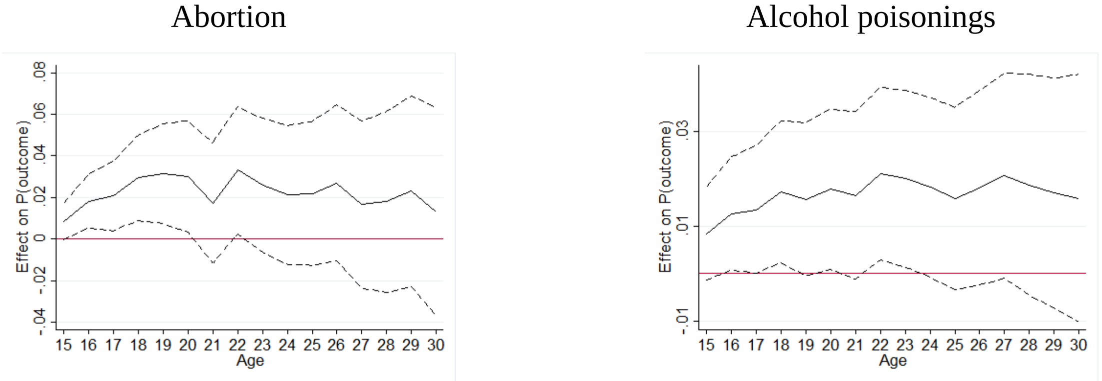
Note. The sample consists of women born November through February in the period Nov. 1981 - Feb. 1992. Variables for the father or mother are measured the year prior to childbirth. Table A.3 shows the list of variables. The regressions include controls for missing variables and centered birth year. * p<0.1, ** p<0.05, *** p<0.01.

b. Main outcomes

i. Probability of abortions and alcohol poisonings

Figure 5 shows the main estimation results from IV-regressions. The solid line shows the effect of being young-for-grade on the probability of an abortion or alcohol poisoning by a given age from 15 to 30. Appendix Table A.7 shows the estimates corresponding to the graphs. Appendix Figure A.4 shows the reduced form scatterplots for age 18.

Figure 5. Results across ages for abortion and alcohol poisonings. Outcomes measured by a given age.



Note. Figure shows estimates from IV-regressions of outcome measured by a given age on young-for-grade instrumented by being born January 1 or later. For the ages 15-25, the sample consists of women born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. The dashed lines indicate the 95% confidence bands.

The positive estimates for abortions in Figure 5 show that being young-for-grade leads to an increase in the probability of an abortion. The figure shows that by age 20, being young-for-grade leads to a statistically significant 3%-point higher probability of an abortion. However, by age 30, the estimate is insignificant and shows a 1.3%-point increase in abortions. The economic size of the estimates decreases considerably from age 15 to 30. By age 15 and 16, the estimate for being young-for-grade corresponds to 100% of the mean. By age 20, the estimate is 36% of the mean and by age 30, it is 7%. Thus, the results suggest that being young-for-grade causes a temporary increase in abortions, but the effect fades out in the long run and only an insignificant positive effect remains by age 30.

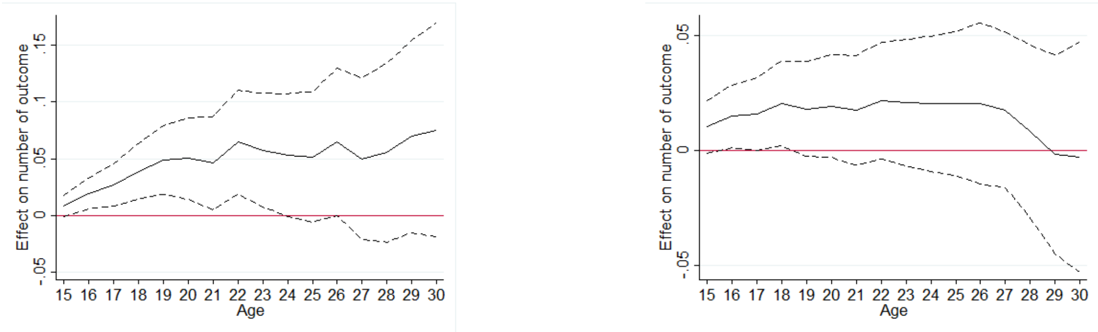
Figure 5 also shows a positive effect of being young-for-grade on alcohol poisonings. By age 20, being young-for-grade leads to a statistically significant 1.8%-point increase in the probability of an alcohol poisoning. By age 30, the increase is 1.6%-point but insignificant. The economic size of the estimates is large. The estimate of being young-for-grade corresponds to 58% of the mean by age 20. From age 20, the probability of an alcohol poisoning does not increase much and, thus, the economic size of the estimate is still large by age 30; it is 37% of the mean. However, even though the economic size of the estimate is large by age 30, I lose substantial precision with the smaller sample size for older ages. Thus, it is difficult to make conclusions about the long-run effect of being young-for-grade.

Appendix Table A.3 showed that women who experienced the first event by age 20 were similar to women who experienced the first event by age 30. Thus, the estimates for different ages do not reflect that relative age for grade affect different women at different ages. Instead, the estimates reflect that relative age for grade have a different effect over the educational cycle. I will address this further in Section IV.e.i on the mechanisms. In addition, analyses of heterogeneous effects show that women with a low socio-economic background drive the results. This is also the group of women who is most likely to experience an event. The results are available from the author upon request.

ii. Number of abortions and alcohol poisonings

Figure 6. Results across ages for abortion and alcohol poisonings. Outcomes measure number of events by a given age.

Abortion Alcohol poisonings



Note. Figure shows estimates from IV-regressions of outcome measured by a given age on young-for-grade instrumented by being born January 1 or later. For the ages 15-25, the sample consists of women born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. The dashed lines indicate the 95% confidence bands.

It is not only of policy interest to lower the number of women who experience an abortion or alcohol poisoning, but also to lower the number of abortions and alcohol poisonings. Among women who have an abortion by age 30, 31% have more than one abortion. For alcohol poisonings, the number is 19%. However, Figure 5 does not address whether being young-for-grade has an effect on subsequent abortions and alcohol poisonings. For that reason, Figure 6 shows figures where the outcome is the number of abortions or alcohol poisonings by a given age. The outcomes include women with zero abortions or alcohol poisonings. Thus, Figure 6 should be interpreted together with Figure 5. Appendix Table A.8 shows the estimates in a table instead of a figure.

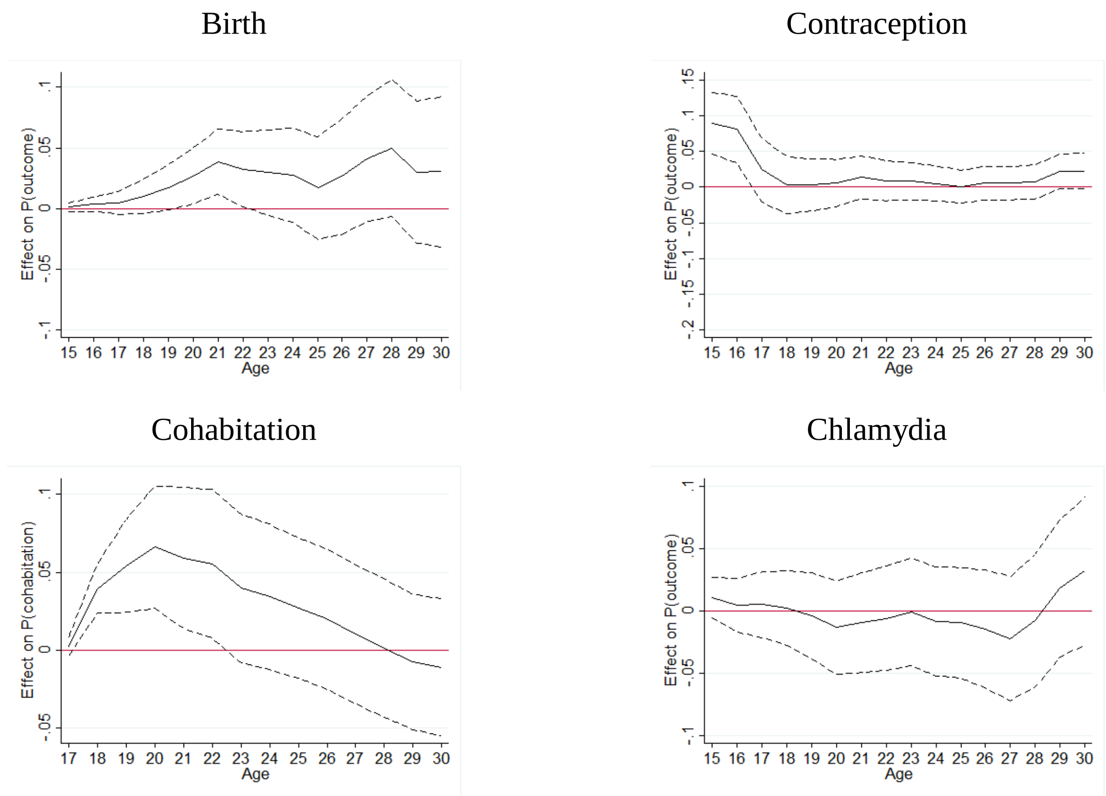
Figure 6 for abortions shows that being young-for-grade leads to an increase of .05 abortions by age 20. This estimate is larger than the .03 for the dummy variable. Thus, being young-for-grade leads to an additional increase in subsequent abortions. Interestingly, the estimate becomes more positive after age 20 and it is still statistically significant by age 25. By age 30, being young-for-grade leads to .075 more abortions, but the estimate is imprecise and insignificant. However, until age 25, Figure 6 shows an effect of being young-for-grade on the intensive margin of abortions with increasing subsequent abortions.

The group of women who have more than one alcohol poisoning is smaller than the group who have more than one abortion. Figure 6 for alcohol poisonings shows that the estimates for the number of alcohol poisonings are almost similar to the estimates for the dummy variable. Thus, there is no effect of being young-for-grade on subsequent alcohol poisonings.

c. Supplementary outcomes for understanding effects on abortions

To understand the results for abortions better, I investigate the effect of being young-for-grade on a range of outcomes related to abortions. Specifically, I consider births, contraception, cohabitation and chlamydia. Births inform on the choice between an abortion and giving birth and contraception and cohabitation relates to sexual activity and relationships. Chlamydia shows whether the results for abortions transfer to another consequence of risky sex.

Figure 7. Results across ages for birth, contraception, cohabitation and chlamydia. Outcomes measured by a given age.



Note. Figure shows estimates from IV-regressions of outcome measured by a given age on young-for-grade instrumented by being born January 1 or later. For the ages 15-25, the sample consists of women born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. The sample is smaller for chlamydia for age 15 to 17 as the data is only available from 1999. The age for birth refers to birthdate of the first child - 280 days, which is the approximate time of conception. The dashed lines indicate the 95% confidence bands.

Figure 7 shows no significant effect on births by age 30 and a significantly positive effect between ages 19 to 22. Thus, the increase in abortions is due to more pregnancies and not due to a change in the choice between an abortion and giving birth. Appendix Figure A.5 shows the effect of being young-for-grade on an indicator for a pregnancy and confirms this result.

Results for contraception in Figure 7 show an 8.9%-point increase in contraceptive use by age 15 and an 8%-point increase by age 16. The estimates are statistically significant. By age 17, the estimate is smaller and insignificant and by age 18, the estimate is virtually zero. The estimates for age 15 and 16 are large in magnitude and correspond to 30% and 16% of the mean. To the extent that the increased contraceptive use reflects an increase in sexual activity, this can explain the large effect sizes for abortions by age 15 and 16. If being young-for-grade increases sexual activity enough to try hormonal contraception at least once, being young-for-grade is also likely to lead to more abortions. In this context, effect sizes of 100% of the mean are reasonable.

If women are in relationships, they are likely to be more sexually active, which increases the risk of abortions. Thus, if being young-for-grade leads to more women being in relationships this can lead to more abortions. I do not observe relationships in the data, but I do observe cohabitation. Figure 7 shows that being young-for-grade leads to a significantly higher probability of cohabitation until age 22. The increase in cohabitation until age 22 supports that relationships drive the increase in abortions until age 20. Furthermore, Appendix Figure A.5 shows results for the first partner's age relative to the woman's age. The point estimates show that being young-for-grade leads to an older partner for age 17 and 18 conditional on cohabiting, but the estimates are not statistically significant due to the few women who cohabit in the teenage years.

Chlamydia is most common in adolescence (see Appendix Figure A.6) and is another consequence of risky sex. Results for chlamydia in Figure 7 shows no significant effect of being young-for-grade. The null-effect suggests that the sexual behavior that causes chlamydia is different from the sexual behavior that causes abortions. This is consistent with the fact that chlamydia and abortions only overlapped partially in Appendix Table A.5. Consequently, the same interventions will not necessarily affect both abortions and chlamydia.

To sum up, the analysis of the additional outcomes shows that the increase in abortions is due to more pregnancies. The increase in pregnancies is likely due to an earlier involvement in romantic relationships and an earlier sexual debut. The positive effect of being young-for-grade on abortions does not transfer to chlamydia, another consequence of risky sex.

d. Men

The main analysis focuses on the effects for women because abortions and contraceptive use are only available for women. However, fatherhood (for women: birth), alcohol poisonings, cohabitation and chlamydia treatment are also available for men. Appendix Table A.13 shows the means for these outcomes for men. Men are more likely to have experienced an alcohol poisoning by a given age than women, but less likely to have been treated for chlamydia. Men are also less likely to have a child and cohabit by a given age than women.

Figure 8. Results across ages for fatherhood, alcohol poisonings, cohabitation and chlamydia for men. Outcomes measured by a given age.

Note. Figure shows estimates from IV-regressions of outcome measured by a given age on young-for-grade instrumented by being born January 1 or later. For the ages 15-25, the sample consists of men born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. The sample is smaller for chlamydia for age 15 to 17 as the data is only available from 1999. The age for births refers to birthdate of the first child - 280 days, which is the approximate time of conception. The dashed lines indicate the 95% confidence bands.

Figure 8 shows the effect of being young-for-grade on the outcomes for men. For fatherhood, alcohol poisonings and chlamydia the estimate for being young-for-grade is close to zero and statistically insignificant for all ages. For cohabitation, being young-for-grade leads to a statistically significant higher probability of cohabitation by age 20, but for all other ages the estimate is close to zero and insignificant. This shows that relative age for grade is not a determinant of adolescent risky health behavior for men and that the behavior and channels that affect women do not affect men in a similar way. This is consistent with Argys and Rees (2008) who do not find an effect for boys.

e. The channels relative age for grade operate through

The effect of being young-for-grade can operate through three channels: an advanced educational cycle, having older peers and school starting age in itself. In this section, I address these channels. However, I caution that disentangling the effect of being young-for-grade into separate channels is difficult.

i. Effects of the educational cycle

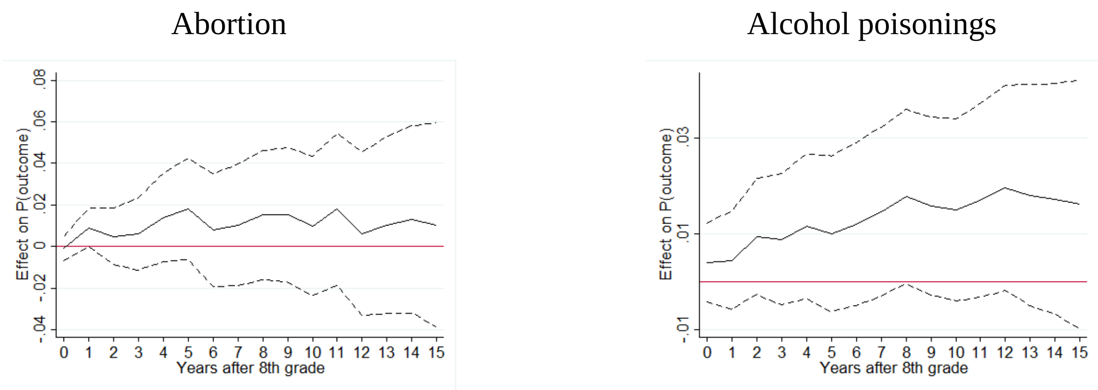
In this subsection, I investigate the effect of the educational cycle. First, I show the effect of relative age for grade on enrollment and completion of different types of education. Second, I address the educational cycle as a channel for the effect of relative age for grade on abortions and alcohol poisonings. Appendix Figure A.7 shows the effect of being young-for-grade on the probability of enrollment into high school and further education at each age[21]. The figure shows that being young for grade advances the timing of enrollment. Furthermore, results for enrollment into any education shows that being young-for-grade leads to a lower probability of enrollment into any education at all ages except at age 21 and 22. This is consistent with the fact that completion of an additional grade increases the probability of having finished an education. The higher probability of enrollment at ages 21 and 22 corresponds to the ages where young-for-graders start enrolling in further education, whereas old-for-graders still have gap years between high school and further education. Appendix Figure A.8 shows that by age 30 relative age for grade have no statistically significant effect on completion of high school or further education. This is consistent with the

[21] Notice that I define age at enrollment from a given birthdate. As the women are born November through February and the school year runs from August to July the women are often enrolled half a year before their birthdate and half a year after.

theoretical prediction (Figure 1) and the previous literature (Black et al. (2011), Frederiksson and Öckert (2014), Landersø et al. (2017)).[22]

If the educational cycle truly drives the results for abortions and alcohol poisonings, then measuring the outcomes in grade instead of age should nullify the effect. Thus, the solid line in Figure 9 shows the effect of being young-for-grade on abortions or alcohol poisonings by grade 8, grade 9 etc. (see also Figure 1). The figure shows a positive, but insignificant effect on abortions. Indeed, at no point from grade 8 and up to 15 years after is there a significant effect of being young-for-grade. One explanation is that when I eliminate effects from the educational cycle being young-for-grade have no effect on abortions. However, one should note that large positive point estimates persist and that the results measured in age and grade are not significantly different. Figure 9 also shows a positive effect of being young-for-grade on alcohol poisonings. For some years after grade 8, the estimates are significant at the 10% level. Thus, the educational cycle does not appear to be the driver of the positive effect on alcohol poisonings.

Figure 9. Results across grades for abortion and alcohol poisonings. Outcomes measured by years after 8th grade.



Note. Figure shows estimates from IV-regressions of outcome measured by a given number of years after 8th grade on young-for-grade instrumented by being born January 1 or later. For the ages 15-25, the sample consists of women born Nov. 1981-Feb. 1992. From age 25, each additional year drops the latest cohort. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. The dashed lines indicate the 95% confidence bands.

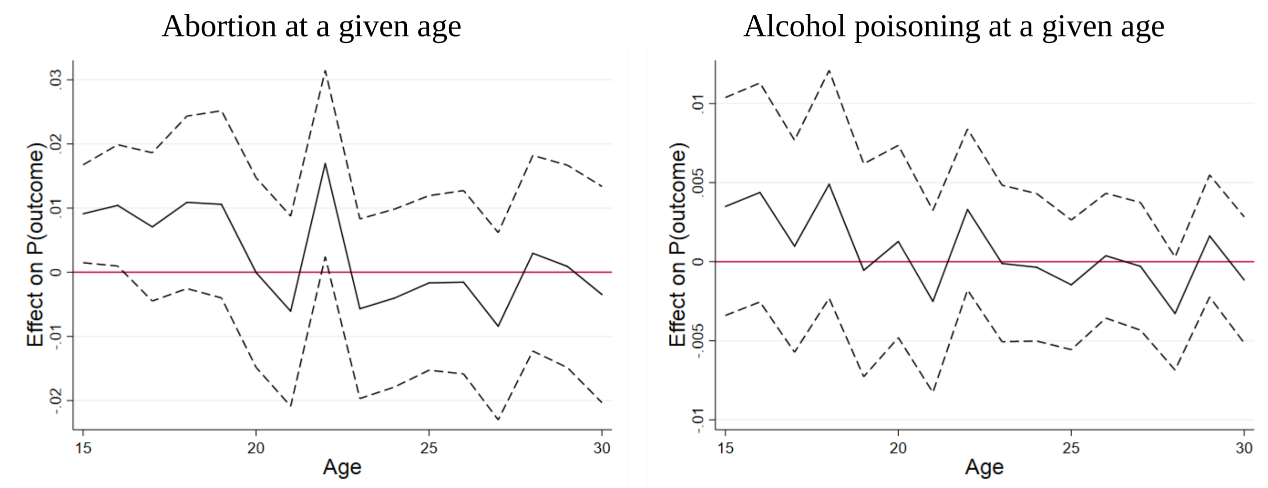
Appendix Figure A.9 shows similar results for births, contraception, cohabitation and chlamydia. Interestingly, for contraception, cohabitation and chlamydia being young-for-grade now leads to a

[22] Bedard and Dhuey (2006) finds that an earlier school start leads to worse performance in school. Elder and Lubotsky (2009) find similar results, but highlights that the effects fades throughout school. Thus, effects of relative age for grade on school performance are likely small in adolescence.

significantly negative effect for some years after grade 8. The explanation is that being young-for-grade now implies that a woman is a year younger in a given grade. Thus, the negative effect reflects the fact that biological age is a strong determinant of contraceptive use and cohabitation up to 5 and 7 years after grade 8, respectively, and a determinant of chlamydia 4 to 7 years after grade 8.

The educational cycle may work through an incapacitation effect. On the one hand, enrollment into education reduces time and increases opportunity costs of engaging in risky health behaviors. On the other hand, classmates provide a peer group with whom to interact. To investigate the incapacitation effect, Figure 10 shows results for abortions and alcohol poisonings measured at a given age. In contrast to the dummy variable for an event by a given age, this measure is memoryless and includes repeated events. Thus, the figure shows the immediate effect on an outcome from changes in enrollment patterns created by the relative age for grade. For both abortions and alcohol poisonings, being young-for-grade leads to a higher probability of an event at a given age until around age 19. The estimate then approaches zero except from at age 22, which have a positive effect. The pattern is not consistent with the enrollment pattern from Appendix Figure A.7 and, thus, does not suggest an obvious incapacitation effect.

Figure 10. Results across ages for abortion and alcohol poisonings. Outcomes measured at a given age.



Note. Figure shows estimates from IV-regressions of indicator for abortion or alcohol poisoning AT a given age on young-for-grade instrumented by being born January 1 or later. The sample consists of women born Nov. 1981-Feb. 1992. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. The dashed lines show the 95% confidence bands.

ii. Peers' age

Another potential mechanism is older peers. Appendix Figure A.10 shows the distribution of grade 8 peers' average age separately for young-for-graders and old-for-graders.[23] There are two aspects to peers' age. One is an absolute effect where peers' absolute age forms social norms and expectations. Another is a relative effect, where the age relative to the peers matters. To investigate peers' age, I follow a strategy similar in spirit to Black et al. (2013) and add the peers' average age as an additional explanatory variable in equation (2). I instrument peers' average age with the average expected age according to the school starting age rule.[24],[25]

Appendix Table A.15 shows the results for regressions that include peers' average age. For abortion by age 20, the table shows that increasing the peers' average age with 0.1 years leads to 0.51%-point higher probability of an abortion. The estimate is larger and statistically significant for age 18 and 19. The estimate speaks to peers' absolute age. The coefficient on young-for-grade does not change much. This is as expected because the estimate still reflects the effect of being the youngest versus the oldest in a grade. Given the narrow sample around the threshold, there is very little variation in the age relative to peers conditional on relative age for grade.

For alcohol poisoning by age 20, the table shows that an increase in peer's average age with 0.1 years leads to 0.29%-point lower probability of an alcohol poisoning. This suggest that older peers leads to fewer alcohol poisonings.

To sum up, peers' average age appears to have an effect on abortions, but not on alcohol poisonings. This is not contrary to the prior evidence in favor of the educational cycle. More, it suggests that part of effects of the educational cycle could come from the peers' age.

---

[23] I use the peers in grade 8 because peers in high school or further education are missing for women not enrolled in education. Furthermore, grades are much larger for these types of education.

[24] I use peers born in all parts of the year and, thus, the expected school starting age (ESSA) rule is equivalent to ESSA = 7.6-(birthmonth-1)/12.

[25] This is a valid instrument if women do not select into a school based on the time of the year the peers are born. The F-statistic for joint significance of a woman's background characteristics with the average expected school starting age have a p-value of 0.04. However, the results are robust to including school fixed effects.

V. Discussion and conclusion

This paper investigates the effect of relative age for grade on abortions and alcohol poisonings in adolescence and early adulthood. I exploit exogenous variation in relative age for grade from a rule stating that children should start school in the calendar year they turn seven. I find that being young-for-grade leads to a 3%-point higher probability of an abortion by age 20 (36% of the mean), but by age 30 the estimate is smaller and insignificant. Thus, the results suggest only a temporary increase in abortions. For alcohol poisonings, there is a significant 1.8%-point increase by age 20 (58% of the mean). By age 30, the increase is 1.6%-points, but imprecisely estimated and insignificant.

Furthermore, I find that being young-for-grade leads to a significant increase in the number of abortions by age 25. Together with the results for the probability of an abortion, the results show an effect of being young-for-grade on both the extensive and intensive margin of abortions. Effects on the extensive margin are present until age 20, whereas effects on the intensive margin are present until age 25. There is no effect of being young-for-grade on the intensive margin of alcohol poisonings.

I shed further light on the results for abortions by investigating effects on births, contraception, cohabitation and chlamydia. I find that the increase in abortions is due to more pregnancies. The increase in pregnancies is likely due to an earlier sexual debut and an earlier involvement in romantic relationships. These results align with previous studies that find that a later school start leads to fewer teenage births (Black et al., 2011) and postpones the timing of childbirths and marriage with no or small effects on completed fertility and probability of ever marrying (Skirbekk et al., 2004). Contrary to this, McCrary and Royer (2011) find no effect of the school starting age on fertility in the U.S, where women who drop out of school drive the effects. I show that this is not the case in my setting, which may explain the different results.

Interestingly, I find that relative age for grade does not affects men's alcohol poisonings, chlamydia, cohabitation and fatherhood in a similar way as for women. Thus, the behavior and channels that drive results for women do not carry over to men. I also find that there is only a partial overlap between abortions and chlamydia and the results for abortions do not carry over to chlamydia. One explanation is that different sexual behavior drives chlamydia and abortions. Indeed, chlamydia is more likely if you have sex with more partners whereas abortions are more likely if you have sex regularly. Regular sex happens within a steady relationship, where chlamydia is less common.

While the strong credibly causal design of the paper, the register data and the many outcomes to address women’s sexual behavior are strengths, it is a limitation that I cannot separate the individual channels more. This is a general disadvantage of the setup where the treatment consists of several elements. However, I do provide evidence to suggest that abortions are driven by the educational cycle whereas the channels for alcohol poisonings are less clear.

The paper most similar to this is Argys and Rees (2008) who find that being young relative to your peers has no effect on women’s sexual behavior and a positive effect on drinking. At first, Argys and Rees’ results might appear to contradict my results, but Argys and Rees’ compares outcomes within a grade and not within an age. Thus, their results should be compared to my results in Figure 9. In this part of my analysis, I do not find an effect of being young-for-grade on abortions and a borderline significant increase in alcohol poisonings. This is more similar to Argys and Rees (2008).

In recent years in the US, there has been a trend towards delaying the school start (Demming and Dynarski, 2008). My results support this decision because I show that a later school start leads to fewer abortions and alcohol poisonings in adolescence. This is important for parents who do not only care about the educational outcomes of their children but also for their children’s health and well-being.

For policies, the results show that not only biological age, but also relative age for grade is a determinant of the peak in risky sex and heavy drinking in adolescence. In that spirit, politicians may consider giving more attention to forming policies and goals based on the grade and not only biological age. However, it is also important to highlight that my analysis only sheds light on an individual’s decision regarding relative age for grade. Indeed, it does not inform on the consequences of changing the school starting age for all, because, in this case, relative age for grade stays constant for all children.

References

Argys, L.M., and D.I. Rees. (2008): Searching for Peer Group Effects: A Test of the Contagion Hypothesis. *Review of Economics and Statistics.* 90(3). 442–58.

Arnold, G., B. Depew (2018): School starting age and long-run health in the United States. *Health Economics*. 27. 1904-1920.

Bedard, K. and Dhuey, E. (2006): The persistence of early childhood maturity: international evidence of long-run age effects. *Quarterly Journal of Economics.* 121(4). 1437-1472.

Black, S.E., Devereux, P.J. and Salvanes, K.G. (2011): Too Young to Leave the Nest? The Effects of School Starting Age. *Review of Economics and Statistics*. 93(2). pp. 455-467.

Black, S.E., Devereux, P.J. and Salvanes, K.G. (2013): Under pressure? The effect of peers on outcomes of young adults. *Journal of Labor Economics.* 31(1). 119–153.

Calonico, S., Cattaneo, M.D. and Titiunik, R. (2014): Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*. 82(6). 2295-2326.

Card, D. and Giuliano, L. (2013): Peer Effects and Multiple Equilibria in the Risky Behavior of Friends. *Review of Economics and Statistics*. 95(4). 1130-1149.

Cawley, J. and C. Ruhm. (2011): The Economics of Risky Health Behaviors. In *Handbook of Health Economics*. Vol. 2. ed. Anthony J. Culyer and Joseph P. Newhouse. 95–199. New York: Elsevier.

The Danish health law, https://www.retsinformation.dk/Forms/R0710.aspx?id=199871

Datta Gupta, N and Nilsson, A. (2020): Legal drinking, injury and harm: Evidence from Denmark. Working paper.

Dee, T. and Sievertsen, H.H. (2018): The Gift of Time? School Starting Age and Mental Health. *Health Economics*. 27. 781-802.

Deming, D. and S. Dynarski (2008): The Lengthening of Childhood. *Journal of Economic Perspectives*. 22(3). pp. 71-92.

Dickert-Conlin, S. and A. Chandra (1999): Taxes and the Timing of Births. *Journal of Political Economy*. 107(1). pp. 161-177.

Dincer, M.A., N. Kaushal and M. Grossman (2014): Women's Education: Harbinger of Another Spring? Evidence from a Natural Experiment in Turkey. *World Development*. 64. pp. 243-258.

Elder, T.E. and Lubotsky, D. (2009): Kindergarten entrance age and children's achievement: impacts of state policies, family background and peers. *Journal of Human Resources.* 44(3). 641-683.

Elsner, B. and Isphording, I.E. (2018): Rank, Sex, Drugs, and Crime. Journal of Human Resources. 53(2). 356-381.

Evans, W.N., M.S. Morrill, S.T. Parente (2010): Measuring inappropriate medical diagnosis and treatment in survey data: The case of ADHD among school-age children. *Journal of Health Economics*. 29. 657-673.

Finnish Institute for health and welfare (2019). Induced abortions in the Nordic countries 2017. Statistical Report 4/2019, 22.3.2019. THL. DOI: https://thl.fi/en/web/thlfi-en/statistics/statistics-by-topic/sexual-and-reproductive-health/abortions/induced-abortions-in-the-nordic-countries

Fredriksson, P. and B. Öckert (2005): Is Early Learning Really More Productive? The Effect of School Starting Age on School and Labor Market Performance. Iza Discussion paper no. 1659.

Fredriksson, P. and B. Öckert (2014): Life-cycle effects of age at school start. *Economic Journal*. 124(579). 977–1004.

Jatlaoui T.C., M.E.Boutot, M.G. Mandel, M.K. Whiteman, A. Ti, E. Petersen, and K.Pazol (2018): Abortion Surveillance — United States, 2015. MMWR Surveillance Summaries. 67(13). 1–45. DOI: http://dx.doi.org/10.15585/mmwr.ss6713a1

Jones, R.K. and K. Kost (2007): Underreporting of Induced and Spontaneous Abortion in the United States: An Analysis of the 2002 National Survey of Family Growth. *Studies in Family Planning*. 38(3). pp. 187-197.

Knudsen, L.B. (2007): Ung2006. 15-24-åriges seksualitet – viden, holdninger og adfærd. Sundhedsstyrelsen.

Landersø, R., Nielsen, H.S. and Simonsen, M. (2017): School Starting Age and the Crime-age Profile. *Economic Journal*. 127(602). pp. 1096-1118.

Landersø, R., Nielsen, H.S. and Simonsen, M.: Effects of School Starting Age on the Family. Forthcoming in *Journal of Human Resources*.

McCrary, J. and Royer, H. (2011): The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Policies Using Exact Date of Birth. *American Economic Review*. 101(1). pp. 158-195.

Reynoso, A. and Rossi, M.A. (2019): Teenage risky behavior and parental supervision: the unintended consequences of multiple shifts school systems. *Economic Inquiry*. 57(2). 774-791.

Skirbekk. V., Kohler. H., and Prskawetz. A. (2004). Birth Month, School Graduation, and the Timing of Births and Marriages. *Demography*. 41(3). 547-568.

Statens Serum Institute (2018): URL: https://www.ssi.dk/sygdomme-beredskab-og-forskning/sygdomsovervaagning/k/klamydia---opgoerelse-over-sygdomsforekomst-2017?fbclid=IwAR02DTpaImvvgNESk2owA5w65qFutTiUL7IIdGjCUtc2nhoAAXWrjQrHsl4. Accessed March 20, 2019.

Tan, P.L. (2017): The impact of school entry laws on female education and teenage fertility. *Journal of Population Economics*. 30. 503-536.

Tierney, K.I. (2019): Abortion Underreporting in Add Health: Findings and Implications. *Population Research and Policy Review*. 38(3). pp. 417-428.

WHO (2018): *Global status report on alcohol and health 2018*. Geneva: World Health Organization. DOI: https://www.who.int/publications-detail/global-status-report-on-alcohol-and-health-2018

Yakusheva, O. and Fletcher, J. (2015): Learning from Teen Childbearing Experiences of Close Friends: Evidence using Miscarriages as a Natural Experiment. *The Review of Economics and Statistics*. 97(1). pp. 29-43.

Appendix

Table A.1. Fraction of girls starting school in 2007 and 2008 who repeat a grade.

Grade	Fraction repeating a grade
Kindergarten class	0.111
1^{st} grade	0.008
2^{nd} grade	0.005
3^{rd} grade	0.003
4^{th} grade	0.003
5^{th} grade	0.003
6^{th} grade	0.004
7^{th} grade	0.005
8^{th} grade	0.010
9^{th} grade	0.010

Note. Sample of girls starting school in 2007 and 2008. For kindergarten class it also includes girls who start school later.

Table A.2. First stage across grades.

	Born 1997, grade 4	Born 1997, Grade 8	Born 2000, Grade 1	Born 2000, Grade 4	Born 2000, Grade 8
Born after New Year (=1)	-0.2147***	-0.2079***	-0.2455***	-0.2358***	-0.2265***
	(0.0165)	(0.0166)	(0.0164)	(0.0165)	(0.0166)
# Observations	9,513	9,409	9,917	9,767	9,727

Note. Table shows results for the probability of being young-for-grade regressed on a dummy variable for being born January 1 or later. Sample of girls born November through February in 1997 or 2000. Grade refers to the grade used for the definition of being young-for-grade.

Table A.3. Means and standard deviation for background characteristics of the sample.

	Mean	SD	Abortion by age 20	Abortion by age 30	Alc. poisoning by age 20	Alc. poisoning by age 20
Explanatory variables						
"Old-for-grade" (0/1)	0.54	(0.50)	0.53	0.54	0.50	0.55
School starting age, 1st grade	7.12	(0.45)	7.12	7.13	7.08	7.13
Background characteristics						
Non-western immigrant or descendant (0/1)	0.05	(0.21)	0.04	0.05	0.04	0.02
Parents cohabiting or married (0/1)	0.77	(0.42)	0.67	0.68	0.69	0.71
Apgar, 5 minutes	9.97	(2.83)	9.99	9.99	9.90	9.93
Birth weight, grams	3,357.48	(600.65)	3318.57	3322.31	3304.90	3330.73
Gestational length, weeks	39.60	(1.80)	39.64	39.60	39.62	39.59
Mother, year prior to birth						
Mother, unemployed (0/1)	0.12	(0.33)	0.16	0.16	0.17	0.16
Mother, out of the labor force (0/1)	0.10	(0.30)	0.16	0.14	0.13	0.14
Mother, some further educ. (0/1)	0.22	(0.41)	0.13	0.14	0.14	0.16
Mother, month of educ.	141.25	(29.12)	132.38	133.55	131.22	135.10
Mother, age at birth	27.44	(4.86)	26.12	26.38	26.01	26.92
Father, year prior to birth						
Father, unemployed (0/1)	0.08	(0.26)	0.11	0.11	0.12	0.12
Father, out of the labor force (0/1)	0.05	(0.21)	0.07	0.07	0.05	0.06
Father, some further educ. (0/1)	0.19	(0.40)	0.12	0.13	0.12	0.14
Father, month of educ.	147.08	(31.85)	138.58	139.79	138.68	141.83
Father, age at birth	30.40	(5.79)	29.32	29.56	29.26	29.96
# observations	89,496		7,404	13,486	8,633	2,768

Note. The sample consists of women born November through February in the period Nov. 1981- Feb. 1992 except from the sample by age 30 where women are born Nov. 1981- Feb. 1987. I measure variables for the father or mother the year prior to childbirth.

Table A.4. Means for outcomes by a given age.

Age	Abortion	Alcohol poisoning	Births	Contraception	Cohabitation	Chlamydia	Pregnancy	Partner's relative age	# obs.
15	0.008	0.010	0.001	0.295		0.019	0.010		89,496
16	0.017	0.015	0.004	0.499		0.040	0.022		89,496
17	0.030	0.019	0.009	0.655	0.004	0.071	0.040	4.990	89,496
18	0.047	0.024	0.021	0.755	0.027	0.106	0.067	4.335	89,496
19	0.065	0.028	0.039	0.815	0.107	0.147	0.099	3.671	89,496
20	0.083	0.031	0.061	0.855	0.213	0.184	0.133	3.327	89,496
21	0.099	0.034	0.087	0.883	0.340	0.217	0.169	3.079	89,496
22	0.114	0.036	0.120	0.903	0.451	0.245	0.208	2.934	89,496
23	0.127	0.037	0.162	0.918	0.540	0.269	0.253	2.855	89,496
24	0.139	0.039	0.214	0.928	0.613	0.285	0.307	2.803	89,496
25	0.151	0.040	0.273	0.936	0.676	0.298	0.365	2.746	89,496
26	0.161	0.041	0.345	0.943	0.729	0.306	0.434	2.729	79,807
27	0.170	0.042	0.421	0.949	0.772	0.310	0.504	2.729	70,718
28	0.178	0.042	0.498	0.953	0.809	0.310	0.575	2.732	61,846
29	0.185	0.043	0.566	0.957	0.839	0.307	0.636	2.737	53,358
30	0.191	0.043	0.620	0.960	0.860	0.307	0.685	2.763	45,183

Note. All outcomes are dummy variables. For the ages, 15-25 the sample consists of women born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. The sample is smaller for chlamydia for age 15 to 17 as the data is only available from 1999. The age for births refers to birthdate of the first child - 280 days, which is the approximate time of conception. Pregnancy refers to births, abortions and miscarriages. I calculate the partner's relative age from the partner and the woman's exact birthdates. Number of observations for "Partner's relative age" correspond to women who have cohabited by a given age. The fraction of women, who have had chlamydia, falls between age 28 and 29 because of different trends between cohorts.

Table A.5. Means for outcomes conditional on abortion or alcohol poisoning by age 20 or 30.

	Overall mean	Abortion by age 20	Abortion by age 30	Alc. poisoning by age 20	Alc poisoning by age 30
Outcome by age 20					
Abortion	0.083			0.193	
Alcohol poisonings	0.031	0.072			
Chlamydia	0.184	0.363		0.323	
Contraception	0.855	0.963		0.935	
Birth	0.061	0.189		0.117	
# observations	89,496	7,404		2,768	
Outcome by age 30					
Abortion	0.191				0.356
Alcohol poisonings	0.043		0.080		
Chlamydia	0.307		0.445		0.444
Contraception	0.960		0.987		0.981
Birth	0.620		0.710		0.636
# observations	45,183		8,633		1,930

Note. The sample consists of women born November through February in the period Nov. 1981- Feb. 1992 except from the sample for age 30 where women are born Nov. 1981- Feb. 1987.

Table A.6. Means for number of outcomes by a given age.

Age	Abortion	Alcohol poisoning	Births	Chlamydia	# cohabiting partners	# obs.
15	0.0084	0.0112	0.0012	0.0220		89,496
16	0.0180	0.0165	0.0037	0.0479		89,496
17	0.0325	0.0214	0.0098	0.0892	0.0037	89,496
18	0.0527	0.0273	0.0224	0.1385	0.0272	89,496
19	0.0764	0.0325	0.0421	0.1986	0.1083	89,496
20	0.1008	0.0367	0.0694	0.2584	0.2220	89,496
21	0.1251	0.0405	0.1041	0.3148	0.3625	89,496
22	0.1487	0.0435	0.1493	0.3651	0.4988	89,496
23	0.1705	0.0464	0.2097	0.4122	0.6220	89,496
24	0.1919	0.0491	0.2868	0.4471	0.7355	89,496
25	0.2124	0.0512	0.3774	0.4762	0.8435	89,496
26	0.2321	0.0528	0.4954	0.4924	0.9495	79,807
27	0.2491	0.0539	0.6312	0.4986	1.0452	70,718
28	0.2638	0.0553	0.7801	0.4979	1.1338	61,846
29	0.2773	0.0564	0.9266	0.4949	1.2090	53,358
30	0.2900	0.0573	1.0628	0.4930	1.2740	45,183

Note. All outcomes measure number of events. For the ages, 15-25 the sample consists of women born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. The sample is smaller for chlamydia for age 15 to 17 as the data is only available from 1999. The age for births refers to birthdate of the first child - 280 days, which is the approximate time of conception.

Table A.7. Regression estimates for different ages, by a given age.

Age	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
Abortion	0.008*	0.018***	0.021**	0.030***	0.031**	0.030**	0.017	0.033**	0.026	0.021	0.022	0.027	0.017	0.018	0.023	0.013
	(0.005)	(0.007)	(0.009)	(0.011)	(0.012)	(0.014)	(0.015)	(0.016)	(0.016)	(0.017)	(0.018)	(0.019)	(0.020)	(0.022)	(0.023)	(0.026)
Alcohol poisonings	0.008*	0.013**	0.013**	0.017**	0.016*	0.018**	0.017*	0.021**	0.020**	0.018*	0.016	0.018*	0.021*	0.019	0.017	0.016
	(0.005)	(0.006)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.011)	(0.012)	(0.012)	(0.013)
Contraception	0.089***	0.080***	0.024	0.003	0.003	0.006	0.014	0.009	0.008	0.005	0.001	0.006	0.005	0.007	0.022*	0.022*
	(0.022)	(0.024)	(0.023)	(0.021)	(0.019)	(0.017)	(0.015)	(0.014)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)
Chlamydia	0.011	0.005	0.005	0.002	-0.004	-0.013	-0.009	-0.006	-0.001	-0.008	-0.010	-0.015	-0.022	-0.008	0.018	0.032
	(0.008)	(0.011)	(0.013)	(0.015)	(0.017)	(0.019)	(0.020)	(0.021)	(0.022)	(0.022)	(0.023)	(0.024)	(0.025)	(0.027)	(0.028)	(0.030)
Births	0.001	0.004	0.005	0.010	0.018*	0.027**	0.039***	0.033**	0.030*	0.028	0.017	0.026	0.041	0.050*	0.030	0.030
	(0.002)	(0.003)	(0.005)	(0.007)	(0.009)	(0.012)	(0.014)	(0.016)	(0.018)	(0.020)	(0.021)	(0.024)	(0.027)	(0.029)	(0.030)	(0.032)
Cohabitation			0.002	0.040***	0.054***	0.066***	0.059**	0.056**	0.040	0.034	0.027	0.020	0.010	0.001	-0.007	-0.011
			(0.003)	(0.008)	(0.015)	(0.020)	(0.023)	(0.024)	(0.024)	(0.024)	(0.023)	(0.023)	(0.023)	(0.023)	(0.022)	(0.022)
# obs	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	79,807	70,718	61,846	53,358	45,183

Note. Table shows estimates from IV-regressions of outcome measured by a given age on young-for-grade instrumented by being born January 1 or later. For the ages 15-25, the sample consists of women born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. The sample is smaller for chlamydia for age 15 to 17 as the data is only available from 1999. The age for births refers to birthdate of the first child - 280 days, which is the approximate time of conception. * p<0.1, ** p<0.05, *** p<0.01.

Table A.8. Regression estimates for different ages, outcome measures number of events by a given age.

Age	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
Abortion	0.009*	0.019***	0.027***	0.039***	0.049***	0.050***	0.046**	0.064***	0.057**	0.053*	0.051*	0.065**	0.050	0.055	0.070	0.075
	(0.005)	(0.007)	(0.009)	(0.012)	(0.015)	(0.018)	(0.021)	(0.023)	(0.025)	(0.028)	(0.029)	(0.033)	(0.036)	(0.040)	(0.043)	(0.048)
Alcohol poisonings	0.010*	0.015**	0.016**	0.020**	0.018*	0.019*	0.017	0.022*	0.021	0.020	0.020	0.020	0.018	0.008	-0.002	-0.003
	(0.006)	(0.007)	(0.008)	(0.009)	(0.011)	(0.011)	(0.012)	(0.013)	(0.014)	(0.015)	(0.016)	(0.018)	(0.017)	(0.019)	(0.022)	(0.025)
# obs.	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	79,807	70,718	61,846	53,358	45,183

Note. Table shows estimates from IV-regressions of outcome measured by a given age on young-for-grade instrumented by being born January 1 or later. The outcomes include zero abortions and alcohol poisonings. For the ages 15-25, the sample consists of women born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff.
* p<0.1, ** p<0.05, *** p<0.01.

Table A.9. Regression estimates for different bandwidths, by a given age.

Age	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
Panel A: Abortion																
1 month	0.009	0.014	0.021	0.022	0.028	0.037*	0.020	0.038*	0.029	0.030	0.027	0.034	0.020	0.039	0.045	0.049
	(0.007)	(0.010)	(0.013)	(0.015)	(0.018)	(0.020)	(0.021)	(0.023)	(0.024)	(0.025)	(0.026)	(0.028)	(0.030)	(0.032)	(0.034)	(0.037)
# obs.	44,852	44,852	44,852	44,852	44,852	44,852	44,852	44,852	44,852	44,852	44,852	40,039	35,385	30,932	26,719	22,653
3 months	0.005	0.009**	0.011*	0.017**	0.020**	0.020**	0.011	0.020*	0.017	0.012	0.018	0.020	0.015	0.017	0.030*	0.026
	(0.003)	(0.004)	(0.006)	(0.007)	(0.008)	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)	(0.012)	(0.013)	(0.014)	(0.015)	(0.016)	(0.017)
# obs.	138,498	138,498	138,498	138,498	138,498	138,498	138,498	138,498	138,498	138,498	138,498	123,852	109,827	96,221	83,070	70,457
MSE	0.009	0.015	0.018	0.020	0.025	0.033	0.010	0.026	0.012	0.015	0.006	0.012	0.004	0.029	0.051	0.060
	[-0.006; 0.025]	[-0.004; 0.036]	[-0.011; 0.049]	[-0.017; 0.056]	[-0.016; 0.068]	[-0.011; 0.082]	[-0.039; 0.062]	[-0.025; 0.082]	[-0.043; 0.069]	[-0.041; 0.075]	[-0.054; 0.066]	[-0.053; 0.079]	[-0.065; 0.073]	[-0.044; 0.100]	[-0.021; 0.128]	[-0.019; 0.139]
Bandwidth	34.35	33.97	33	32.65	32.31	33.14	30.47	30.69	30.11	32.42	31.71	30.41	32.34	32.61	33.96	35.57
Panel B: Alcohol poisoning																
1 month	0.008	0.015*	0.017*	0.021*	0.021*	0.023*	0.019	0.027**	0.022	0.024*	0.021	0.024	0.017	0.018	0.012	0.011
	(0.007)	(0.009)	(0.010)	(0.011)	(0.012)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)	(0.016)	(0.017)	(0.018)	(0.019)
# obs.	44,852	44,852	44,852	44,852	44,852	44,852	44,852	44,852	44,852	44,852	44,852	40,039	35,385	30,932	26,719	22,653
3 months	0.005	0.006	0.004	0.005	0.003	0.002	0.003	0.005	0.004	0.003	0.002	0.002	0.001	0.003	0.004	0.000
	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)	(0.009)	(0.009)
# obs.	138,498	138,498	138,498	138,498	138,498	138,498	138,498	138,498	138,498	138,498	138,498	123,852	109,827	96,221	83,070	70,457
MSE	0.008	0.016	0.018	0.024	0.025	0.029	0.026	0.036	0.032	0.032	0.029	0.032	0.027	0.028	0.023	0.024
	[-0.008; 0.026]	[0.000; 0.038]	[0.001; 0.044]	[0.005; 0.053]	[0.004; 0.058]	[0.006; 0.062]	[-0.001; 0.059]	[0.009; 0.071]	[0.004; 0.067]	[0.004; 0.068]	[-0.000; 0.065]	[0.005; 0.072]	[0.002; 0.068]	[-0.003; 0.069]	[-0.011; 0.062]	[-0.010; 0.065]
Bandwidth	32.00	47.45	49.62	48.52	46.28	38.88	35.78	35.89	35.22	34.69	34.69	43.15	50.54	40.13	39.62	43.01

Note. Table shows estimates from IV-regressions of outcome measured by a given age on young-for-grade instrumented by being born January 1 or later. For the ages 15-25, the sample consists of women born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. P-values for joint significance of background variables (similar to Table A.11) is 0.56 for the sample +/- 1 month around the cutoff and 0.002 for the sample +/- 3 months around the cutoff. Results for MSE report the MSE optimal estimate and bandwidth with 95% confidence bands according to Calonico et al. (2014). * p<0.1, ** p<0.05, *** p<0.01.

Table A.10. Regression estimates using a second order polynomial that varies around the cutoff as control function, outcomes measured by a given age.

Age	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
Abortion	0.007	0.013	0.024	0.024	0.036	0.047*	0.025	0.044	0.031	0.029	0.022	0.024	-0.002	0.030	0.042	0.054
	(0.008)	(0.012)	(0.016)	(0.019)	(0.022)	(0.025)	(0.027)	(0.029)	(0.030)	(0.031)	(0.032)	(0.035)	(0.037)	(0.040)	(0.042)	(0.045)
Alcohol poisonings	0.009	0.017	0.019	0.026*	0.028*	0.031**	0.027	0.040**	0.036**	0.037**	0.035*	0.041**	0.034*	0.041*	0.033	0.032
	(0.009)	(0.011)	(0.012)	(0.014)	(0.015)	(0.016)	(0.016)	(0.017)	(0.017)	(0.018)	(0.018)	(0.019)	(0.020)	(0.022)	(0.022)	(0.023)
# obs.	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	79,807	70,718	61,846	53,358	45,183

Note. Table shows estimates from IV-regressions of outcome measured by a given age on young-for-grade instrumented by being born January 1 or later. For the ages 15-25, the sample consists of women born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. * p<0.1, ** p<0.05, *** p<0.01.

Table A.11. Balance of covariates.

	Joint regression	Separate regressions
Non-western immigrant or descendant (0/1)	0.00346	0.00341
	(0.00482)	(0.00283)
Mother unemployed (0/1)	0.00179	0.00267
	(0.00264)	(0.00456)
Mother, out of the labour force (0/1)	0.00235	0.00223
	(0.00294)	(0.00422)
Mother, educ. above high school (0/1)	-0.00347	-0.00699
	(0.00301)	(0.00582)
Mother, months of educ.	0.00007	0.0768
	(0.00004)	(0.407)
Father, unemployed (0/1)	-0.000346	0.000396
	(0.00327)	(0.00367)
Father, out of the labour force (0/1)	-0.00602	-0.00411
	(0.00411)	(0.00297)
Father, educ. above high school (0/1)	-0.00570*	-0.0102*
	(0.00291)	(0.00560)
Father, months of educ.	0.00004	-0.103
	(0.00004)	(0.450)
Parents living together (0/1)	0.00168	0.00343
	(0.00216)	(0.00570)
Mother, age at birth	-0.000536**	-0.120*
	(0.000243)	(0.0659)
Father, age at birth	0.000275	-0.0200
	(0.000194)	(0.0794)
Apgar score, 5 minutes	0.000202	0.0263
	(0.000296)	(0.0395)
Gestational length, weeks	-0.000700	-0.0136
	(0.000567)	(0.0251)
Births weight, grams	0.000003*	12.78
	(0.000002)	(8.344)
# Observations	89,496	
F-stat	1.377	
P-val	0.148	

Note. For the joint regression, the outcome is an indicator for being born January 1 or later. For the separate regressions, the row variable is the outcome and the independent variable an indicator for being born January 1 or later. The sample consists of women born November to February in the period Nov. 1981-Feb. 1992. Variables for the father or mother are measured the year prior to childbirth. The regressions include controls for missing variables, centered birth year and distance to the cutoff. * p<0.1, ** p<0.05, *** p<0.01.

Table A.12. Regression estimates for donut approach, outcomes measured by a given age.

Age	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
Abortion: excl. +/- 1 day	0.009*	0.017**	0.018**	0.026**	0.030**	0.028**	0.018	0.032**	0.026	0.022	0.023	0.028	0.018	0.018	0.017	0.004
	(0.005)	(0.007)	(0.009)	(0.011)	(0.013)	(0.014)	(0.015)	(0.016)	(0.017)	(0.017)	(0.018)	(0.020)	(0.021)	(0.023)	(0.024)	(0.026)
# obs.	88,276	88,276	88,276	88,276	88,276	88,276	88,276	88,276	88,276	88,276	88,276	78,719	69,751	60,997	52,632	44,566
Abortion: excl. +/- 7 days	0.010*	0.022***	0.028***	0.043***	0.045***	0.039**	0.028	0.051***	0.044**	0.033	0.041*	0.038	0.025	0.014	0.010	-0.012
	(0.006)	(0.008)	(0.010)	(0.013)	(0.015)	(0.017)	(0.018)	(0.019)	(0.020)	(0.021)	(0.022)	(0.023)	(0.025)	(0.027)	(0.029)	(0.031)
# obs.	79,784	79,784	79,784	79,784	79,784	79,784	79,784	79,784	79,784	79,784	79,784	71,142	63,042	55,158	47,630	40,398
Alcohol poisonings: excl. +/- 1 day	0.008	0.011*	0.011	0.015*	0.012	0.015*	0.015	0.019**	0.017*	0.016	0.013	0.015	0.020*	0.017	0.014	0.013
	(0.005)	(0.006)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)	(0.013)	(0.013)
# obs.	88,276	88,276	88,276	88,276	88,276	88,276	88,276	88,276	88,276	88,276	88,276	78,719	69,751	60,997	52,632	44,566
Alcohol poisonings: Excl. +/- 7 days	0.012*	0.013*	0.011	0.014	0.008	0.012	0.012	0.013	0.013	0.012	0.009	0.010	0.015	0.012	0.011	0.009
	(0.006)	(0.007)	(0.008)	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)	(0.013)	(0.014)	(0.014)	(0.015)	(0.016)
# obs.	79,784	79,784	79,784	79,784	79,784	79,784	79,784	79,784	79,784	79,784	79,784	71,142	63,042	55,158	47,630	40,398

Note. Table shows estimates from IV-regressions of outcome measured by a given age on young-for-grade instrumented by being born January 1 or later. For the ages 15-25, the sample consists of women born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. * p<0.1, ** p<0.05, *** p<0.01.

Table A.13. Means for outcomes for boys by a given age.

Age	Alcohol poisoning	Chlamydia	Birth	Cohabitation	# observations
15	0.0100	0.0066	0.0003		89,215
16	0.0152	0.0123	0.0007		89,215
17	0.0206	0.0230	0.0020	0.0006	89,215
18	0.0266	0.0406	0.0052	0.0043	89,215
19	0.0319	0.0653	0.0113	0.0249	89,215
20	0.0363	0.0904	0.0207	0.0705	89,215
21	0.0401	0.1162	0.0336	0.1515	89,215
22	0.0434	0.1421	0.0515	0.2480	89,215
23	0.0459	0.1652	0.0753	0.3418	89,215
24	0.0483	0.1850	0.1086	0.4280	89,215
25	0.0502	0.2007	0.1487	0.5067	89,215
26	0.0524	0.2117	0.2019	0.5822	79,783
27	0.0542	0.2209	0.2649	0.6425	70,628
28	0.0553	0.2270	0.3309	0.6924	61,761
29	0.0563	0.2296	0.3989	0.7346	53,119
30	0.0572	0.2320	0.4638	0.7686	45,023

Note. All outcomes are dummy variables. For the ages, 15-25 the sample consists of men born November 1981-February 1992. From age 25, each additional age deletes the latest cohort. The sample is smaller for chlamydia for age 15 to 17 as the data is only available from 1999. The age for births refers to birthdate of the first child - 280 days, which is the approximate time of conception.

Table A.14. Regression estimates for different grades, by a given number of years after 8th grade.

Years after 8th grade	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Abortion	-0.001	0.009*	0.005	0.006	0.014	0.018	0.008	0.010	0.015	0.015	0.010	0.018	0.006	0.010	0.013	0.010
	(0.003)	(0.005)	(0.007)	(0.009)	(0.011)	(0.012)	(0.014)	(0.015)	(0.016)	(0.017)	(0.017)	(0.019)	(0.020)	(0.022)	(0.023)	(0.025)
Alcohol poisonings	0.004	0.005	0.010	0.009	0.012	0.010	0.012	0.015	0.018*	0.016*	0.015	0.017*	0.020*	0.018	0.017	0.016
	(0.004)	(0.005)	(0.006)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.011)	(0.012)	(0.012)	(0.013)
Contraception	-0.102***	-0.132***	-0.121***	-0.097***	-0.059***	-0.043**	-0.026	-0.021	-0.012	-0.012	-0.005	-0.001	0.001	0.004	0.015	0.018
	(0.017)	(0.022)	(0.024)	(0.022)	(0.020)	(0.018)	(0.017)	(0.015)	(0.014)	(0.013)	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)
Chlamydia	-0.005	-0.019*	-0.016	-0.028*	-0.050***	-0.051***	-0.053***	-0.044**	-0.038*	-0.024	-0.028	-0.032	-0.031	-0.018	0.005	0.022
	(0.007)	(0.010)	(0.013)	(0.015)	(0.017)	(0.018)	(0.019)	(0.020)	(0.021)	(0.022)	(0.022)	(0.024)	(0.025)	(0.027)	(0.028)	(0.030)
Births	-0.002	-0.001	0.000	-0.001	-0.001	-0.006	0.008	-0.004	-0.006	-0.020	-0.037*	-0.046**	-0.034	-0.029	-0.029	-0.019
	(0.001)	(0.002)	(0.003)	(0.005)	(0.008)	(0.010)	(0.012)	(0.014)	(0.016)	(0.018)	(0.020)	(0.023)	(0.026)	(0.028)	(0.030)	(0.032)
Cohabitation				-0.001	-0.034***	-0.064***	-0.074***	-0.047**	-0.036	-0.044*	-0.022	-0.022	-0.018	-0.024	-0.028	-0.041*
				(0.006)	(0.012)	(0.018)	(0.022)	(0.024)	(0.024)	(0.024)	(0.023)	(0.024)	(0.023)	(0.023)	(0.023)	(0.023)
# obs	81,778	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	89,496	79,807	70,718	61,846	53,358	45,183

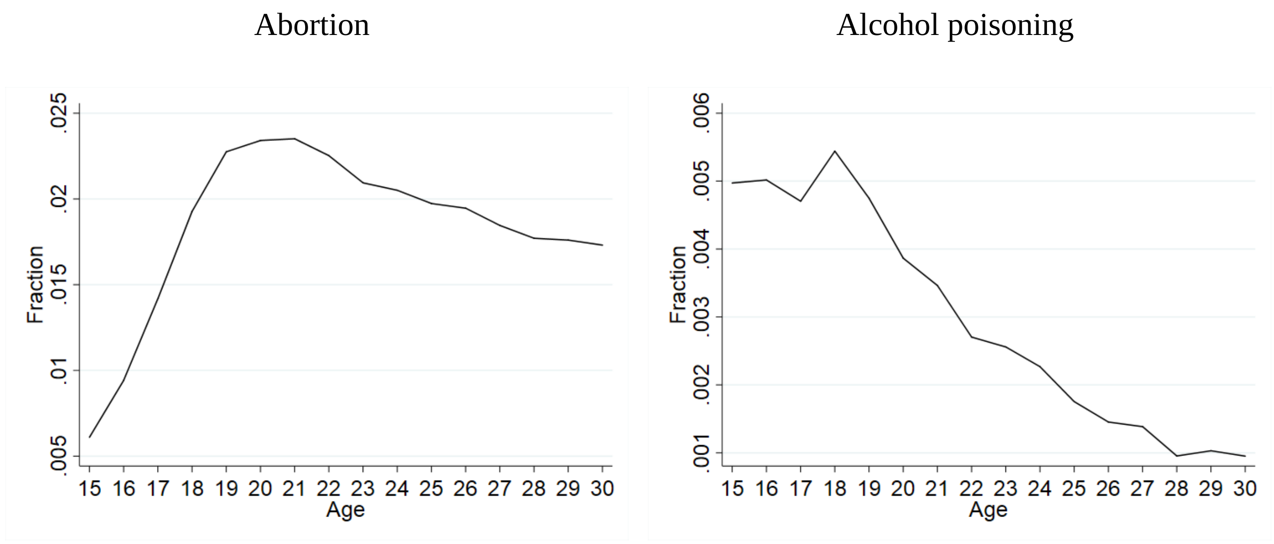
Note. Table shows estimates from IV-regressions of outcome measured by a given grade on young-for-grade instrumented by being born January 1 or later. For years 1-10, the sample consists of women born November 1981-February 1992. From year 10, each additional year deletes the latest cohort. For year 0, women born in 1981/1982 are deleted. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. The sample is smaller for chlamydia for years 0 to 4 as the data is only available from 1999. The age for births refers to birthdate of the first child - 280 days, which is the approximate time of conception. * p<0.1, ** p<0.05, *** p<0.01.

Table A.15. Results with peers' average age.

Age	Abortion		Alcohol poisoning	
	Young-for-grade	Peers' avg. age	Young-for-grade	Peers' avg. age
15	0.009*	0.017	0.008*	-0.013
	(0.005)	(0.012)	(0.005)	(0.014)
16	0.018***	0.014	0.012**	-0.025
	(0.007)	(0.018)	(0.006)	(0.017)
17	0.021**	0.042*	0.013*	-0.030
	(0.009)	(0.023)	(0.007)	(0.019)
18	0.030***	0.060**	0.017**	-0.016
	(0.011)	(0.029)	(0.008)	(0.021)
19	0.032***	0.065*	0.015*	-0.026
	(0.012)	(0.033)	(0.008)	(0.022)
20	0.031**	0.051	0.017**	-0.029
	(0.014)	(0.037)	(0.009)	(0.024)
21	0.017	0.037	0.016*	-0.038
	(0.015)	(0.040)	(0.009)	(0.025)
22	0.033**	0.023	0.021**	-0.027
	(0.016)	(0.043)	(0.009)	(0.025)
23	0.025	-0.015	0.020**	-0.023
	(0.016)	(0.045)	(0.009)	(0.026)
24	0.021	-0.013	0.018*	-0.029
	(0.017)	(0.047)	(0.010)	(0.026)
25	0.021	-0.014	0.016	-0.035
	(0.018)	(0.048)	(0.010)	(0.027)
26	0.026	-0.010	0.018*	-0.021
	(0.019)	(0.052)	(0.010)	(0.028)
27	0.016	-0.036	0.020*	-0.031
	(0.020)	(0.056)	(0.011)	(0.030)
28	0.017	-0.030	0.018	-0.031
	(0.022)	(0.060)	(0.012)	(0.032)
29	0.023	-0.012	0.017	-0.010
	(0.023)	(0.065)	(0.012)	(0.034)
30	0.013	-0.007	0.016	0.016
	(0.026)	(0.071)	(0.013)	(0.037)

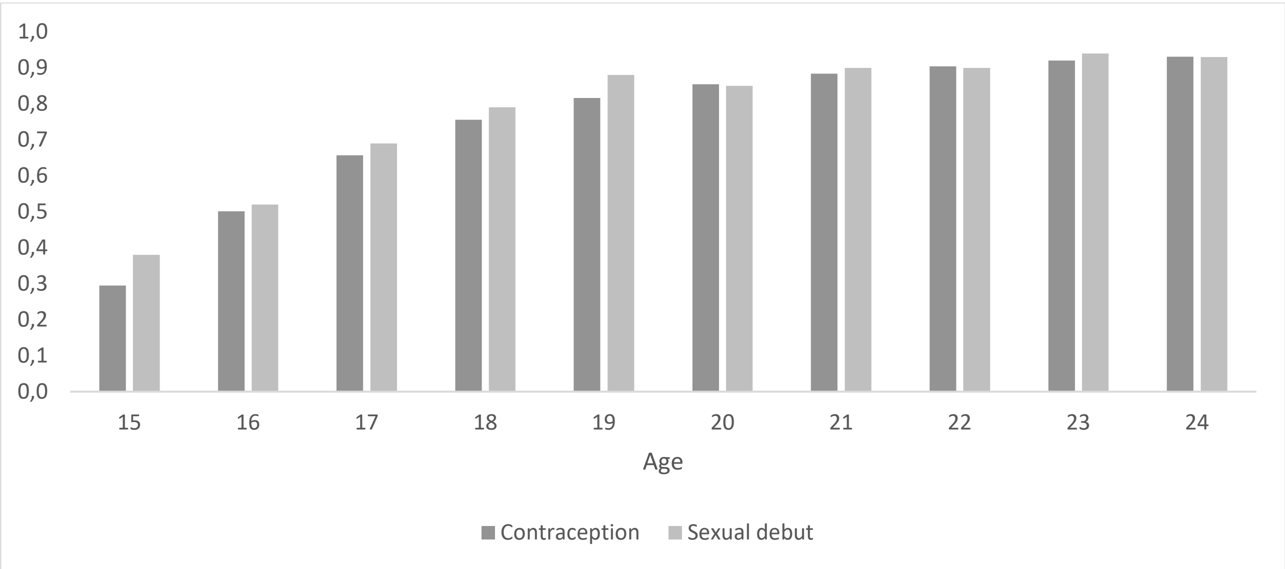
Note. Table shows estimates from IV-regressions of outcome measured by a given age on young-for-grade instrumented by being born January 1 or later and peers' average age instrumented with the average expected age according to the school starting age rule. For the ages 15-25, the sample consists of women born November 1981-February 1992. I exclude 18 women with no peers. From age 25, each additional age deletes the latest cohort. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. * p<0.1, ** p<0.05, *** p<0.01.

Figure A.1. Fraction of women who experience an outcome at a given age.



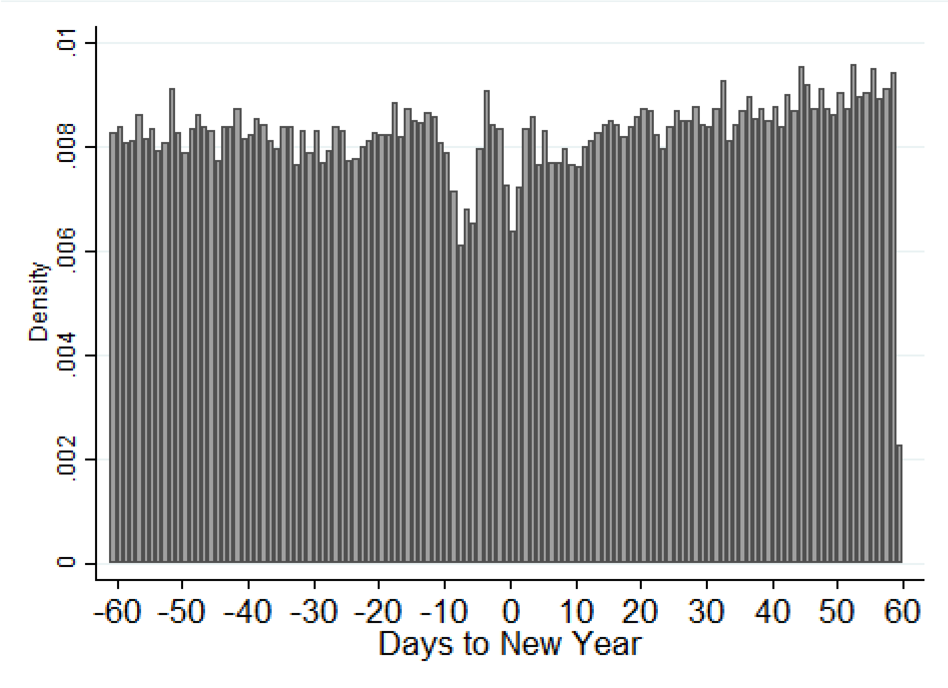
Note. Fraction of women, who experience an outcome at a given age. For the ages, 15-25 the sample consists of women born Nov. 1981-Feb. 1992. From age 25, each additional age drops the latest cohort.

Figure A.2. Sexual debut according to survey compared with register data for using hormonal contraception.



Note. The numbers for the sexual debut are from Knudsen (2007). Contraception refers to women who have used hormonal contraception according to my data.

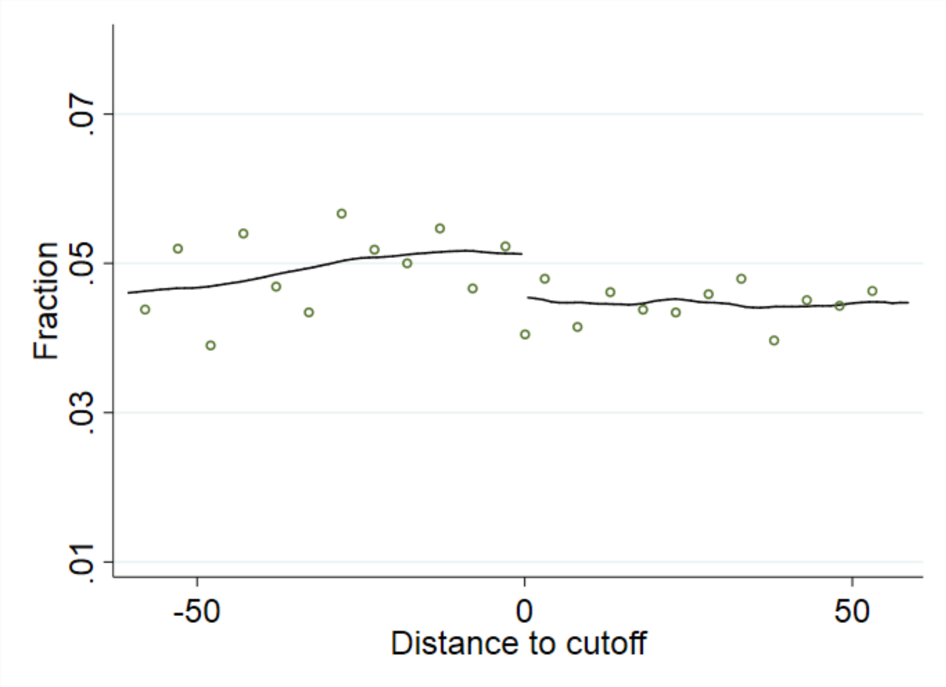
Figure A.3. Histogram of number of births on each birthdate.



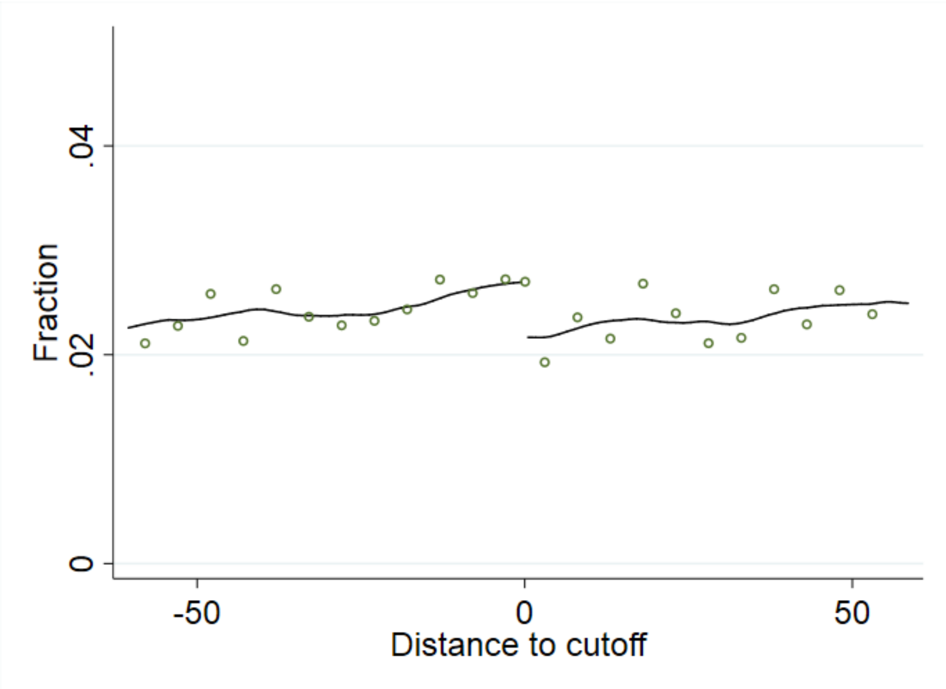
Note. The sample consists of women born November through February in the period November 1981-February 1992.

Figure A.4. Reduced form plots, abortion or alcohol poisoning by age 18.

Abortion, by age 18

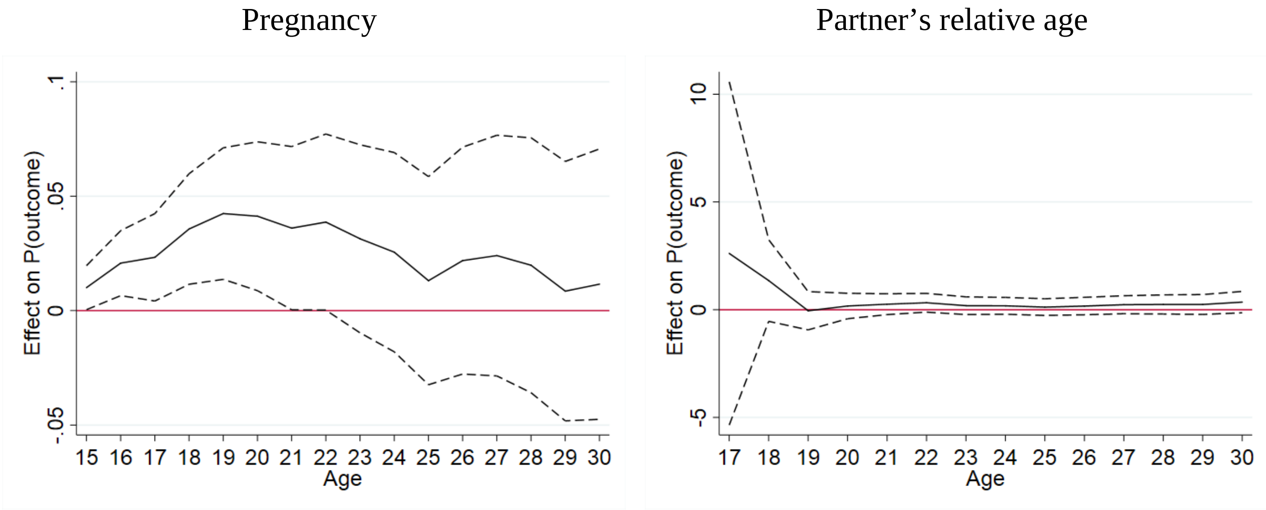


Alcohol poisoning, by age 18



Note. The sample consists of women born in November through February in the period Nov. 1981- Feb. 1992. The dots show the probability of having experienced an outcome in bins of five birthdates.

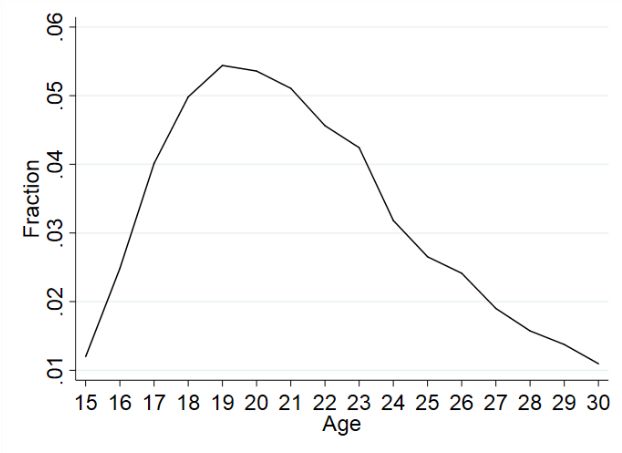
Figure A.5. Effect of being young-for-grade on probability of pregnancy and partner's relative age by a given age.



Note. Figure shows estimates from IV-regressions of indicator for pregnancy or first cohabiting partner's age relative to a woman's age by a given age on young-for-grade instrumented by being born January 1 or later. Pregnancy includes abortions, births and miscarriages. I calculate the partner's relative age from the partner and the woman's exact birthdates. The sample consists of women born Nov. 1981-Feb. 1992. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. The dashed lines show the 95% confidence bands.

Figure A.6. Fraction of women who experience chlamydia at a given age.

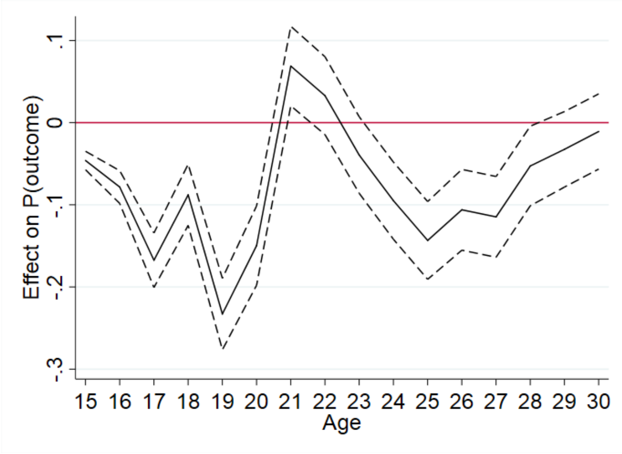
Chlamydia



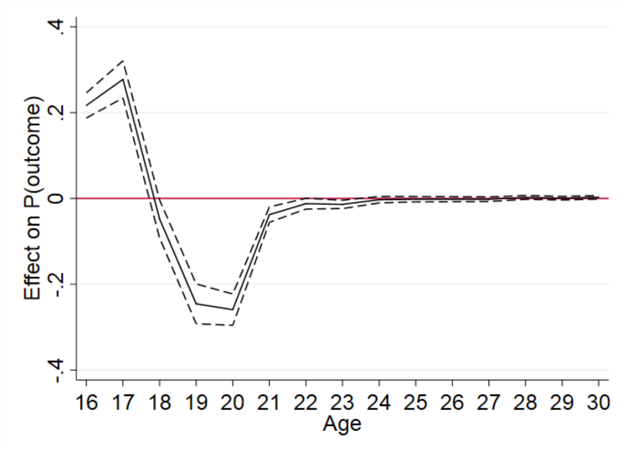
Note. Fraction of women, who experience chlamydia at a given age. For the ages, 15-25 the sample consists of women born Nov. 1981-Feb. 1992. From age 25, each additional age drops the latest cohort.

Figure A.7. Results across ages for probability of enrollment into any education, high school or further education. Outcomes measured at a given birthdate.

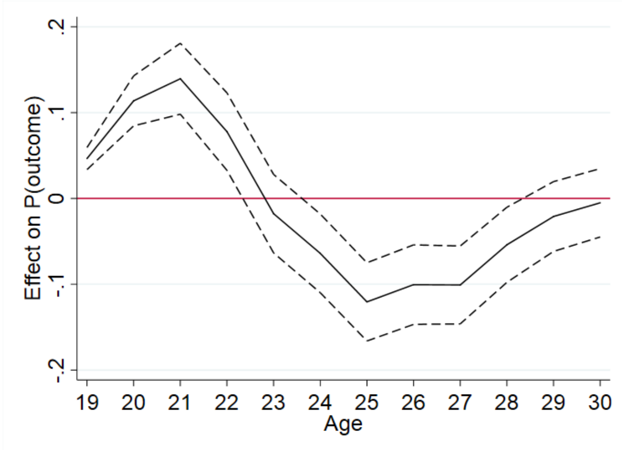
Enrollment in any education at a given birthdate



Enrollment in high school at a given birthdate

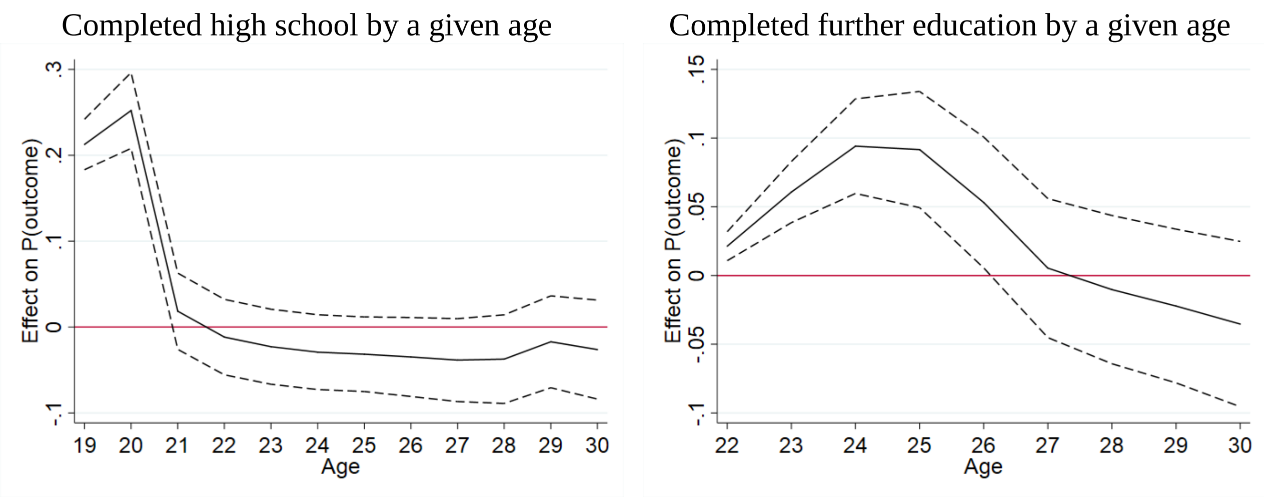


Enrollment in further education at a given birthdate



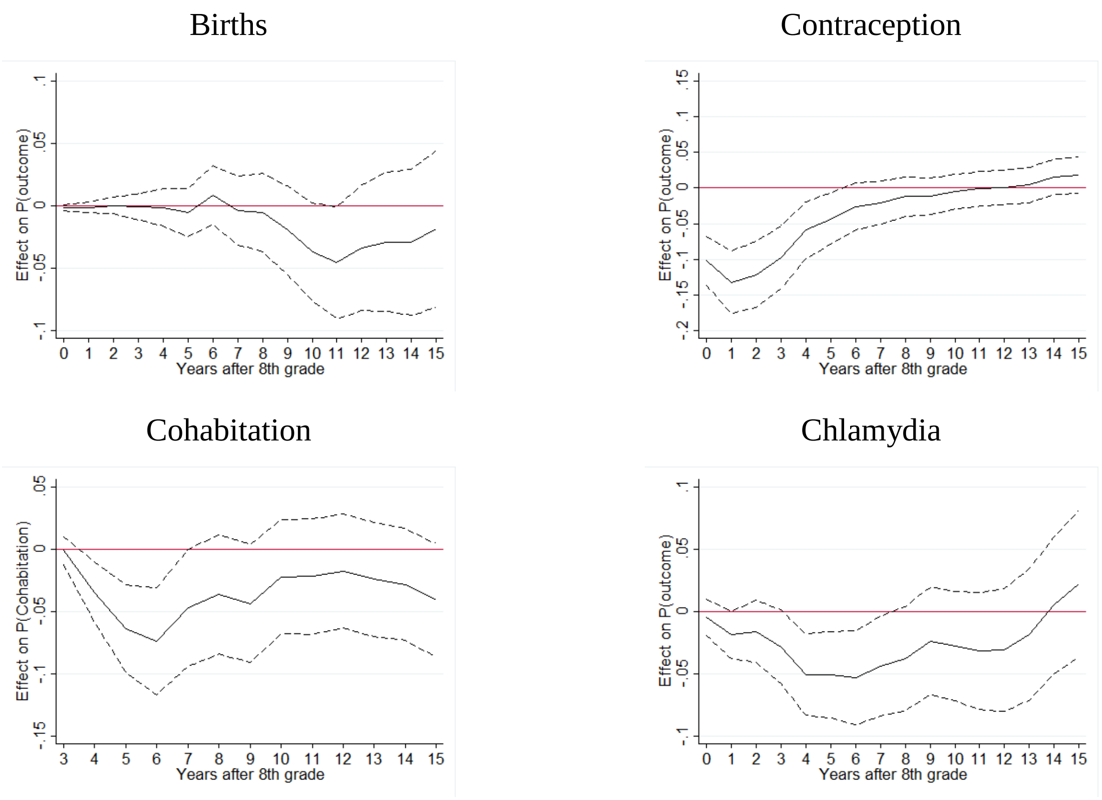
Note. Figure shows estimates from IV-regressions of indicator for enrollment into any education, high school or further education at a given birthdate on young-for-grade instrumented by being born January 1 or later. Age refers to the woman's birthdate. The sample consists of women born Nov. 1981-Feb. 1992. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. The dashed lines show the 95% confidence bands.

Figure A.8. Results across ages for probability of completed high school or further education. Outcomes measured by a given age.



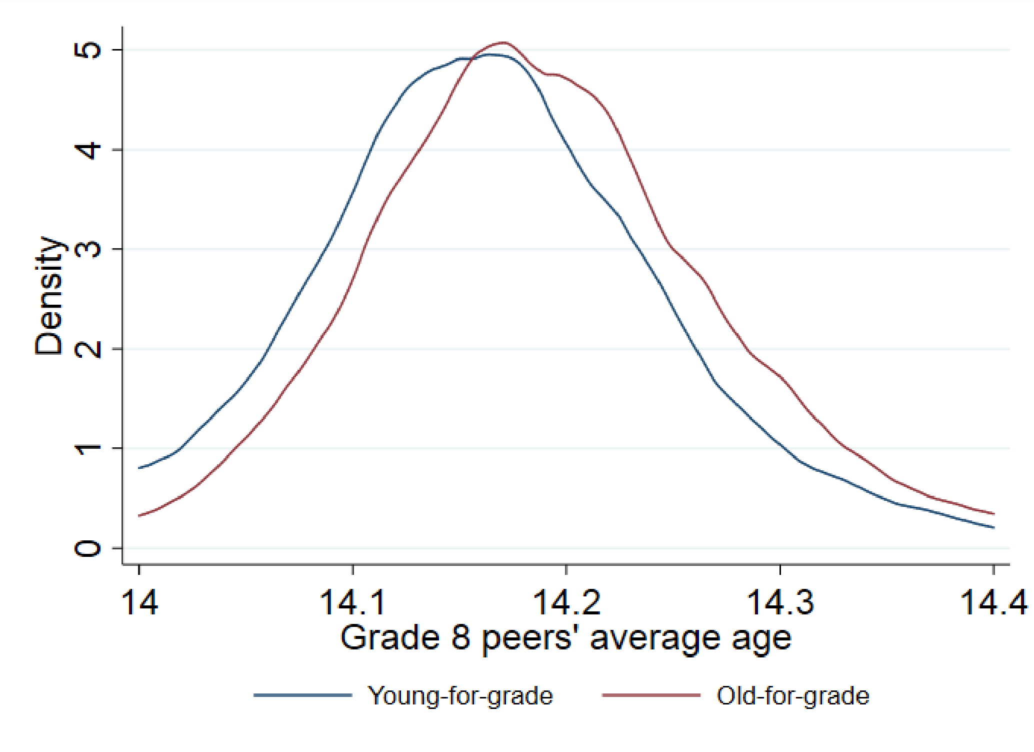
Note. Figure shows estimates from IV-regressions of indicator for completion of high school or further education by a given age on young-for-grade instrumented by being born January 1 or later. The sample consists of women born Nov. 1981-Feb. 1992. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. The dashed lines show the 95% confidence bands.

Figure A.9. Results across grades for births, contraception, cohabitation and chlamydia. Outcomes measured by years after 8th grade.



Note. Figure shows estimates from IV-regressions of outcome measured by a given number of years after 8th grade on young-for-grade instrumented by being born January 1 or later. For years 1-10, the sample consists of women born November 1981-February 1992. From year 10, each additional year deletes the latest cohort. For year 0, women born in 1981/1982 are deleted. The estimates control for background variables (see Table A.3), missing variables, centered birth year and distance to the cutoff. The sample is smaller for chlamydia for years 0 to 4 as the data is only available from 1999. The age for births refers to birthdate of the first child - 280 days, which is the approximate time of conception. The dashed lines show the 95% confidence bands.

Figure A.10. Distribution of grade 8 peers' average age.



Note. The sample consists of women born November through February in the period November 1981-February 1992. I exclude 18 women because they do not have any peers.