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DISCUSSION PAPER SERIES

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ABSTRACT

Timing of School Entry and Personality Traits in Adulthood

This paper investigates the long-run consequences of a later school entry for personality traits. For identification, we exploit the statutory cutoff rules for school enrollment in Germany within a regression discontinuity design. We find that relatively older school starters have persistently lower levels of neuroticism in adulthood. This effect is entirely driven by women, which has important implications for gender gaps in the labor market, as women typically score significantly higher on neuroticism at all stages of life, which puts them at a disadvantage. Our results suggest that family decisions regarding compliance with enrollment cutoffs may have lasting implications for gender gaps in socio-emotional skills.

JEL Classification: I21, I28, J24, D19

Keywords: school starting age, personality, socio-emotional skills, education

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1 Introduction

School entry laws are among the most common public policies worldwide and determine the timing of a child's transition to formal education. There is considerable variation in the average age at entry across countries (OECD, 2020), and a large body of literature has studied its implications for children's cognitive development and later life outcomes.[1] Much less attention has been paid to its consequences for the formation and persistence of other, so-called non-cognitive, skills.[2] This is surprising given the growing importance of such skills in the labor market (e.g., Del Bono et al., 2024). However, there is evidence that late school starters are overrepresented in highly competitive sectors and occupations.[3] Beyond high-level cognitive abilities, such jobs arguably require certain non-cognitive skills and, in particular, specific personality traits. But is there a causal link between the timing of school entry and personality?

The economic and psychological literature emphasizes the role of the family and external factors, e.g. schools, in shaping non-cognitive skills, including personality (e.g., Borghans et al., 2008, Bertrand and Pan, 2013, Heckman et al., 2019, Bleidorn et al., 2019). The family environment may influence skill development, e.g., through genetics, but also investments in parenting or education (Francesconi and Heckman, 2016, Elkins and Schurer, 2020). Parental adherence to enrollment cutoffs may affect their children's socio-emotional skills via school starting age, e.g., by providing them with extra time to develop additional coping mechanisms before school entry (e.g., Caspi et al., 2005). Older children, benefiting from emotional maturity and positive feedback from teachers, often exhibit higher self-confidence and emotional stability (e.g., Thompson et al., 2004, Crawford et al., 2011, Page et al., 2019). Even if some differences initially arise solely due to the advantage in chronological age, the self-productivity and dynamic complementarity of skills create mechanisms that might perpetuate the effects into adolescence and beyond (Cunha and Heckman, 2007, 2008).[4]

[1] This includes positive effects on student achievement (e.g., Bedard and Dhuey, 2006, Puhani and Weber, 2008, Elder and Lubotsky, 2009, Smith, 2009, Cook and Kang, 2016, Dhuey et al., 2019), typically small but positive effects on earnings and employment (e.g., Fertig and Kluve, 2005, Black et al., 2011, Fredriksson and Öckert, 2014, Røed-Larsen and Solli, 2017, Peña, 2017), and generally positive but small effects on educational attainment (e.g., Angrist and Krueger, 1992, Black et al., 2011, Fredriksson and Öckert, 2014, Cook and Kang, 2016).

[2] In Economics, non-cognitive skills refer to a broad set of attributes, including socio-emotional skills (e.g., emotional regulation, social interaction) and personality traits (e.g., neuroticism, openness) (e.g., Gensowski et al., 2021, Del Bono et al., 2024). The terms are often used interchangeably.

[3] A late school start is associated with an overrepresentation in sports (e.g., Musch and Grondin, 2001), politics (Muller and Page, 2016, Tukiainen et al., 2019), and science (Fukunaga et al., 2013), and an overrepresentation among corporate CEOs (Du et al., 2012), chess players (Breznik and Law, 2016), as well as holders of personal Wikipedia entries (Peña and Stephens-Davidowitz, 2021).

[4] For example, greater maturity might promote efficient learning and each small learning achievement

This paper uses institutionally induced variation in the timing of individual school entry to analyze its long-run effects on personality traits in adulthood. Specifically, we exploit statutory cutoff rules for school enrollment in Germany within a regression discontinuity design (RDD). Despite some legal leeway for family discretion, compliance with school enrollment rules is generally high in Germany, making it an ideal setting to examine the long-run effects of a later versus earlier school start. For the main analysis, we use large-scale survey data on the Big Five personality traits of individuals born between the late 1940s and 1990s from the German Socio-economic Panel (SOEP). We complement the SOEP with data on the timing of school entry and educational trajectories of the relevant cohorts from the National Educational Panel Study (NEPS-SC6).

We find that a later school start permanently reduces the neuroticism level of women. There are no corresponding effects on men. Our findings are robust across an extensive battery of robustness checks and bear important implications for other long-term outcomes, as higher levels of neuroticism are typically associated with worse emotional well-being and socioeconomic outcomes (e.g., Gensowski et al., 2021), as well as lower earnings (e.g., Mueller and Plug, 2006, Gensowski, 2018). The positive results on attainment and academic tracking may partly explain these long-term reductions in neuroticism, particularly for women. The effects on other personality traits are, if anything, only transitory and potentially coincide with other life events.

This study contributes to several literatures. First, we contribute to the literature on school starting age effects on the formation of non-cognitive skills, including socio-emotional skills. Several studies have documented short-term benefits on students' behavior and socio-emotional development (e.g., Mühlenweg et al., 2012, Crawford et al., 2014, Datar and Gottfried, 2015, Lubotsky and Kaestner, 2016, Cornelissen and Dustmann, 2019). However, the evidence on their persistence is far from conclusive.[5] Morover, most studies could not examine the effects beyond the age of 16. We complement this literature by studying the long-run effects on personality traits and their dynamics over the life cycle. A long-term perspective is crucial as non-cognitive skills

might reinforce a child's self-esteem (Doyle et al., 2017). Along similar lines, Dhuey and Lipscomb (2008) argue that the relative maturity of older school entrants makes them disproportionately more likely to be selected for "leading" roles within their school cohort, which facilitates the development of labor market relevant skills.

[5]For example, Lubotsky and Kaestner (2016) find a large first-grade advantage of older school starters in the approach to learning and interpersonal skills, which however fade away quickly. In contrast, Cornelissen and Dustmann (2019) and Dee and Sievertsen (2018) show that substantial effects on disruptive behavior and hyperactivity, respectively, last up to age 11. Recently, Yamaguchi et al. (2023) document substantial gaps in conscientiousness, self-control, and self-efficacy, which remain relatively constant from grade 4 through 9. Similarly, for Seoul, Shin (2023) shows positive effects on self-esteem during middle and high school (i.e., until grade 10) for girls but no for boys. Page et al. (2019) find that older school starters report higher self-confidence and competitiveness in adulthood.

are still being developed during adolescence and early adulthood (e.g., Bleidorn, 2015).

Second, we contribute to the literature highlighting the importance of noncognitive skills in evaluating the long-run impacts of early-childhood interventions. Much of the existing economic research focuses on early-childhood interventions targeted at improving the skills of disadvantaged groups, such as the STAR experiment or Perry Preschool project (e.g., Chetty et al., 2011, Heckman et al., 2013, Algan et al., 2022), documenting positive effects on especially non-cognitive skills. On the other hand, much of the psychological literature has focused on clinical interventions (see Roberts et al., 2017, for an overview). Our study extends this literature by studying the unintended long-term impacts of a non-targeted, universal policy, highlighting the policy relevance of personality traits (Bleidorn et al., 2019).

Finally, our findings highlight the implications of enrollment cutoffs for the gender gaps in personality traits, contributing to the literature on gender disparities in non-cognitive skills (e.g., Gensowski et al., 2021) and sources of gender inequality in the labor market outcomes (e.g., Olivetti et al., 2024). Women consistently score higher on neuroticism than men across the life cycle, and this trait is linked to adverse labor market outcomes, including lower earnings (e.g., Mueller and Plug, 2006, Gensowski, 2018). By demonstrating that a later school start reduces neuroticism for women, we provide evidence that it compresses gender disparities in personality traits associated with disadvantageous socioeconomic outcomes.

This paper proceeds as follows. Section 2 provides institutional details. Section 3 describes the data and Section 4 the empirical strategy. Section 5 presents the main results on personality and robustness checks. Section 6 discusses potential mechanisms and presents results on education. Section 7 concludes.

2 Institutional background

In Germany, children turning six years old before a statutory cutoff date are scheduled for school entry at the beginning of the upcoming school year. Those turning six years old after the cutoff start compulsory schooling one year later. This is independent of Kindergarten attendance, which is voluntary and, in contrast to the U.S., is not an integral part of the German school system.[6] Compulsory schooling duration in Germany

[6] Kindergartens are formally childcare institutions for children from three years of age. They are typically not free of charge, although publicly subsidized(see, e.g., Bauernschuster and Schlotter, 2015, Bauernschuster et al., 2016, for details). As for West Germany, which we focus on, the share of individuals who ever attended kindergarten increased over time from around 50% among cohorts born in the 1950s to nearly 100% among those born in the 1980s. These numbers come from survey data from the NEPS-SC6 (for details, see Section 3) and mostly reflect half-day attendance at ages five to six. Administrative data

is grade-based and not age-based (e.g., in the U.S. or U.K.), which implies that it is, per se, unrelated to an individual's school starting date. From primary school through university education, education is generally free of charge.

The responsibility for educational policies lies with the federal states (e.g., Helbig and Nikolai, 2015), inducing some variation in enrollment cutoffs across states. For example, in the 1950s and 1960s, the most prevalent cutoff was March 31, but for many years, several states stuck to June 30 or December 31 (see Cygan-Rehm, forthcoming, for details). Moreover, all states moved the cutoff dates at least once during this period, so the cutoff also varied over time. In 1964, the *Hamburg Accord* introduced a uniform cutoff date for school enrollment on June 30, which became a common standard for nearly three decades. Since the early 2000s, many states moved the cutoffs forward by several months to postpone school entry. Currently, the statutory cutoff is largely state-specific again and varies between June 30 and December 31.

Some school entry laws explicitly allow to deviate from the sharp cutoff date by defining an early-enrollment exception rule. The specific regulations differ across states and over time (Kamb and Tamm, 2023, Görlitz et al., 2024), but typically, children born in the three months after the cutoff date are eligible for early enrollment upon application. There is limited scope for further exceptions, but families and public authorities have some remaining leeway if the statutory regulations are at odds with individual factors such as a child's intellectual and emotional maturity. Such cases are, however, accompanied by complex administrative procedures, which require extensive paperwork. Thus, maybe unsurprisingly, most families comply with the law.

Figure 1 shows the fractions of early, regular, and late school entrants according to official school statistics. The numbers reveal a high compliance with the school entry laws. Specifically, approximately 90% of children start school "on time", and this pattern remained relatively stable over time. However, official statistics include school entries within an early-enrollment exception rule in regular enrollments, so compliance with the sharp cutoff dates is somewhat lower. Using survey data, in Section 5.1, we show that it nonetheless reaches 75% on average. This suggests that approximately 15% of families make use of the statutory early-enrollment exceptions. Beyond that, early enrollment is rather rare (3%-5%) as documented in Figure 1. Only 5%-8% of children start school with a delay, suggesting that redshirting is uncommon in Germany. Taken together, the German institutional context implies that a child's birthdate and state of residence largely determine the timing of school enrollment. Our empirical analysis exploits this plausibly exogenous, institutionally induced variation.

on this issue are not available before 1990. There is virtually no private market for childcare services, so the most viable alternative to kindergarten attendance is informal care (mostly by mothers).

Typically, after four years in primary school, children are tracked into one out of three secondary school types: basic track (*Hauptschule*), middle track (*Realschule*), and high school (*Gymnasium*).[7] There are also alternative school types such as comprehensive schools without tracking (*Gesamtschule*) or schools for children with special needs (*Sonderschule, Förderschule*), but the vast majority of cohorts considered in this study participated in the traditional tripartite system. Generally, the tracks differ in duration and curriculum and prepare for different professional careers. In the period under study, the basic track lasted until grade eight or nine and prepared for an apprenticeship in blue-collar jobs. The middle track comprises ten grades and typically leads to an apprenticeship or training in white-collar jobs. Successful high school completion after grade 12 or 13 in the *Gymnasium* track, the academic track, gives access to higher education in colleges or universities. We discuss results on tracking and educational attainment in Section 6.

3 Data and descriptives

For the main analysis, we use data from the German Socio-Economic Panel (SOEP 1984-2019), the longest-running representative longitudinal survey of private households in Germany conducted annually since 1984 (Goebel et al., 2019). The data is provided by the Research Data Center of the Socio-Economic Panel (FDZ SOEP) at the German Institute for Economic Research (DIW Berlin). In addition to a relatively stable set of core socio-demographic characteristics collected annually, each year, the questionnaire includes additional modules asking in-depth questions on specific topics. Of our main interest are several measures of non-cognitive skills, including the Big Five Inventory (Costa and McCrae, 1999). The relevant questions were asked in 2005, 2009, 2012, 2013, 2017, and 2019. The SOEP gathers information on personality traits through a 15-item measurement (i.e., three questions per trait). Appendix Table A.1 details the measurement of the Big Five Inventory, which comprises openness to experience, conscientiousness, extraversion, agreeableness, and neuroticism.

In our main analysis, we focus on personality traits measured during prime ages between 25 and 60. This corresponds to a potential working life span and excludes periods when personality might change rapidly for reasons related to adolescence, education, or retirement.[8] It also mitigates concerns about potentially selective mortality or panel

[7]The tracking depends on various criteria, which differ by state. Details are provided, e.g., in Lüdemann and Schwerdt (2013).

[8]By focusing on ages 25-60, we also align with the recent findings by Fitzenberger et al. (2022) who document the additive separability of age, period, and cohort effects for a broad range of personality traits within this age range. While modeling the life-cycle profiles of personality traits is not the main

participation at advanced ages. For robustness, we re-run the main analysis using an extended age range. Due to differences between the Federal Republic of Germany and the socialist German Democratic Republic until the Reunification in 1990 (e.g., distinct educational systems), we focus on individuals who were born and enrolled in school in the West German states (excl. Berlin) throughout.[9]

The SOEP traces educational transitions only as long as they occur within the panel (i.e., between 1984 and 2019). Thus, information on the actual enrollment date is unavailable for most respondents in our estimation sample, as they entered school between the 1950s and 1980s. Thus, to provide evidence on compliance with the school entry cutoffs for the cohorts under study, we use auxiliary data from the National Educational Panel Study: Starting Cohort Adults (NEPS-SC6).[10] The study started in 2007/2008 and includes a representative sample of the population born between 1944 and 1986. The key advantage of the data is the availability of detailed information on educational trajectories in monthly spells, which is collected retrospectively during the first interview and, if necessary, updated by more recent information from successive interviews. Thus, using the information on the date of primary school entry and the date of birth, we can infer who complied with the school entry cutoffs. In addition, the NEPS provides information on parental socio-economic background, which allows us to investigate whether compliance is potentially selective.

Since personality traits were assessed in multiple waves in the SOEP for a given individual, we observe the outcomes up to six times at different ages. Our main estimation sample includes 42,052 observations on 20,491 individuals. In the main analysis, we pool all available data and weigh the person-year observations by the inverse of the number of times each individual appears in the panel. This gives each person an equal weight in the estimations. We also include a full set of survey year and year of birth indicators, which flexibly capture any calendar time and cohort-specific effects and non-parametrically control for age. Nevertheless, in the robustness section, we show that our main results are nearly identical when we use pooled panel estimations or just one

aim of our study, we show that our main conclusions do not change after controlling for age, period, and cohort effects. Nevertheless, we also show that the estimated SSA effects are not entirely constant over the life cycle.

[9] There is no direct information on the state of school enrollment for the cohorts in our SOEP sample. Thus, we construct a proxy by using the available information on the state of birth (30% of the sample), the state of residence in childhood (21%), and the first state of residence ever observed for a given individual in the SOEP (49%). Our results are robust to alternative approaches to approximate the state of schooling. This is not surprising given that regional mobility in Germany is generally low, and there is a substantial match between the different regional variables. We also exclude individuals who lived in East German states in 1989 because they potentially attended school in the GDR.

[10] The NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi, Germany) in cooperation with a nationwide network (see Blossfeld and Roßbach, 2019).

observation per person. This is not surprising given that for a given individual, the treatment variable is time-invariant, and personality traits demonstrate relatively stable developments during prime ages. To account for repeated observations for each person, in the main analysis, we cluster the standard errors at the individual level.

Figure 2 shows the age-personality profiles in the SOEP data. Specifically, we plot raw age-specific means for an extended age range from 17 to 70.[11] The figure uncovers several important patterns. First, save for conscientiousness, the Big Five personality traits (if anything) change slowly over the life cycle but are not completely constant. Specifically, we observe a steep growth in conscientiousness between late adolescence and mid-20s and a flattening of the curve from age 30 onward. Openness to experience follows a slightly decreasing trend until the mid-30s and remains stable until retirement. Extraversion and neuroticism also slightly decrease with age, while agreeableness remains fairly stable. Second, the most pronounced changes occur during adolescence and relatively late in life, which corroborates our focus on the prime-age range marked by the dashed vertical lines. Finally, women of all ages score higher than men on all personality traits. The differences are the largest for neuroticism and the smallest for openness to experience. These patterns are generally in line with a broader literature on age gradients in the Big Five personality traits (for recent summaries, e.g., Fitzenberger et al., 2022, Gensowski et al., 2021).

Finally, we link the survey data to administrative data on the relevant institutional details for the period under study (described in Section 2). Specifically, for each state and each school year starting from 1951/2 and 1970/1, we use the information on the statutory cutoff date from Cygan-Rehm (forthcoming) and extend the data to more recent years. We retrieved the relevant details from public records, mostly including original state laws. Thus, based on an individual's birth month and the relevant state-specific cutoff date for school enrollment, we assign an indicator of whether an individual was born before or after the cutoff.

Table 1 provides the variable means for our main estimation sample. The average gender-specific scores for personality traits confirm the graphical inspection from Figure 2: women score higher on all traits. However, we do not observe meaningful differences between the subsamples regarding demographic characteristics and parental background. Appendix Table A.2 describes the NEPS sample. The auxiliary data reveal that, on average, individuals from the relevant cohorts are approximately 6.5 years old upon school entry, slightly lower than the expected SSA according to the statutory cutoffs (ca. 6.6 years old). About 40% of individuals start school in the year of their

[11] The means are calculated after extending the pooled panel to ages 17-70, which yields 56,413 age-year observations.

seventh (instead of the sixth) birthday, which we define as old for grade. The overall compliance with the cutoff is high, reaching 75% despite the relatively generous exception rules covering almost one-third of individuals. No substantial gender-specific differences exist in school starting age and compliance with the cutoffs. The respective SOEP and NEPS samples are generally comparable regarding socio-demographic composition.

4 Empirical strategy

We aim to estimate the effect of a later versus earlier school entry on personality traits measured in adulthood. The actual timing of school enrollment might depend on a child's maturity, behavior, parental preferences, attitudes, and other unobservable factors that correlate with a child's development and later outcomes. To address this endogeneity issue, we exploit a plausibly exogenous variation in the expected timing of a child's school entry induced by the applicable school entry rule. Specifically, following recent studies on other outcomes (e.g., Landersø et al., 2017, Dhuey et al., 2019, Oosterbeek et al., 2021), we focus on the intention to treat (ITT) effect of being born after the cutoff within a regression discontinuity design (RDD).[12]

For this purpose, we estimate the following reduced-form equation

$$Y_{ict} = \beta \; After_i + f(run_i) + \pi_c + \pi_t + X_i'\delta + \varepsilon_{ict}, \tag{1}$$

where Y_{ict} is an outcome of individual i from birth cohort c in survey year t. Our main outcomes are the Big Five personality traits, which comprise openness to experience, conscientiousness, extraversion, agreeableness, and neuroticism. Before running the regressions, we standardize the outcomes to ease the interpretation and facilitate comparisons with earlier studies on the determinants of non-cognitive skills.

The main explanatory variable of interest is the indicator $After$, which takes the value of one for individuals born up to six months after the cutoff date and zero for those born up to six months before the cutoff. $f(run_i)$ denotes a control function in the running variable. In our main analysis, run corresponds to the month of birth, which is discrete, so we have to rely on a parametric control function. In our preferred

[12] Earlier studies typically used an instrumental variable approach, which scales the intention-to-treat estimate by the first-stage effect of school entry cutoff on school starting age (e.g., Puhani and Weber, 2008, Black et al., 2011, Fredriksson and Öckert, 2014). This facilitates an interpretation of the IV estimate as an effect of a one-year increase in school starting age. However, IV estimates reflect a combination of absolute versus relative age and starting early versus late, which might violate the underlying exclusion and monotonicity assumptions. Thus, we follow recent studies (e.g., Landersø et al., 2017, Dhuey et al., 2019, Landersø et al., 2020, Oosterbeek et al., 2021) and focus on reduced-form effects.

specification, $f(run_i)$ is a linear trend in which the slope may differ on either side of the cutoff, but we also present results adding quadratic trends. We normalize the month of birth to zero for the last month before the cutoff. Thus, run ranges from -5 to 6, measuring the relative distance between an individual's birthdate and the school entry cutoff. For robustness tests, we also use a finer-grained run at the level of the week of birth, which we observe for a 70% subsample in the SOEP data.[13] The weekly-level run ranges from -25 to 26, which allows us to also apply a non-parametric estimation.

Our main model specification includes birth cohort and survey year fixed effects (π_c and π_t, respectively). Birth cohort fixed effects flexibly capture any time-specific shocks that might have affected individuals born in different years. Within a given cohort, the control function $f(run_i)$ holds the age at observation constant. The survey year fixed effects net out any time effects that are specific to the measurement of the outcomes in different calendar years. X is a vector of observable characteristics that might correlate with the $After$ dummy and personality traits. In our main specification, X includes a gender-specific effect. Nevertheless, we also test the sensitivity of our main results to the inclusion of further covariates such as state of school enrollment and parental background characteristics. ε_{ict} is an error term.

The control function captures smooth trends in personality traits along an individual's distance from the cutoff. Thus, identification is achieved by the discrete jump in the timing of school entry at the cutoff. The intuition behind equation (1) is to compare the personality traits of adults who were born at roughly the same date, but who differ substantially in when they started formal education. Specifically, those born after the cutoff are expected to have started school one year later. However, since the sharp cutoff rule is not fully binding, the time difference at the cutoff will likely be smaller. We explore this issue in Section 5.1 using the NEPS-SC6 data.

The main identification assumption is that $f(run_i)$ is a sufficiently smooth function with no other discontinuity at the cutoff except for a different timing of school entry (Lee and Lemieux, 2010). We perform various tests to support the plausibility of this assumption. A potential threat could be that certain families time the child's birth in response to the expected school-entry cutoff. Although this is highly unlikely, to mitigate such concerns, we carefully examine whether there is a differential mass of births around the cutoff and find no evidence for potential manipulation.[14] To further sup-

[13] To keep the sample size and composition constant, we impute the missing information on the week of birth for the remaining 30% of the respondents. For this purpose, we use the information on the actual month of birth of a given respondent and randomly assign the exact date of birth within this month. The random component of this imputation procedure leads (if anything) to an attenuation bias in our regressions using the weekly running variable.

[14] Appendix Figure A.1 and Figure A.2 show that the distribution of births around the cutoff is smooth in

port the "as good as" random treatment assignment at the cutoff, we also check that the observable characteristics are balanced across the cutoff.[15]

In our empirical approach, β measures the local effect of a later school entry for the group of compliers, i.e., individuals whose school start was delayed solely because they were born after the administrative cutoff. We describe the average characteristics of this group in Section 5.1. Strictly seen, we also need the assumption that there are no defiers, i.e., no children born before (after) the cutoff delay (speed up) enrollment only because they are constrained by the law. Aliprantis (2012) and Barua and Lang (2016) show the monotonicity assumption is likely to be violated in the U.S. setting mostly due to the common redshirting decisions. Landersø et al. (2017, 2020) argue, however, that defiance is highly unlikely in the Danish context, which does not facilitate an earlier or later start by more than one year compared to the law-induced enrollment date. Following this argument, defiance seems also implausible in the German setting.

In Section 5.3, we perform a variety of sensitivity analyses, including specifications that include additional covariates, models with a more flexible function in the running variable, a donut-hole RDD, and nonparametric estimations using local polynomial regressions (e.g., Calonico et al., 2014, 2020). In our main analysis, we consider the widest possible bandwidths comprising all individuals born up to six months around the relevant cutoff date. Nevertheless, we show that our conclusions are robust to narrowing the bandwidths.

For inference, in the main analysis, we cluster the standard errors at the individual level to account for repeated outcome observations per person in the panel. In Section 5.3, we also report the results from alternative inference methods such as clustering by the running variable as earlier proposed in Lee and Card (2008), Eicker-Huber-White heteroskedasticity-robust standard errors as suggested in Kolesár and Rothe (2018), or clustering at the state level given that cutoff rules are state specific. While estimating the local polynomial regressions, we use the bias-corrected inference recommended in Calonico et al. (2020). The different inference methods lead to similar conclusions.

both surveys, regardless of whether we measure the distance to the cutoff in months or weeks. Consistent with the graphical inspection, using density tests for discrete running variables suggested by Frandsen (2017), we cannot reject the no-discontinuity hypothesis.

[15] Specifically, we regress a set of predetermined covariates on the $After$ dummy by using our main model specification as in equation (1). The results of these balancing tests are reported in Appendix Table A.3. We generally do not find systematic correlation patterns between the treatment variable and the observable characteristics, including family background and kindergarten attendance. The vast majority of the estimated coefficients are small and statistically insignificant. The only exception is some regional imbalance in the form of significantly lower proportions of individuals from Rhineland-Palatinate in the NEPS samples. Otherwise, the results largely support the assumption of smoothness.

5 Main results

5.1 Compliance with the cutoff date

In Section 2, we argued that compliance with the school entry laws in German is generally high and constant over time (see Figure 1). In the official statistics, however, children of families who take advantage of an early school entry exemption are counted as regular school entrants. Thus, compliance with the sharp cutoff will likely be lower and potentially selective. Hence, we first shed more light on these issues using the NEPS.

We begin with plotting the compliance against the running variable in Appendix Figure A.3. The figure confirms that, on average, nearly three-quarters of parents comply with the sharp cutoff. However, compliance is generally lower in the vicinity of the cutoff, and particularly, individuals born in the first three months after the cutoff are more likely to start school earlier than stipulated by the sharp cutoff date. This is not surprising given the relatively generous statutory exception rules in many states, which give families a legal option to enroll their child earlier.

In Figure 3, we examine the direct relationship between the $After$ dummy and the actual timing of school entry using two common timing measures: school starting age and the probability of being relatively old for the grade. Each data point represents the mean of the respective measure depending on the distance to the cutoff measured in months. Given the relatively high (though not perfect) compliance, we expect a significant discontinuity in these measures at the cutoff. In general, the patterns for the two timing measures are very similar, although mirrored; we observe a relatively smooth (downward or upward) trend for individuals born before the cutoff, followed by a large discontinuity of about 0.4 years, or 40 percentage points, immediately after the cutoff. The underlying trends in the running variable on either side of the cutoff are fairly linear, which justifies our choice of $f(run_i)$ in the main specification. Using weekly-level information on date of birth (see Appendix Figure A.4), we find that the quadratic fits, if anything, are largely driven by children born one to two weeks around the cutoff. The overall patterns are very similar for men and women.

We confirm the magnitude of the discontinuities in a regression framework (see Appendix Table A.4). In the first column, we use our main model specification, which includes a separate linear trend in the monthly running variable on either side of the cutoff, cohort, survey year fixed effects, and a female dummy. The point estimates remain remarkably stable when we extend the model to include family background characteristics (column 2), state fixed effects (column 3), and other policy changes (column 4), or when we drop all controls (column 5). This suggests that compliance with the cutoff is not highly selective and is consistent with compliers having, on average, similar background characteristics to non-compliers (see Appendix Table A.5).

The estimated effects on the timing of school entry are somewhat weaker when we add quadratic trends in the running variable (column 6), reflecting the graphical evidence of relatively higher noncompliance right at the cutoff. In general, as long as not selective, the locally higher noncompliance would lead to an attenuation bias in our main results in Section 5.2 if $f(run_i)$ was specified too flexibly. We illustrate this issue using the weekly running variable to estimate donut-hole types of regressions (see Appendix Table A.6). Indeed, we find that the results from the linear and quadratic specification converge when we exclude observations close to the cutoff. This suggests that our choice of a linear $f(run_i)$ helps to limit the potentially confounding effects of the substantial noncompliance at the cutoff in full-sample regressions.

Taken together, the German institutional context implies that a child's date of birth significantly affects the timing of school enrollment. The estimate of around 0.4 is generally in line with previous findings for Germany from different samples (e.g., Puhani and Weber, 2008, Mühlenweg and Puhani, 2010, Görlitz et al., 2022).[16] This effect size implies that we can scale the reduced-form effects on personality traits estimated in Section 5.2 from the SOEP by a factor of 2.5 to interpret them as instrumental variable (IV) estimates of starting school one year later. Interestingly, we do not find substantial differences between men and women in the magnitude of the first-stage effect. Thus, gender-specific differences in the potential effects of the cutoff rules on long-term outcomes cannot be attributed to gender differences in compliance.

5.2 Effects on personality traits

In this section, we present our main results on the consequences of the cutoff rules for personality traits measured in adulthood. Similar to Section 5.1, we begin with a series of plots that relate mean scores on the Big Five personality traits to the month of birth (see Figure 4). Each graph plots the first- and second-order polynomials in the running variable separately fitted on either side of the cutoff. In most cases, the linear and quadratic trends produce similar extrapolations near the cutoff. Some notable exceptions are clearly due to relatively more noise near the cutoff, likely due to the lower compliance. This can be seen more clearly in similar plots using week of birth as the running variable (see Appendix Figure A.5).

[16] Similar discontinuities have also been found in Dutch data (0.4, see Oosterbeek et al. (2021)). Studies from Norway and Sweden typically estimate higher first-stage effects (of approximately 0.8, see, e.g., Black et al. (2011), Fredriksson and Öckert (2014) while the average compliance in Denmark seems to be much lower (of approximately 0.2, see, e.g., Landersø et al. (2017).

The first column of graphs covers the entire sample and consistently suggests that individuals born after the school entry cutoff tend to score slightly lower on agreeableness and neuroticism in adulthood. The remaining personality traits appear to be balanced across the cutoff, although in some cases, this conclusion depends on the functional form of the running variable (with smaller discontinuities if the trends are linear). Stratifying the sample by gender in the middle and right panels uncovers some interesting heterogeneities. Specifically, men born before and after the cutoff display slightly more pronounced differences in conscientiousness than women. In contrast, the discontinuity in neuroticism levels seems to be larger for women than for men. Nevertheless, if anything, the differences between the earlier and later-born individuals are relatively small, and the patterns are noisy except for the lower levels of neuroticism for women born after the cutoff.

To estimate the size of the discontinuities at the cutoff, we follow the approach described in Section 4. Panel A of Table 2 displays the results for the entire sample. Each coefficient on the $After$ dummy comes from a separate RDD estimation of equation (1). All regressions include linear trends in the month of birth fitted separately on either side of the cutoff, cohort, and survey year fixed effects, and a gender dummy. The results suggest that being born after the cutoff significantly reduces levels of neuroticism by almost 0.06 standard deviations (SD), on average. The estimated effects on the remaining traits are smaller and statistically insignificant.

Given that earlier literature points to substantial gender-specific differences in personality traits (e.g., Gensowski et al., 2021), we might expect potentially different responses for men and women and split the sample by gender in Panels B and C. We find that the negative effect on neuroticism is entirely driven by women. For the remaining traits, the coefficients for men and women often go in opposite directions but are generally small in magnitude and imprecisely estimated. In Panel D, we again pool men and women, but extend the model specification by interacting the $After$ indicator with a $Female$ dummy. The coefficient on the interaction term confirms that the effect on neuroticism is about 0.1 SD larger for women than for men.

To illustrate the magnitude of this reduced-form effect, we can compare it to the gender gap in this trait of 0.470 SD estimated within the same regression. This implies that the enrollment cutoffs have an economically important policy effect of 20% on the compression of the gender gap in neuroticism. Relating the reduced-form effect to the corresponding first-stage effect (see Appendix Table A.4) implies that, for women, a one-year increase in school starting age reduces average adult levels of neuroticism by 0.266 SD. In Section 5.3, we document that these results are robust to the inclusion of additional controls and address other concerns about our main model specification.

For the regressions in Table 2, we estimate the average effects in adulthood by pooling all age-specific assessments of personality traits available for each person in the panel.[17] In Figure 5, we exploit the panel structure of our data to examine how the effect evolves over the life cycle. Each subfigure summarizes the results of 36 separate age-specific estimations of equation (1).[18] Although not all of the estimates are statistically significant (presumably due to smaller sample sizes), the patterns confirm that women born after the cutoff have lower levels of neuroticism throughout most of the prime working ages.

Appendix Figure A.6 shows the estimated effects on personality traits over the life cycle, separately for men and women. For men, the age-specific effects are mostly statistically insignificant and centered around zero, but there is suggestive evidence of negative effects on openness and extraversion around childbirth, which is consistent with the literature (Galdiolo and Roskam, 2014). The effects for women are also mostly insignificant, but the point estimates follow slightly different patterns. For example, some positive effects on conscientiousness and agreeableness occur temporarily between the ages of 30 and 40, which typically coincides with child-rearing. In contrast, positive effects on openness to experience and extraversion occur in the late 40s and 50s, which may reflect interactions with other major life events such as children leaving the nest or divorce (Spikic et al., 2021).

5.3 Robustness analysis

To ensure that our main findings for neuroticism are not driven by particular data choices or model specifications, we conduct several standard sensitivity tests in Figure 6. The corresponding results for all personality traits are included in Appendix Figure A.7. For comparison, the first set of estimates on the right repeats our baseline results. We first show that our results remain almost identical even when we include additional covariates such as family background characteristics (test A) or state fixed effects (test B). We also obtain similar results after controlling for other policy changes that occurred in several states during the period under study, such as the extension of compulsory schooling and the short school years (test C).

[17] As mentioned in Section 3, we reweight the person-year observations to give each person an equal weight in these pooled regressions. In Section 5.3, we show that the results are nearly identical when we alternatively use only one observation per person by replacing the outcomes with individual-specific means.

[18] To reduce random fluctuations and slightly smooth the patterns in Figure 5, we use five-year age intervals based on adjacent years. For example, the first point estimate corresponds to the coefficient obtained from the age group 23-27, where age 25 is the midpoint of the age interval.

The estimated effects are also robust to including quadratic trends in the monthly running variable, safe for wider confidence intervals, and a larger point estimate for men (test D). The latter appears to be driven by observations close to the cutoff, as the point estimate falls to zero when we drop individuals born one month before and after the cutoff (test E). Our results also remain remarkably stable when we use the more granular weekly running variable to estimate our baseline specification (test F), a donut-hole specification that excludes two weeks on either side of the cutoff (test G), the quadratic specification for the trends in the running variable (test H), and even more flexible local polynomial regressions (test I).[19] Similarly, restricting the bandwidths around the cutoff to five months does not affect our baseline results (test J).

We also address potential concerns related to some specific data choices. First, instead of estimating pooled panel regressions with repeated observations for each individual, we include each individual in the estimations only once by defining the outcomes as individual-specific means (test K). The results are almost identical. Second, we extend the age range to 17 to 70, which does not change our conclusions (test L). In the remaining panels, we show that our conclusions also hold when we apply alternative inference methods. Specifically, instead of clustering the standard errors at the individual level, we estimate robust standard errors (test M), and cluster at the level of the running variable (test N) or the state level, since the enrollment cutoffs vary across states (test O). However, given the small number of states in Germany, clustering at the state level may be subject to finite sample problems, so we also estimate a conservative version of the Wild cluster bootstrap (Cameron et al., 2008), which gives less precision but confirms our results (test P).[20]

In general, the various sensitivity tests support our main conclusions that girls who enter school relatively later enjoy a significant reduction in neuroticism levels in adulthood. The effects for males are consistently statistically insignificant and mostly very close to zero. Similarly, for the remaining personality traits, most of the alternative specifications do not yield statistically significant and economically meaningful effects regardless of gender (see Appendix Figure A.7).[21] However, as we mention in Sec-

[19] Specifically, we estimate the robust bias-corrected estimator proposed by Calonico et al. (2020) by using the authors' recommendations for first-order polynomial (i.e., local linear regression) to construct the point estimator and second-order polynomial (i.e., local quadratic regression) to construct the bias correction.

[20] We use the recommended procedure with 999 replications, imposing the null hypothesis, and using the Rademacher weights (Cameron and Miller, 2015, Roodman et al., 2019). Its immediate result is the p-value for the null hypothesis. Thus, the resulting confidence intervals are not necessarily symmetric.

[21] The significant effect on openness after including the quadratic trends in the month of birth (test D) is a striking exception, which we attribute to data variability directly at the cutoff. Note that this effect disappears completely for women and becomes negative for men in a donut-hole regression (test E),

tion 5.2, apart from the remarkably stable effect on neuroticism, effects on other personality traits may emerge at different life stages (see Appendix Figure A.6). This highlights the importance of a life-cycle perspective, which we see as a promising direction for future research.

6 Potential Mechanisms

Our findings suggest a persistent beneficial effect of a later school start for girls, as reduced neuroticism is linked to improved well-being and better socioeconomic outcomes over the life cycle (Gensowski et al., 2021). These long-term effects on personality traits align with the dynamic model of skill formation, which predicts that early differences in skill development can have lasting consequences through self-productivity and skill complementarity (Cunha and Heckman, 2007, 2008).

One potential channel in this dynamic is education, which may also directly affect personality (Dahmann and Anger, 2014, Bach et al., 2019). Although we cannot directly test the distinct roles of self-productivity and skill complementarity in our data, we analyze the effects of being born after the cutoff on educational attainment as an intermediate outcome in Appendix Table A.7. Using data from the SOEP and the NEPS, we estimate effects for the full sample, separately by gender, and using interactions.

Appendix Table A.7 column 1 presents the effects on tracking after primary school, using attendance of *Gymnasium* as an outcome. Consistent with previous evidence (e.g., Mühlenweg and Puhani, 2010, Schneeweis and Zweimüller, 2014, Oosterbeek et al., 2021), the overall effect is positive and significant. However, while for girls, being born after the cutoff significantly increases the probability of enrolling in the academic track by 5.4 percentage points (pp), the positive effect on boys is insignificant.

A similar pattern holds for the estimates in column 2 that examine the effect of school entry timing on obtaining a high school degree, i.e. (*Fach-*)*Abitur*. While the overall effect is positive and insignificant, it is again largely driven by girls, who are 5 pp more likely to complete high school if born after the cutoff. The interaction model in panel D suggests stronger effects for girls for this outcome. The findings are consistent with previous research that shows the positive effects of a later school start on girls' years of schooling (e.g. Fredriksson and Öckert, 2014, Black et al., 2011). Column 4 repeats the analysis using the SOEP. Here, the overall effect remains positive and is significant, but the effect for girls turns insignificant. Instead, the overall positive effect is driven by men.

and is close to zero in all specifications using the weekly running variable (tests F to H).

Columns 3 and 5 present the effects of a later school start on obtaining a college degree, using data from the NEPS and SOEP, respectively. Overall, the effects are mixed, with small and insignificant estimates in the NEPS and slightly positive estimates in the SOEP sample. While our results on college attainment are inconclusive, they are in line with the literature that generally finds fading effects on education (e.g., Oosterbeek et al., 2021), but acknowledges potential effects on university attendance in certain contexts (Bedard and Dhuey, 2006).

Overall, our results on educational attainment largely align with the literature that suggests that early advantages of later school starters might be more pronounced and persistent in selective systems featuring early tracking (e.g., Bedard and Dhuey, 2006, Fredriksson and Öckert, 2014, Oosterbeek et al., 2021). Indeed, previous research shows that the timing of school entry has important consequences for the secondary school track placement in Germany (e.g., Puhani and Weber, 2008, Mühlenweg and Puhani, 2010, Dustmann et al., 2017), and even high school graduation (Görlitz et al., 2022). We confirm these but do not find systematic differences between boys and girls that might explain gendered effects on personality development.

Beyond educational exposure, two of the main mechanisms discussed in the literature on school starting age are absolute and relative age at entry (e.g., Cornelissen and Dustmann, 2019).[22] Absolute age refers to a child's biological age at school entry, which, in line with the *maturity principle* (Caspi et al., 2005), gives older children an emotional and cognitive advantage in handling structured environments and stress (e.g., Lubotsky and Kaestner, 2016), potentially improving emotional stability. Relative age introduces social comparison dynamics, where older children may benefit from leadership roles or more favorable peer (e.g., Dhuey and Lipscomb, 2008) and teacher evaluations (e.g., Nicodemo et al., 2024), potentially reinforcing self-confidence and emotional regulation. But why do we find effects, particularly on neuroticism for women?

Gender differences in internalizing and externalizing behavior are a common stylized fact in psychology (e.g., Achenbach et al., 1991, Leadbeater et al., 1999) and are even associated with differential teacher referrals of children to mental health services (Pearcy et al., 1993). Moreover, younger students often suffer from bullying, lower self-confidence, and more negative self-perceptions due to peer comparisons and teacher biases (e.g., Kretschmann et al., 2021, Crawford et al., 2011, 2014). This suggests that the interaction between relative age and gender bias may contribute to higher levels of

[22] Since the Big Five personality traits are measured in adulthood, where they remain relatively stable (e.g., Specht et al., 2011, Cobb-Clark and Schurer, 2012, 2013, Elkins et al., 2017, Gensowski et al., 2021), we do not consider age-at-test as a mechanism.

neuroticism in girls, as they are more prone to internalizing peer and teacher evaluations. The positive effects of higher maturity at school entry on emotional stability in adulthood, particularly among women, align with the psychological literature. Additionally, recent economic studies also suggest gendered effects of school starting age on non-cognitive skills (e.g., Page et al., 2019, Shin, 2023).

Neuroticism, characterized by emotional sensitivity and anxiety, is particularly influenced by school entry timing. Girls, who generally score higher on neuroticism from adolescence onward (e.g., Vecchione et al., 2012, Van den Akker et al., 2014), may benefit from starting school later, allowing more time to develop emotional resilience (e.g., De Bolle et al., 2015, Soto, 2016). Family environments also complement school influences in shaping non-cognitive outcomes. Bertrand and Pan (2013) highlight that family structure, particularly in disrupted families, plays a key role in non-cognitive development, suggesting that school entry timing interacts with both family dynamics and gendered social influences. Implicit gender biases in both settings (e.g., Carlana, 2019) may further exacerbate the effects of relative age for girls, reinforcing the importance of school entry-timing for long-term emotional development.

7 Conclusions

Despite a large body of evidence on the effects of school start timing on child development, educational attainment, and labor market success (e.g., Bedard and Dhuey, 2006, Puhani and Weber, 2008, Elder and Lubotsky, 2009, Black et al., 2011, Fredriksson and Öckert, 2014, Røed-Larsen and Solli, 2017, Dhuey et al., 2019), little attention has been paid to its impact on non-cognitive skill development. This paper addresses this gap by analyzing the long-term effects of school entry timing on personality traits.

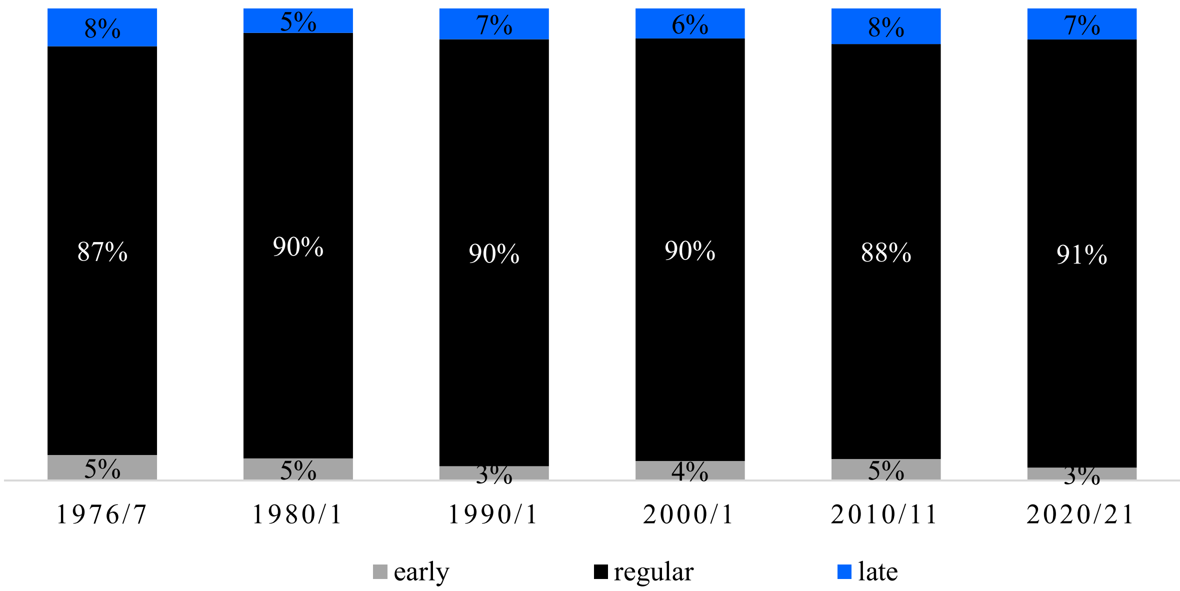
We exploit institutionally induced variation in school entry timing using a regression discontinuity design based on school enrollment cutoff rules in Germany. This context is particularly suitable, as there is no automatic link between school starting age and compulsory schooling duration, and compliance with entry laws is high (e.g., Puhani and Weber, 2008, Mühlenweg and Puhani, 2010, Dustmann et al., 2017, Görlitz et al., 2022). Using data from the German Socio-Economic Panel (SOEP), we focus on the Big Five personality traits, which are determinants of individual outcomes across many life domains (e.g., Almlund et al., 2011, Gensowski et al., 2021).

We find that relatively older school entrants experience a persistent reduction in neuroticism, particularly among women. Neuroticism, associated with emotional instability and negative socioeconomic outcomes (e.g., Almlund et al., 2011, Lundberg, 2013, 2018), is significantly reduced for women who start school later. This aligns with recent evidence that suggests gendered effects on non-cognitive skills (e.g. Page et al., 2019, Shin, 2023). These persistent effects may help mitigate gender disparities in skill endowments, with implications for labor market outcomes. This is consistent with evidence from Sweden and Germany showing positive effects of a later school start on women's earnings, with no corresponding benefit for men (Fredriksson and Öckert, 2014, Cygan-Rehm and Westphal, 2024).

Our analysis of educational outcomes suggests that school entry timing influences academic tracking, particularly benefiting girls, further reinforcing gender-specific effects. The timing of school entry affects personality development, particularly through peer and teacher comparisons. This is especially relevant for girls, who may be more sensitive to such dynamics. Both family and school environments appear to jointly shape non-cognitive traits, particularly neuroticism, over the long term, suggesting that school entry timing plays a key role in gendered personality development.

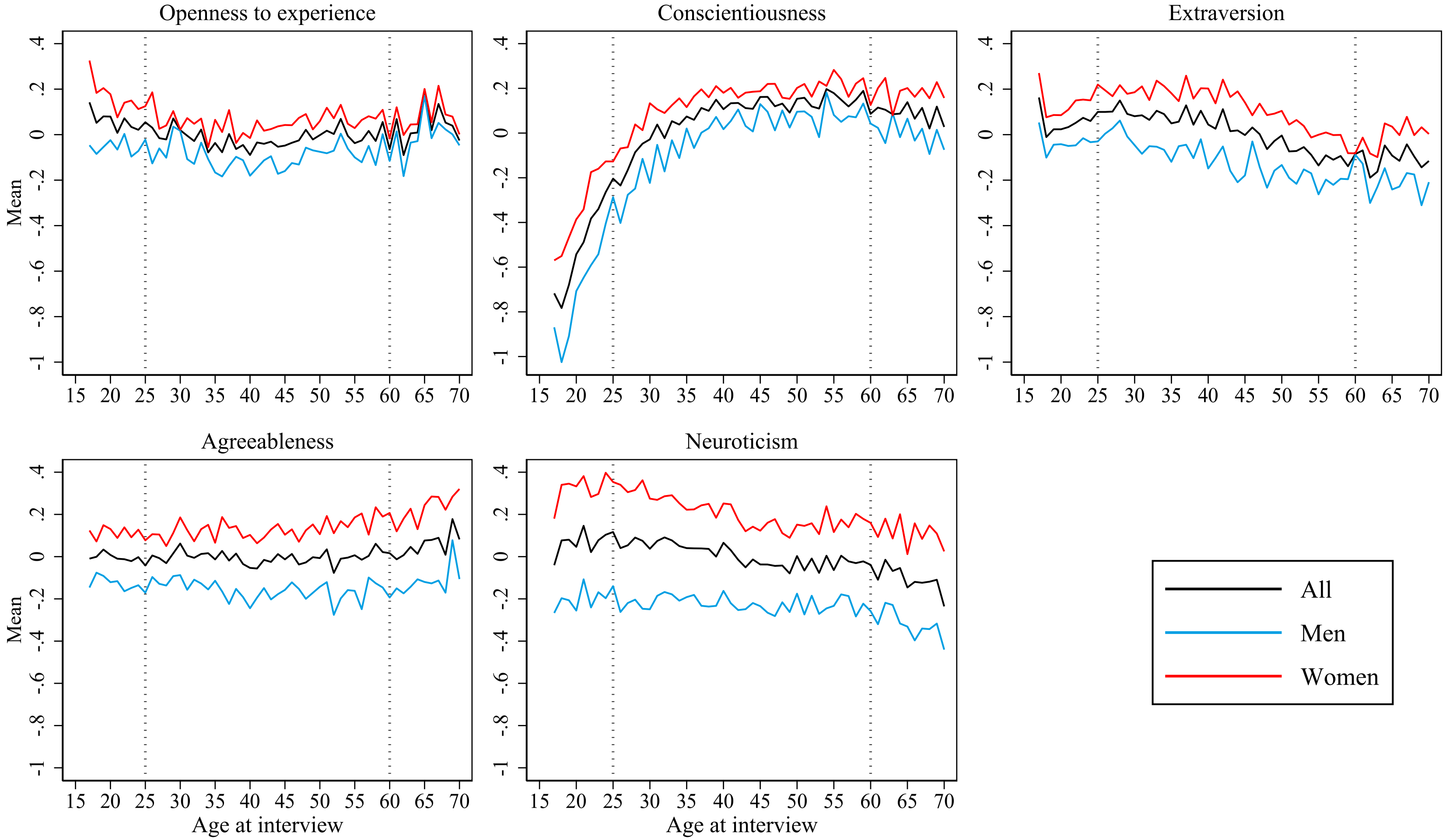
Overall, these findings emphasize the need for policymakers to consider how school entry rules affect not only educational outcomes but also long-term socio-emotional development. Given the relationship between non-cognitive skills and labor market success (e.g., Bowles et al., 2001, Heineck and Anger, 2010, Gensowski et al., 2021), a lifetime perspective is essential for understanding socio-economic disparities.

Figure 1: Primary school entrants by the type of enrollment



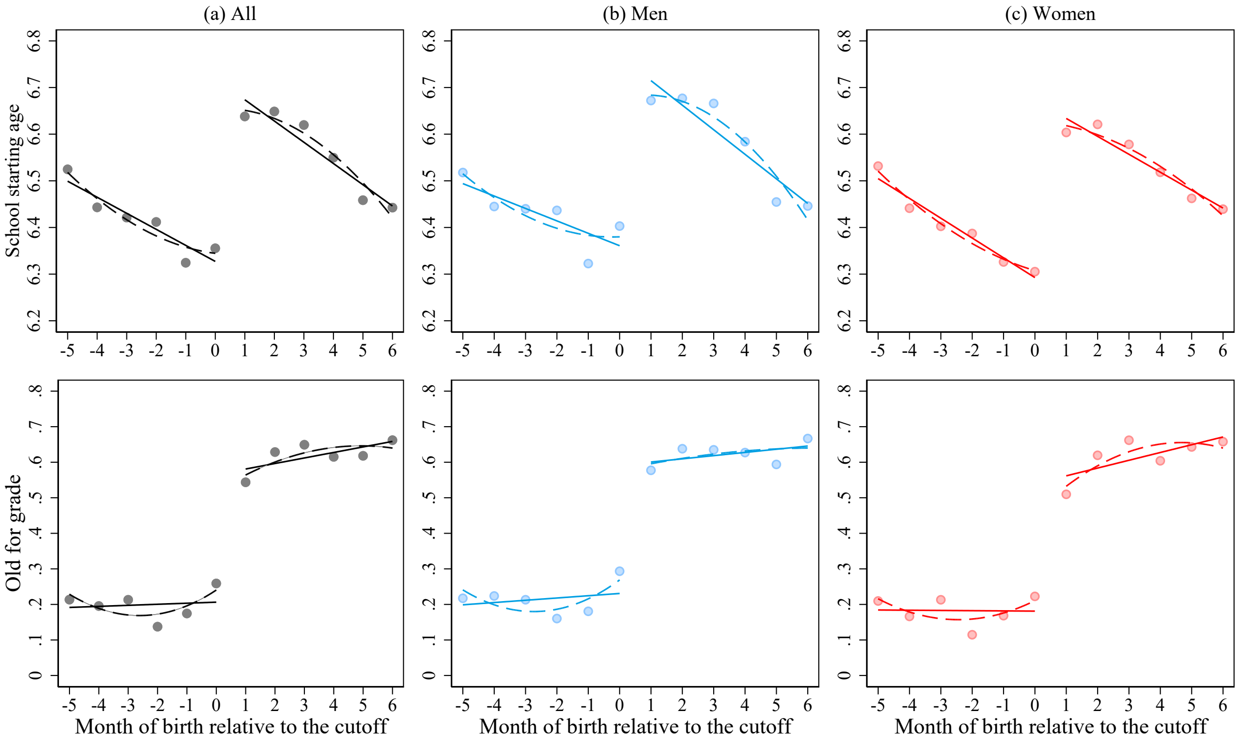
Note: The figure shows the relative numbers of students enrolled in a particular school year by the enrollment type. Data before the school year 1976/7 are not available. Until 1990/1, the numbers include only West German states (incl. West Berlin). Since 2000/1, east German states are also included.
Source: DESTATIS (various years).

Figure 2: Age-specific profiles in personality traits



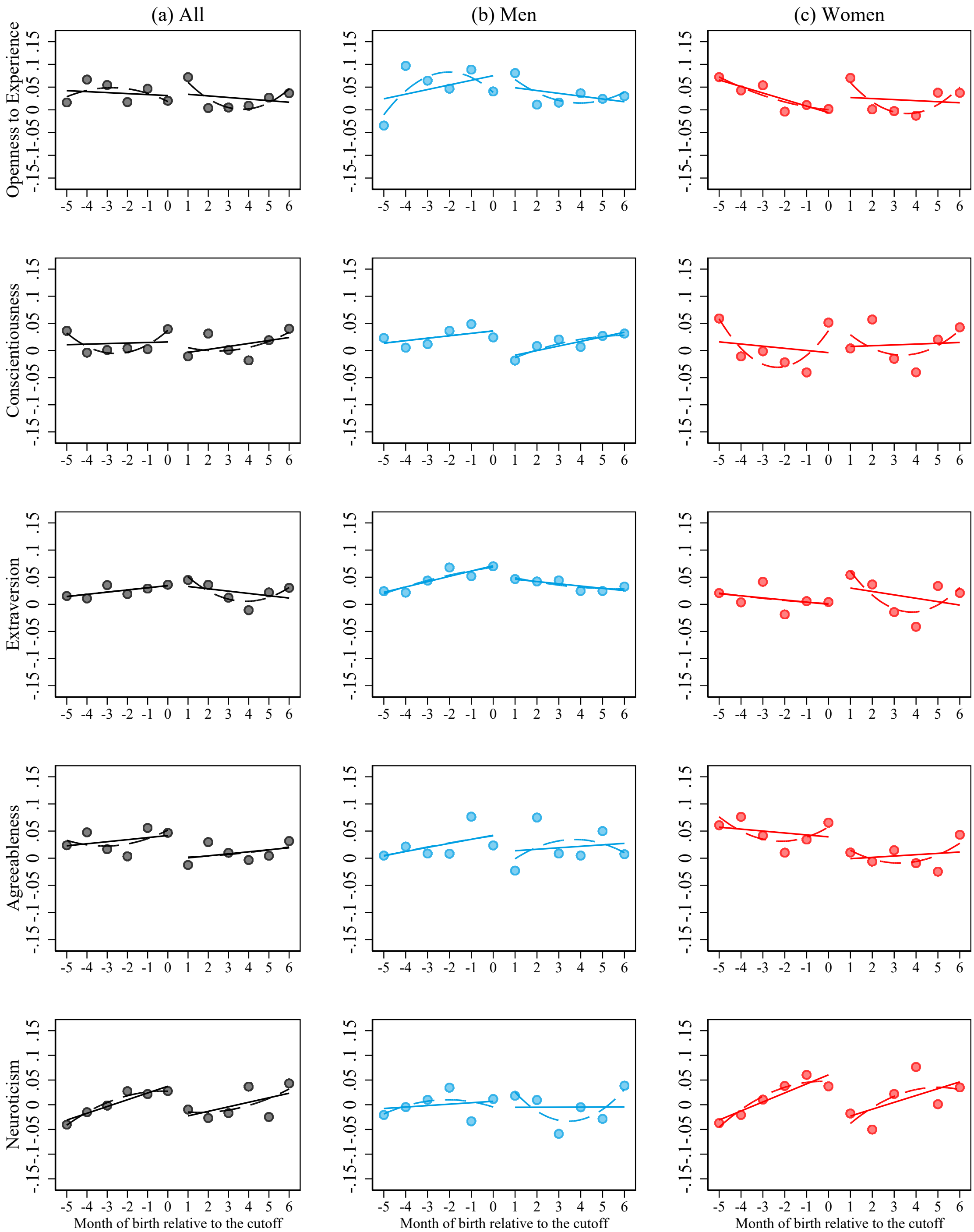
Note: Sample restricted to individuals born and enrolled in school in West German states (excl. Berlin). Personality traits are standardized using the mean and standard deviation of the full sample.
Source: SOEP (doi:10.5684/soep.core.v38.1).

Figure 3: Being born after the cutoff and the timing of school entry



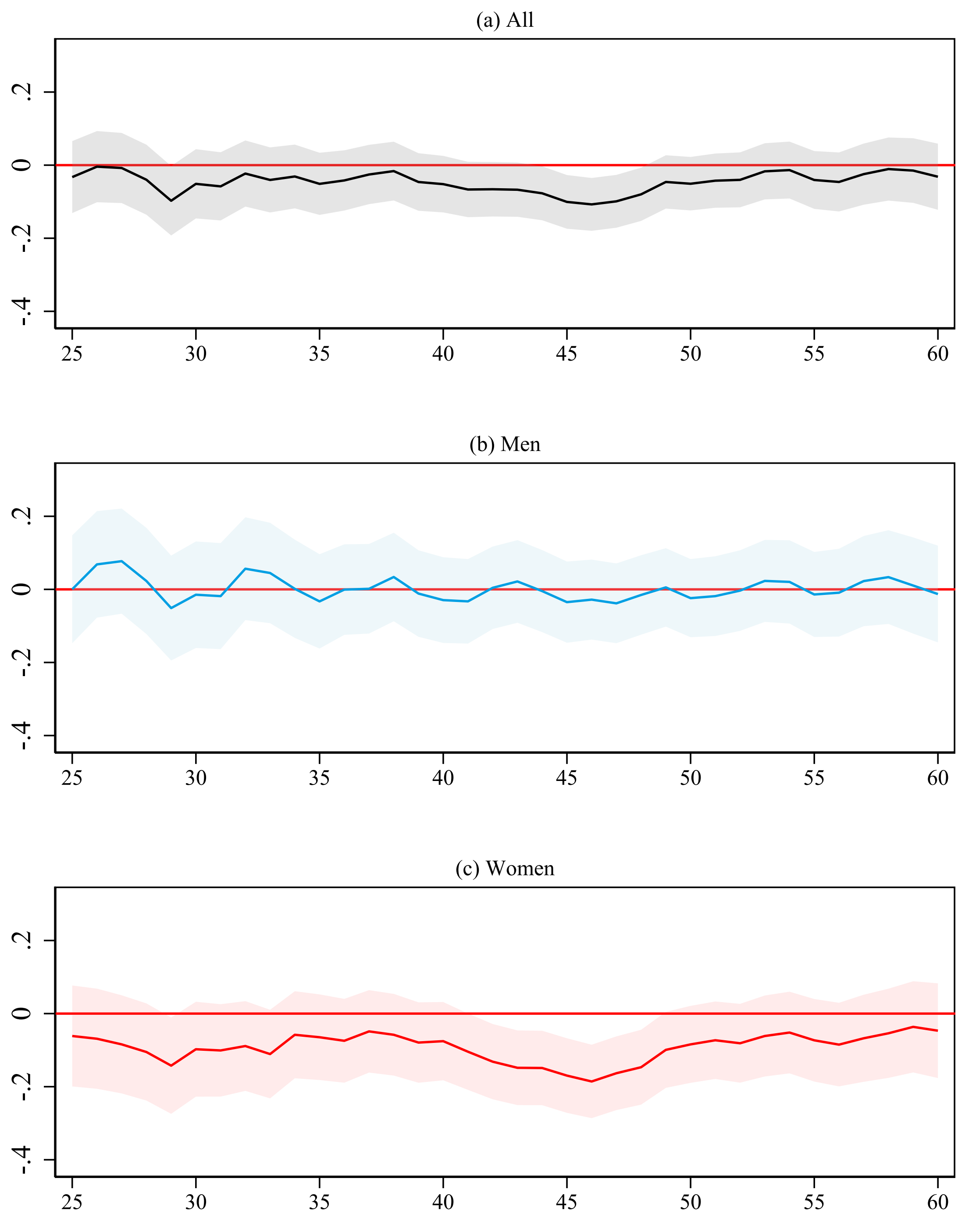
Note: School starting age (in years) is calculated as a difference between the date of a child's school entry and its date of birth. Old for grade indicates school entry in the year of a child's seventh birthday (as opposed to sixth). The solid (dashed) lines represent first (second) order polynomials in the running variable (month of birth) fitted separately to the data on each side of the cutoff
Source: NEPS-SC6 (doi:10.5157/NEPS:SC6:12.0.1).

Figure 4: Personality traits by distance to the cutoff



Note: Sample restricted to individuals born and enrolled in school in West German states (excl. Berlin). Personality traits are standardized using the mean and standard deviation within each sample. *Source:* SOEP (doi:10.5684/soep.core.v38.1).

Figure 5: Age-specific effects on neuroticism



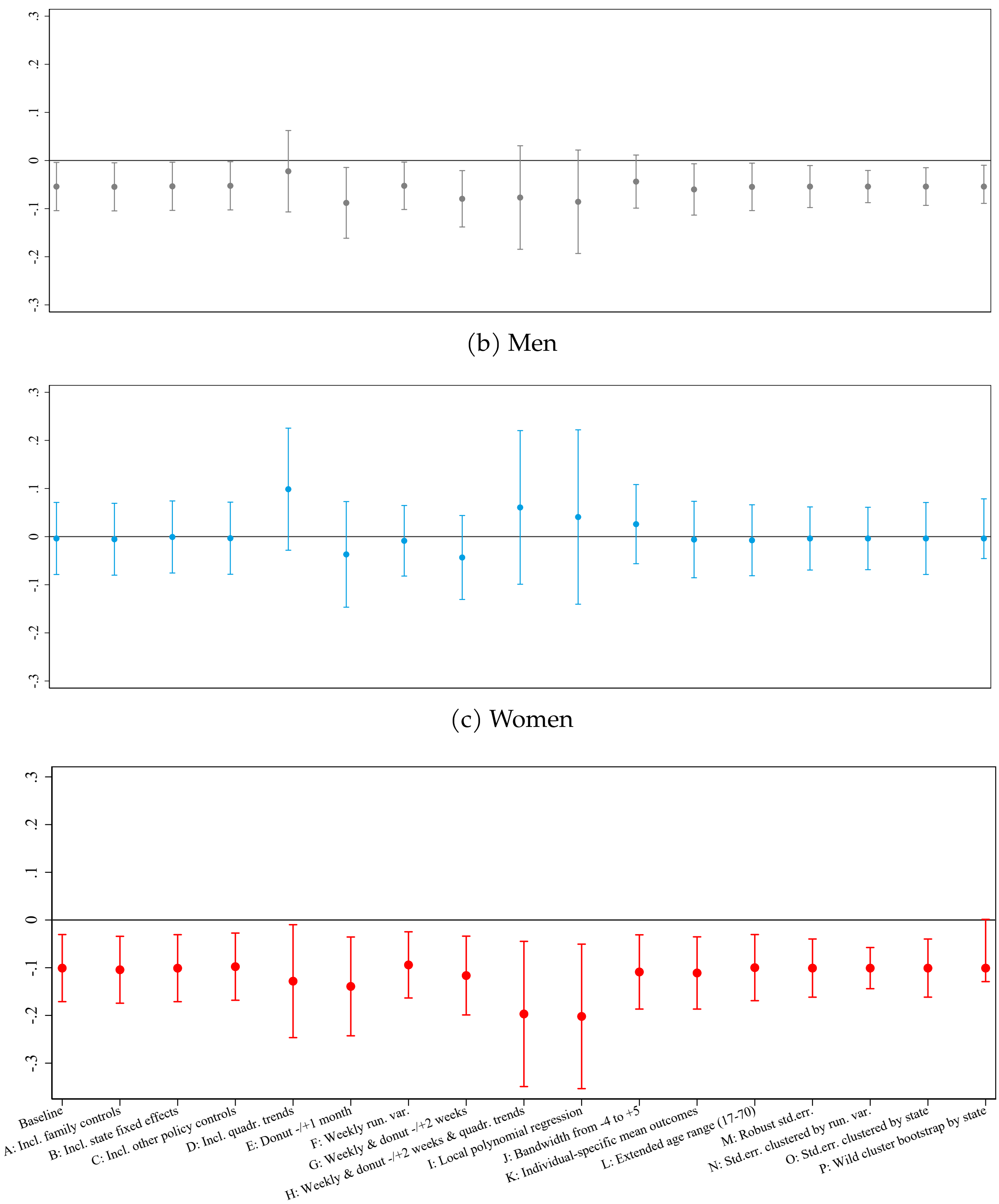
Note: The outcomes are standardized within each estimation sample. Each subfigure plots 36 point estimates on the *After* dummy. Each estimate comes from a separate linear regression of equation (1). All regressions include linear trends in the running variable (month of birth) fitted separately on either side of the cutoff, cohort fixed effects, survey year fixed effects, and a gender dummy. The shaded areas represent 95% confidence intervals around the respective point estimate obtained from standard errors clustered at the individual level.
Source: SOEP (doi:10.5684/soep.core.v38.1).

Figure 6: Robustness analysis: Average effects on neuroticism

(a) All

(b) Men

(c) Women



Note: The outcomes are standardized within each estimation sample. Each point estimate on the *After* dummy comes from a separate linear regression of equation (1). All regressions include linear trends in the running variable (month of birth) fitted separately on either side of the cutoff, cohort fixed effects, and survey year fixed effects. If not stated differently, the 95% confidence intervals around the respective point estimate are obtained from standard errors clustered at the individual level.
Source: SOEP (doi:10.5684/soep.core.v38o).

Table 1: Sample means

Estimation sample	All Mean (SD)	Men Mean (SD)	Women Mean (SD)
A: Outcomes			
Openness to experience	4.63 (1.18)	4.55 (1.15)	4.71 (1.19)
Conscientiousness	5.85 (0.90)	5.78 (0.91)	5.91 (0.87)
Extraversion	4.97 (1.14)	4.84 (1.15)	5.09 (1.12)
Agreeableness	5.36 (0.96)	5.21 (0.97)	5.50 (0.93)
Neuroticism	3.73 (1.27)	3.44 (1.21)	3.99 (1.26)
B: Individual characteristics			
Female	0.52	0.00	1.00
Year of birth	1968.64 (11.27)	1968.39 (11.36)	1968.87 (11.19)
Month of birth	6.37 (3.44)	6.35 (3.45)	6.39 (3.42)
Born after the cutoff (treatment)	0.50	0.50	0.50
Eligible for early-enrollment exception	0.38	0.38	0.38
Age at interview	43.54 (10.08)	43.84 (10.17)	43.27 (9.99)
State: Schleswig-Holstein	0.04	0.04	0.04
State: Hamburg	0.02	0.02	0.02
State: Lower Saxony	0.13	0.13	0.13
State: Bremen	0.01	0.01	0.01
State: North Rhine-Westphalia	0.28	0.28	0.28
State: Hesse	0.09	0.09	0.09
State: Rhineland-Palatinate	0.06	0.06	0.06
State: Baden-Wuerttemberg	0.15	0.16	0.15
State: Bavaria	0.20	0.20	0.20
State: Saarland	0.01	0.01	0.01
Mother's age at birth (yrs)	27.33 (5.68)	27.36 (5.76)	27.31 (5.60)
Mother's age at birth missing	0.05	0.05	0.04
Father's age at birth (yrs)	30.62 (6.56)	30.64 (6.59)	30.61 (6.54)
Father's age at birth missing	0.06	0.06	0.06
Parental education (yrs)	9.93 (1.54)	9.91 (1.52)	9.94 (1.55)
Parental education missing	0.06	0.06	0.06
Migrant parent(s)	0.09	0.09	0.09
Person-year observations	42,052	19,561	22,491
Individuals	20,491	9,740	10,751

Note: Samples restricted to individuals enrolled in school in West German states (excl. Berlin). The means are calculated using all panel observations available for a given individual between age 25 and 60 and weighted by the inverse of the number of times each individual appears in the panel.
Source: SOEP (doi:10.5684/soep.core.v38.1).

Table 2: Reduced-form effects of being born after the cutoff on personality traits

	(1)	(2)	(3)	(4)	(5)
	Openness	Conscientious.	Extraversion	Agreeableness	Neuroticism
Panel A: All					
After	0.009	-0.013	0.006	-0.037	-0.054**
	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
Obs.			42,052		
Individuals			20,491		
Panel B: Men					
After	-0.024	-0.046	-0.027	-0.035	-0.004
	(0.039)	(0.038)	(0.039)	(0.038)	(0.038)
Obs.			19,561		
Individuals			9,740		
Panel C: Women					
After	0.039	0.019	0.042	-0.042	-0.101***
	(0.036)	(0.036)	(0.036)	(0.035)	(0.036)
Obs.			22,491		
Individuals			10,751		
Panel D: All					
After	-0.029	-0.044	-0.032	-0.035	-0.004
	(0.037)	(0.038)	(0.039)	(0.038)	(0.036)
After x Female	0.071	0.059	0.073	-0.004	-0.094*
	(0.052)	(0.051)	(0.052)	(0.051)	(0.050)
Female	0.106**	0.127***	0.182***	0.305***	0.470***
	(0.026)	(0.026)	(0.027)	(0.026)	(0.026)
Obs.			42,052		
Individuals			20,491		

Note: The outcomes are standardized within each estimation sample. Each point estimate on the *After* dummy comes from a separate linear regression of equation (1). All regressions include linear trends in the running variable (month of birth) fitted separately on either side of the cutoff, cohort fixed effects, and survey year fixed effects. Panels A and D also include a gender dummy. In Panel D, the linear trends in the running variable are interacted with the gender dummy. Standard errors in parentheses are clustered at the individual level. Stars indicate statistical significance at the 1% (***), 5% (**), and 10% (*) level.
Source: SOEP (doi:10.5684/soep.core.v38.1).

Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work, the authors used *ChatGPT 4o* in order to conflate ideas, get feedback on logical reasoning, and for table formatting. After using these tools/services, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

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Data statement

This paper uses proprietary data from the German Socio-economic Panel (SOEP) and the National Educational Panel Study: Starting Cohort 6 (NEPS-SC6) that cannot be published. However, the data can be requested (e.g., for replication purposes) from the Research Data Centers (FDZ) of the Socio-economic Panel (SOEP) at the German Institute for Economic Research (DIW Berlin) and the Leibniz Institute for Educational Trajectories (LIfBi), respectively. The authors are willing to assist.

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Appendix

Table A.1: Measurement of the Big Five Inventory in the SOEP

Question (answer on a 7-point scale each)	Personality trait
I consider myself as someone who...	
• *is original, comes up with new ideas*	**openness to experience**
• *values artistic, aesthetic experiences*	
• *has an active imagination*	
• *does a thorough job*	**conscientiousness**
• *does things effectively and efficiently*	
• *tends to be lazy* [reversed]	
• *is communicative, talkative*	**extraversion**
• *is outgoing, sociable*	
• *is reserved* [reversed]	
• *is sometimes somewhat rude to others* [reversed]	**agreeableness**
• *has a forgiving nature*	
• *is considerate and kind to others*	
• *worries a lot*	**neuroticism**
• *gets nervous easily*	
• *is relaxed, handles stress well* [reversed]	

Source: SOEP (doi:10.5684/soep.core.v38.1).

Table A.2: Sample means - NEPS data

	All		Men		Women	
	Mean	(SD)	Mean	(SD)	Mean	(SD)
A: Variables related to school start						
Actual school starting age	6.49	(0.83)	6.51	(1.00)	6.47	(0.60)
Expected school starting age	6.58	(0.31)	6.57	(0.31)	6.58	(0.31)
Old for grade	0.41		0.41		0.40	
Born after the cutoff (treatment)	0.50		0.49		0.51	
Compliance with the cutoff	0.73		0.72		0.75	
Eligible for early-enrollment exception	0.32		0.32		0.32	
B: Individual characteristics						
Female	0.50		0.00		1.00	
Year of birth	1963.91	(10.78)	1963.78	(11.01)	1964.03	(10.55)
Month of birth	6.36	(3.41)	6.35	(3.40)	6.38	(3.43)
Age at first interview	44.87	(11.25)	45.01	(11.56)	44.73	(10.94)
State: Schleswig-Holstein	0.04		0.04		0.04	
State: Hamburg	0.02		0.02		0.02	
State: Lower Saxony	0.14		0.14		0.13	
State: Bremen	0.01		0.01		0.01	
State: North Rhine-Westphalia	0.29		0.29		0.28	
State: Hesse	0.09		0.09		0.09	
State: Rhineland-Palatinate	0.07		0.07		0.06	
State: Baden-Wuerttemberg	0.15		0.14		0.16	
State: Bavaria	0.18		0.18		0.18	
State: Saarland	0.02		0.02		0.02	
Mother's age at birth (yrs)	28.03	(6.08)	28.02	(6.13)	28.04	(6.03)
Mother's age at birth missing	0.03		0.04		0.02	
Father's age at birth (yrs)	31.30	(6.89)	31.35	(6.98)	31.24	(6.80)
Father's age at birth missing	0.05		0.05		0.04	
Parental education (yrs)	9.98	(1.53)	9.97	(1.53)	9.98	(1.54)
Parental education missing	0.04		0.04		0.04	
Migrant parent(s)	0.11		0.10		0.11	
Individuals	11,247		5,580		5,667	

Note: Samples restricted to individuals enrolled in school in West German states (excl. Berlin).
Source: NEPS-SC6 (doi:10.5157/NEPS:SC6:12.0.1).

Table A.3: Balancing tests for background characteristics

Estimation sample	All				Men				Women			
Data	NEPS		SOEP		NEPS		SOEP		NEPS		SOEP	
	Coeff./	p-value	Coeff./	p-value	Coeff./	p-value	Coeff./	p-value	Coeff./	p-value	Coeff./	p-value
Month of Birth	0.011	0.901	-0.011	0.847	0.237	0.056	0.098	0.220	-0.209	0.099	-0.109	0.143
Age at first measurement	0.101	0.000	-0.083	0.000	0.064	0.000	-0.092	0.000	0.137	0.000	-0.073	0.000
State: Schleswig-Holstein	-0.001	0.923	-0.006	0.291	0.007	0.464	-0.007	0.375	-0.008	0.432	-0.006	0.488
State: Hamburg	-0.009	0.119	0.001	0.877	-0.010	0.157	-0.003	0.680	-0.006	0.469	0.004	0.498
State: Lower Saxony	0.012	0.352	0.000	1.000	0.019	0.300	-0.011	0.413	0.004	0.824	0.011	0.402
State: Bremen	0.005	0.251	0.004	0.178	0.006	0.371	0.005	0.254	0.004	0.396	0.003	0.455
State: North Rhine-Westphalia	0.012	0.475	-0.019	0.133	0.032	0.193	-0.040	0.031	-0.004	0.868	-0.000	0.992
State: Hesse	0.020	0.061	0.014	0.095	0.012	0.418	0.027	0.028	0.026	0.078	0.001	0.893
State: Rhineland-Palatinate	-0.036	0.000	-0.006	0.351	-0.039	0.004	-0.003	0.738	-0.033	0.008	-0.009	0.328
State: Baden-Wuerttemberg	0.006	0.672	-0.007	0.505	0.010	0.602	-0.001	0.951	0.000	0.993	-0.013	0.353
State: Bavaria	-0.007	0.616	0.018	0.110	-0.027	0.181	0.035	0.034	0.012	0.553	0.004	0.820
State: Saarland	-0.003	0.617	0.002	0.545	-0.010	0.180	-0.001	0.770	0.004	0.612	0.005	0.288
Mothers age at birth: missing	0.001	0.905	0.003	0.546	0.013	0.209	0.011	0.210	-0.011	0.188	-0.003	0.714
Mothers age at birth (yrs)	0.035	0.879	0.179	0.275	-0.107	0.746	0.150	0.538	0.157	0.626	0.226	0.308
Fathers age at birth: missing	-0.009	0.261	0.002	0.794	0.007	0.553	0.007	0.494	-0.025	0.002	-0.002	0.824
Fathers age at birth (yrs)	-0.052	0.846	0.270	0.159	-0.146	0.704	0.265	0.348	0.100	0.789	0.282	0.281
Parental educ.: missing	-0.002	0.810	0.005	0.458	-0.001	0.910	0.011	0.253	-0.002	0.811	0.001	0.894
Parental educ. (yrs)	-0.017	0.764	-0.011	0.802	-0.119	0.134	0.004	0.948	0.075	0.373	-0.028	0.649
Migrant parent(s)	0.020	0.080	-0.006	0.436	0.025	0.125	-0.006	0.610	0.016	0.336	-0.006	0.584
Kindergarten attendance	0.012	0.457	-	-	0.008	0.717	-	-	0.016	0.336	-	-
F-test	6.904		5.173		3.188		2.973		5.310		2.803	
p-val.	0.000		0.000		0.000		0.000		0.000		0.000	
Obs.	11,247		42,052		5,580		19,561		5,667		22,491	

Note: Each point estimate comes from a separate linear regression of a given background characteristic on the *After* dummy. All regressions include linear trends in the running variable (month of birth) fitted separately on either side of the cutoff, cohort fixed effects, survey year fixed effects, and a gender dummy. Robust standard errors in parentheses. The F-test statistic and the corresponding p-value refer to joint F-tests from a regression of the *After* dummy on the full set of background characteristics in each estimation sample.
Source: NEPS-SC6 (doi:10.5157/NEPS:SC6:12.0.1) and SOEP (doi:10.5684/soep.core.v38.1).

Table A.4: Effect of being born after the cutoff on the timing of school entry (first stage)

	(1) Baseline	(2) Incl. family controls	(3) Incl. state FE	(4) Incl. other policies	(5) No controls	(6) Incl. quadr. trends
	Panel A: School starting age (in yrs)					
All: *After*	0.392***	0.391***	0.391***	0.390***	0.392***	0.309***
	(0.030)	(0.030)	(0.030)	(0.030)	(0.029)	(0.049)
Y-Mean	6.488					
Obs.	11,247					
Men: *After*	0.406***	0.399***	0.403***	0.403***	0.407***	0.299***
	(0.052)	(0.049)	(0.051)	(0.051)	(0.050)	(0.079)
Y-Mean	6.506					
Obs.	5,580					
Women: *After*	0.379***	0.380***	0.378***	0.378***	0.380***	0.317***
	(0.032)	(0.032)	(0.031)	(0.032)	(0.031)	(0.053)
Y-Mean	6.470					
Obs.	5,667					
	Panel B: Old for grade (0/1)					
All: *After*	0.363***	0.363***	0.361***	0.360***	0.359***	0.280***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.030)
Y-Mean	0.408					
Obs.	11,247					
Men: *After*	0.361***	0.363***	0.356***	0.357***	0.361***	0.306***
	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.043)
Y-Mean	0.415					
Obs.	5,580					
Women: *After*	0.364***	0.364***	0.366***	0.361***	0.359***	0.250***
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.043)
Y-Mean	0.401					
Obs.	5,667					

Note: Each point estimate on the *After* dummy comes from a separate linear regression of equation (1). All regressions in columns 1 through 6 include linear trends in the running variable (month of birth) fitted separately on either side of the cutoff. The baseline specification additionally includes cohort fixed effects, survey year fixed effects, and a gender dummy. Family controls include an indicator for at least one foreign-born parent, maternal and paternal age at birth, the highest parental educational attainment (in years), and indicators for missing information on the parental variables. Other policies include indicators for exposure to extended compulsory schooling and shortened school years. Robust standard errors in parentheses. Stars indicate statistical significance at the 1% (***), 5% (**), and 10% (*) level. FE = fixed effects.
Source: NEPS-SC6 (doi:10.5157/NEPS:SC6:12.0.1).

Table A.5: Average characteristics of the compliers

	Compliers Mean	Non-Compliers Mean	Mean difference
Female	0.51	0.48	0.03
Year of birth	1964.34	1962.74	1.60
Age at first interview	44,46	45,98	-1.52
State: Schleswig-Holstein	0.04	0.04	0.00
State: Hamburg	0.02	0.03	-0.01
State: Lower Saxony	0.13	0.14	-0.01
State: Bremen	0.01	0.01	0.00
State: North Rhine-Westphalia	0.29	0.29	0.00
State: Hesse	0.08	0.09	-0.01
State: Rhineland-Palatinate	0.07	0.07	0.00
State: Baden-Wuerttemberg	0.15	0.15	0.00
State: Bavaria	0.19	0.15	-0.04
State: Saarland	0.02	0.02	0.00
Mother's age at birth (yrs)	28.12	27.78	0.34
Mother's age at birth: missing	0.03	0.04	-0.01
Father's age at birth (yrs)	31.35	31.14	0.21
Father's age at birth: missing	0.04	0.05	-0.01
Parental education (yrs)	9.98	9.97	0.01
Parental education: missing	0.04	0.05	-0.01
Migrant parent(s)	0.10	0.11	-0.01
Kindergarden attendance	0.82	0.81	0.01
Kindergarden attendance: missing	0.02	0.02	0.00
Individuals	8,229	3,018	

Note: All individuals who actually enter school in the year they are supposed to according to the school enrolment law are defined as compliers.
Source: NEPS-SC6 (doi:10.5157/NEPS:SC6:12.0.1).

Table A.6: Effect of being born after the cutoff on the timing of school entry (first stage using a weekly running variable)

	(1) Baseline (month)	(2) Weekly run.var. trends lin.	(3) Weekly run.var. quadr.	(4) $-/+1$ week donut trends lin.	(5) $-/+1$ week donut quadr.	(6) $-/+2$ weeks donut trends lin.	(7) $-/+2$ weeks donut quadr.
Panel A: School starting age (in yrs)							
All: *After*	0.392***	0.371***	0.292***	0.407***	0.354***	0.439***	0.425***
	(0.030)	(0.028)	(0.041)	(0.031)	(0.052)	(0.034)	(0.064)
Y-Mean	6.488	6.488	6.488	6.488	6.488	6.486	6.486
Obs.	11,247	11,247	11,247	10,799	10,799	10,347	10,347
Men: *After*	0.406***	0.365***	0.277***	0.418***	0.377***	0.457***	0.477***
	(0.052)	(0.047)	(0.068)	(0.053)	(0.087)	(0.058)	(0.107)
Y-Mean	6.506	6.506	6.506	6.506	6.506	6.504	6.504
Obs.	5,580	5,580	5,580	5,349	5,349	5,126	5,126
Women: *After*	0.379***	0.377***	0.306***	0.396***	0.333***	0.416***	0.364***
	(0.032)	(0.030)	(0.046)	(0.033)	(0.055)	(0.036)	(0.067)
Y-Mean	6.470	6.470	6.470	6.471	6.471	6.469	6.469
Obs.	5,667	5,667	5,667	5,450	5,450	5,221	5,221
Panel B: Old for grade (0/1)							
All: *After*	0.363***	0.366***	0.273***	0.394***	0.311***	0.411***	0.326***
	(0.017)	(0.017)	(0.026)	(0.018)	(0.031)	(0.019)	(0.036)
Y-Mean	0.408	0.408	0.408	0.409	0.409	0.408	0.408
Obs.	11,247	11,247	11,247	10,799	10,799	10,347	10,347
Men: *After*	0.361***	0.353***	0.269***	0.392***	0.336***	0.408***	0.357***
	(0.025)	(0.024)	(0.037)	(0.026)	(0.043)	(0.028)	(0.051)
Y-Mean	0.415	0.415	0.415	0.415	0.415	0.413	0.413
Obs.	5,580	5,580	5,580	5,349	5,349	5,126	5,126
Women: *After*	0.364***	0.378***	0.277***	0.395***	0.284***	0.413***	0.288***
	(0.024)	(0.023)	(0.036)	(0.025)	(0.043)	(0.027)	(0.052)
Y-Mean	0.401	0.401	0.401	0.403	0.403	0.402	0.402
Obs.	5,667	5,667	5,667	5,450	5,450	5,221	5,221

Note: Each point estimate on the *After* dummy comes from a separate linear regression of equation (1). All regressions include linear trends in the running variable (month of birth in column 1 and week of birth elsewhere) fitted separately on either side of the cutoff, cohort fixed effects, survey year fixed effects, and a gender dummy. Regressions in columns 3, 5, and 7 additionally include quadratic trends in the running variable. Samples in columns 4 and 5 exclude $-/+1$ week of births around the cutoff. Samples in columns 6 and 7 exclude $-/+2$ weeks of births around the cutoff. Robust standard errors in parentheses. Stars indicate statistical significance at the 1% (***), 5% (**), and 10% (*) level.
Source: NEPS-SC6 (doi:10.5157/NEPS:SC6:12.0.1).

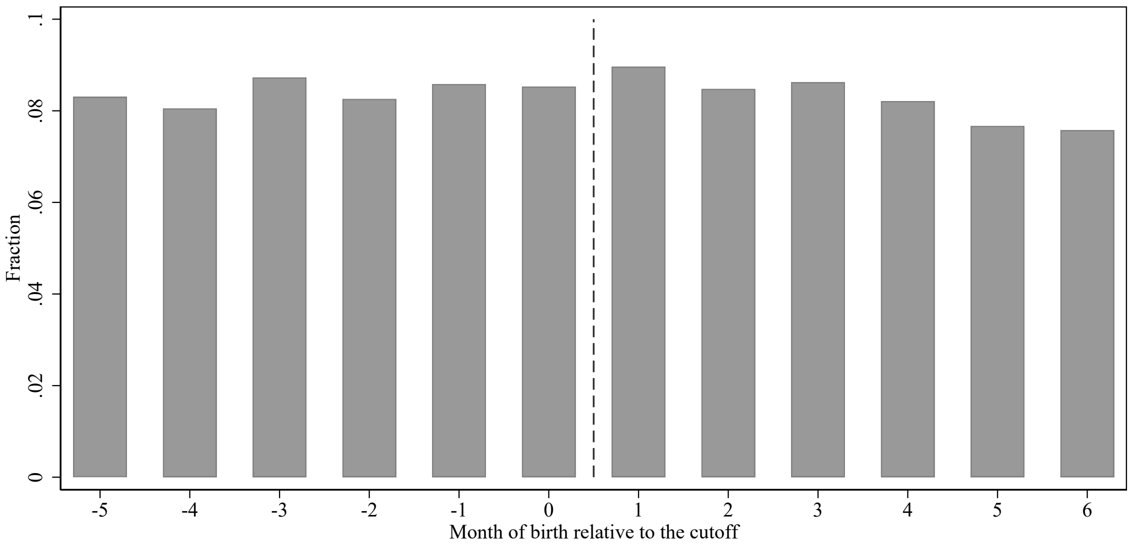
Table A.7: Effect of being born after the cutoff on educational attainment

	NEPS			SOEP	
	Academic Track	High School	College	High School	College
	(1)	(2)	(3)	(4)	(5)
Panel A: All					
After	0.035**	0.017	-0.005	0.026*	0.021*
	(0.018)	(0.018)	(0.017)	(0.014)	(0.012)
Y-Mean	0.327	0.417	0.280	0.403	0.244
Observations	11,247	11,247	11,247	20,491	20,491
Panel B: Men					
After	0.019	-0.016	-0.025	0.041**	0.008
	(0.025)	(0.026)	(0.025)	(0.020)	(0.018)
Y-Mean	0.324	0.441	0.324	0.424	0.272
Observations	5,580	5,580	5,580	9,740	9,740
Panel C: Women					
After	0.054**	0.050**	0.016	0.013	0.033**
	(0.025)	(0.025)	(0.023)	(0.019)	(0.016)
Y-Mean	0.330	0.394	0.238	0.383	0.218
Observations	5,667	5,667	5,667	10,751	10,751
Panel D: All					
After	0.018	-0.014	-0.022	0.041**	0.005
	(0.025)	(0.026)	(0.025)	(0.020)	(0.018)
After x Female	0.035	0.062*	0.034	-0.027	0.029
	(0.035)	(0.036)	(0.033)	(0.027)	(0.024)
Female	-0.012	-0.077***	-0.099***	-0.028**	-0.069***
	(0.018)	(0.018)	(0.017)	(0.014)	(0.012)
Y-Mean	0.327	0.417	0.280	0.403	0.244
Observations	11,247	11,247	11,247	20,491	20,491

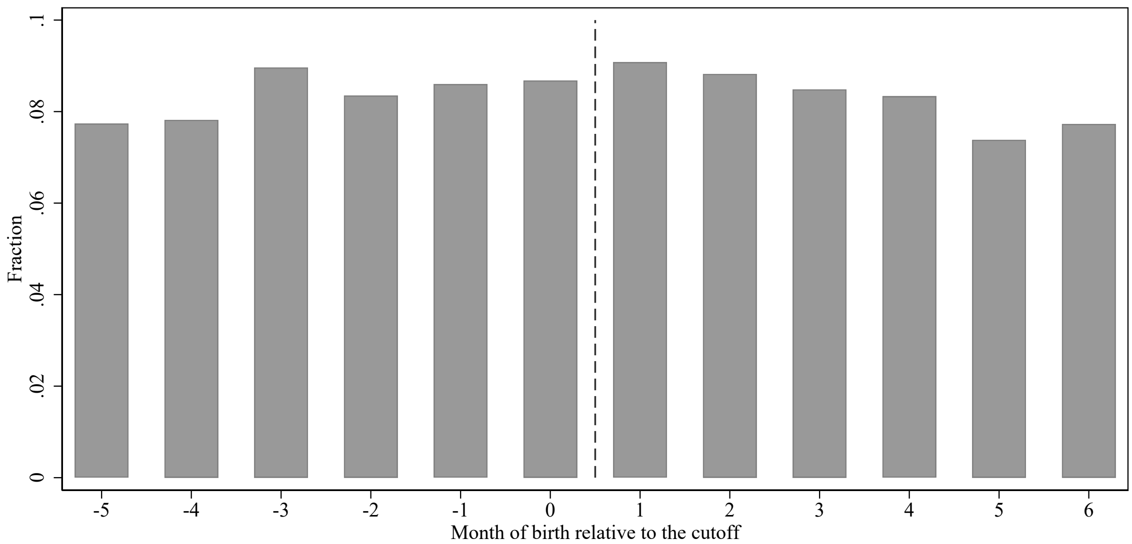
Notes: Each point estimate on the *After* dummy comes from a separate linear regression of equation (1), with different educational outcomes as dummy variable. *Academic Track* signals if a child attended the academic school track. *High School* (*College*) signals an upper secondary (tertiary) degree. All regressions include linear trends in the running variable (month of birth) fitted separately on either side of the cutoff, cohort fixed effects, survey year fixed effects, and a gender dummy. In Panel D, the linear trends in the running variable are interacted with the gender dummy. Robust standard errors in parentheses. Stars indicate statistical significance at the 1% (***), 5% (**), and 10% (*) level.
Source: NEPS-SC6 (doi:10.5157/NEPS:SC6:12.0.1) and SOEP (doi:10.5684/soep.core.v38.1).

Figure A.1: Distribution of births by month of birth

(a) SOEP sample



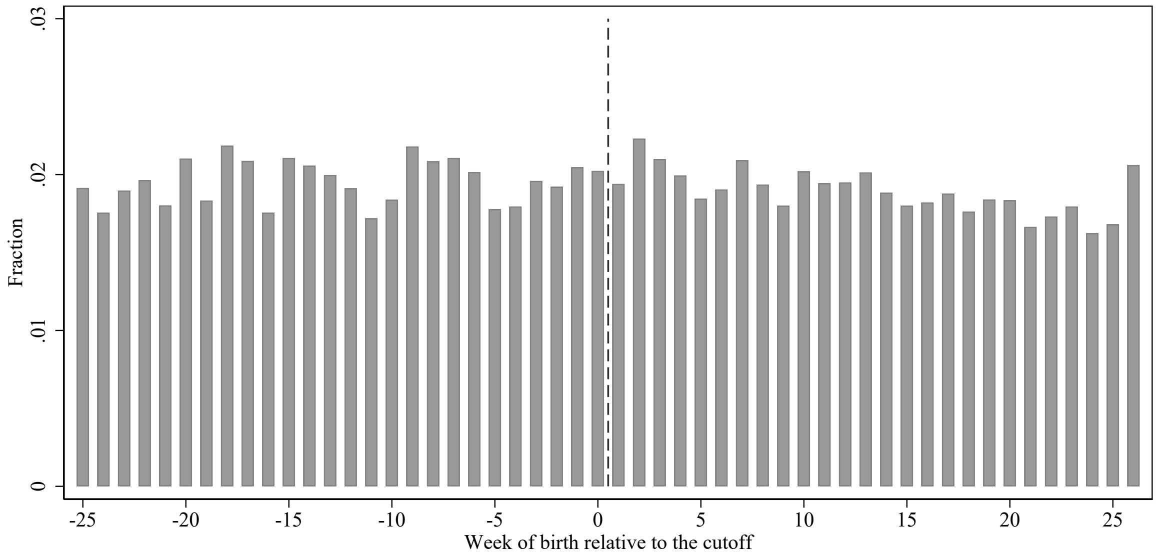
(b) NEPS sample



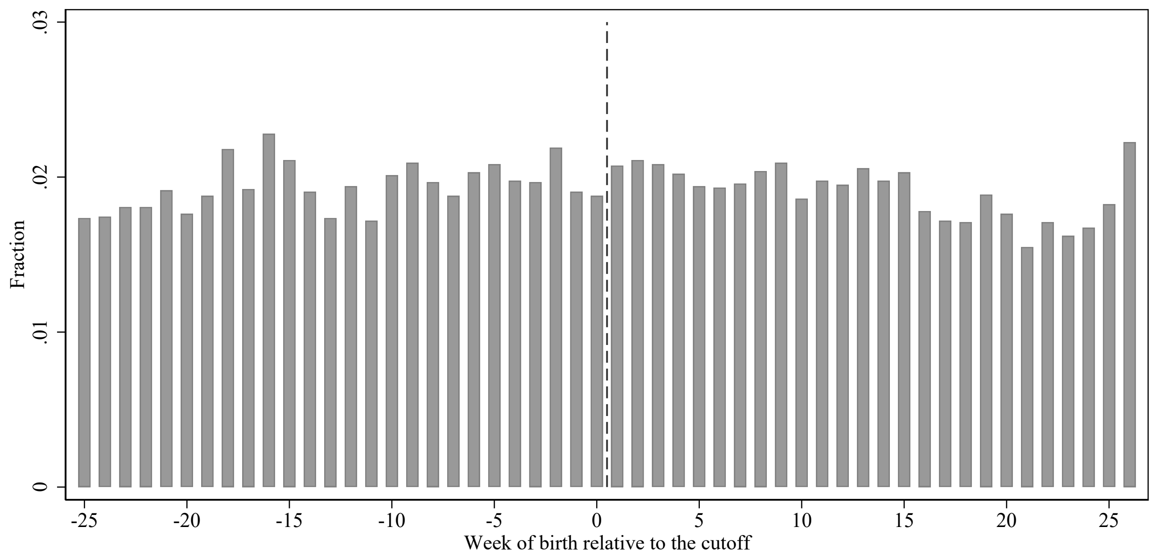
Note: The dashed vertical line shows the cutoff date. The Frandsen (2017) test for discrete running variables testing for a potential discontinuity at the cutoff yields p-values between 0.222 and 0.577 in the SOEP data and 0.684 and 0.817 in the NEPS data for the bounding constant (k) between 0 and 0.04. The smaller k, the more conservative the test.
Source: NEPS-SC6 (doi:10.5157/NEPS:SC6:12.0.1), SOEP (doi:10.5684/soep.core.v38.1).

Figure A.2: Distribution of births by week of birth

(a) SOEP sample



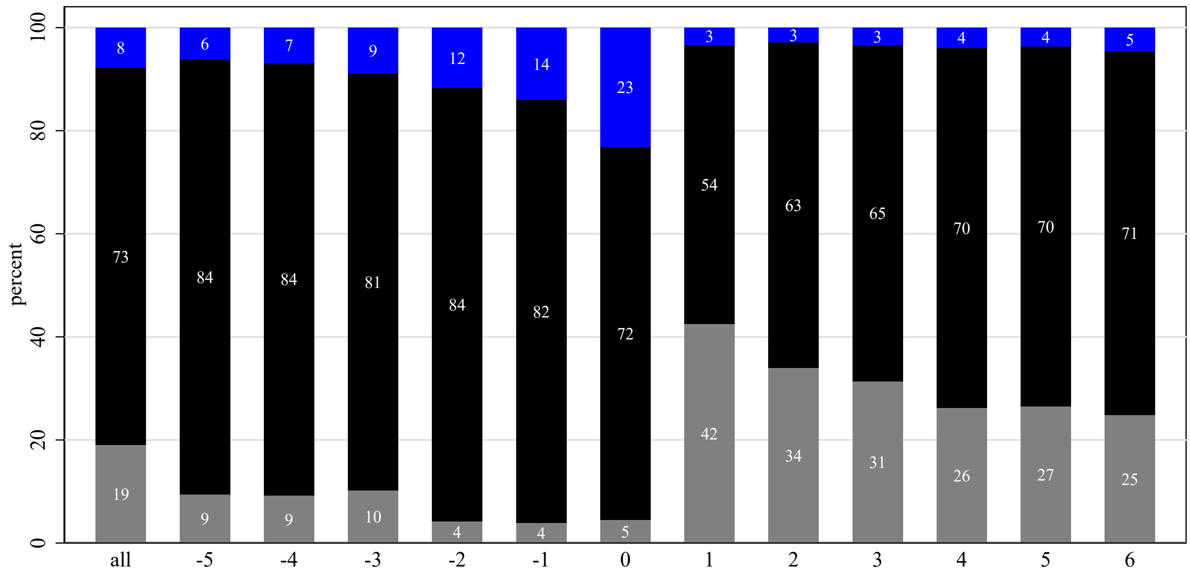
(b) NEPS sample



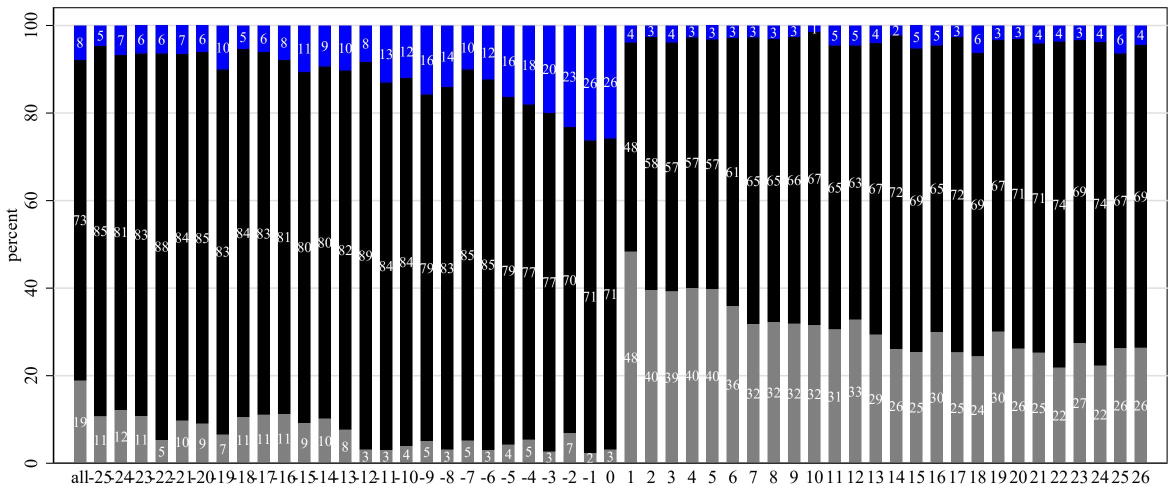
Note: The dashed vertical line shows the cutoff date. The Frandsen (2017) test for discrete running variables testing for a potential discontinuity at the cutoff yields p-values between 0.772 and 0.827 in the SOEP data and 0.451 and 0.510 in the NEPS data for the bounding constant (k) between 0 and 0.04. The smaller k, the more conservative the test.
Source: NEPS-SC6 (doi:10.5157/NEPS:SC6:12.0.1), SOEP (doi:10.5684/soep.core.v38o).

Figure A.3: Compliance with the cutoff by the running variable

(a) Month of birth

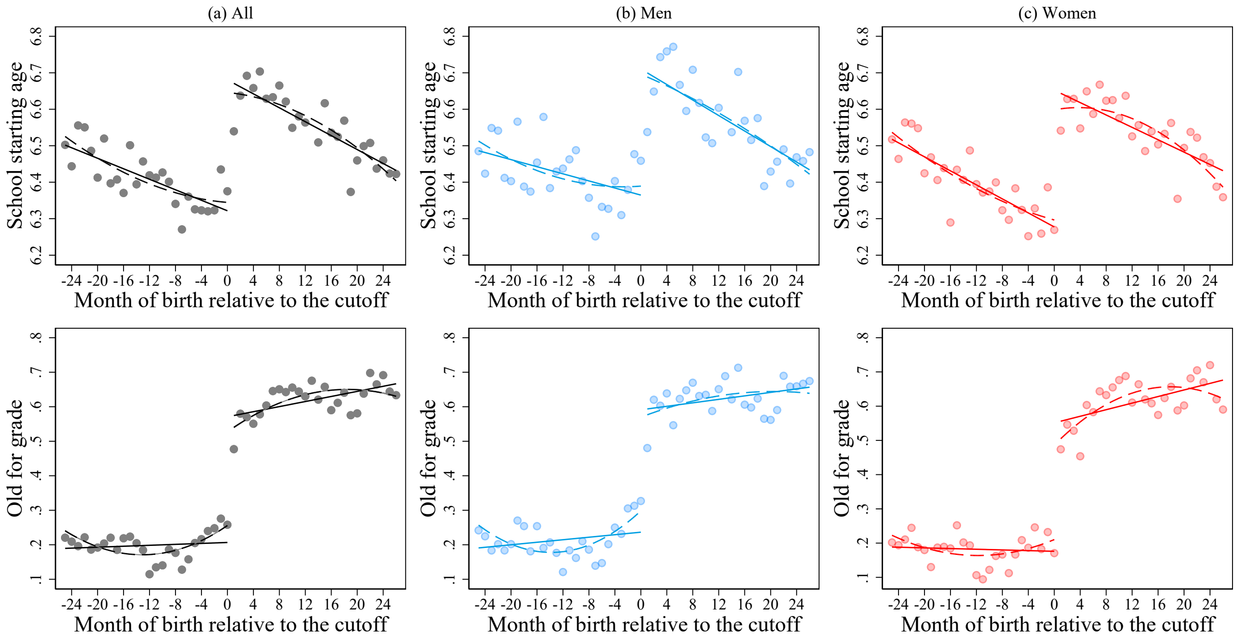


(b) Week of birth



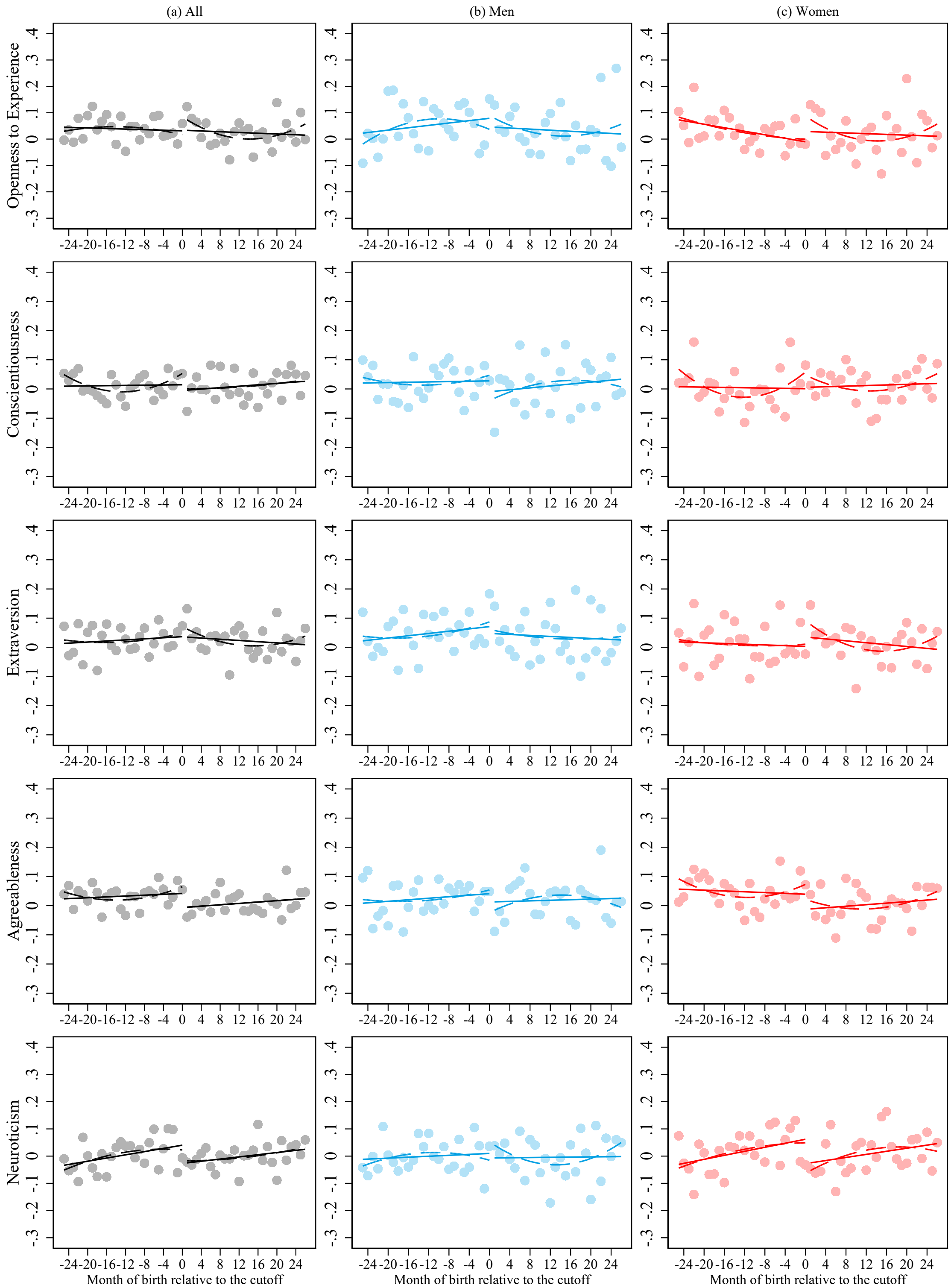
Note: The figures show the relative numbers of school entrants depending on compliance with the sharp cutoff date. The x-axis shows the running variable relative to the cutoff (i.e., month of birth in Panel (a) and week of birth in Panel (b)).
Source: NEPS-SC6 (doi:10.5157/NEPS:SC6:12.0.1).

Figure A.4: Being born after the cutoff and the timing of school entry (weekly running variable)



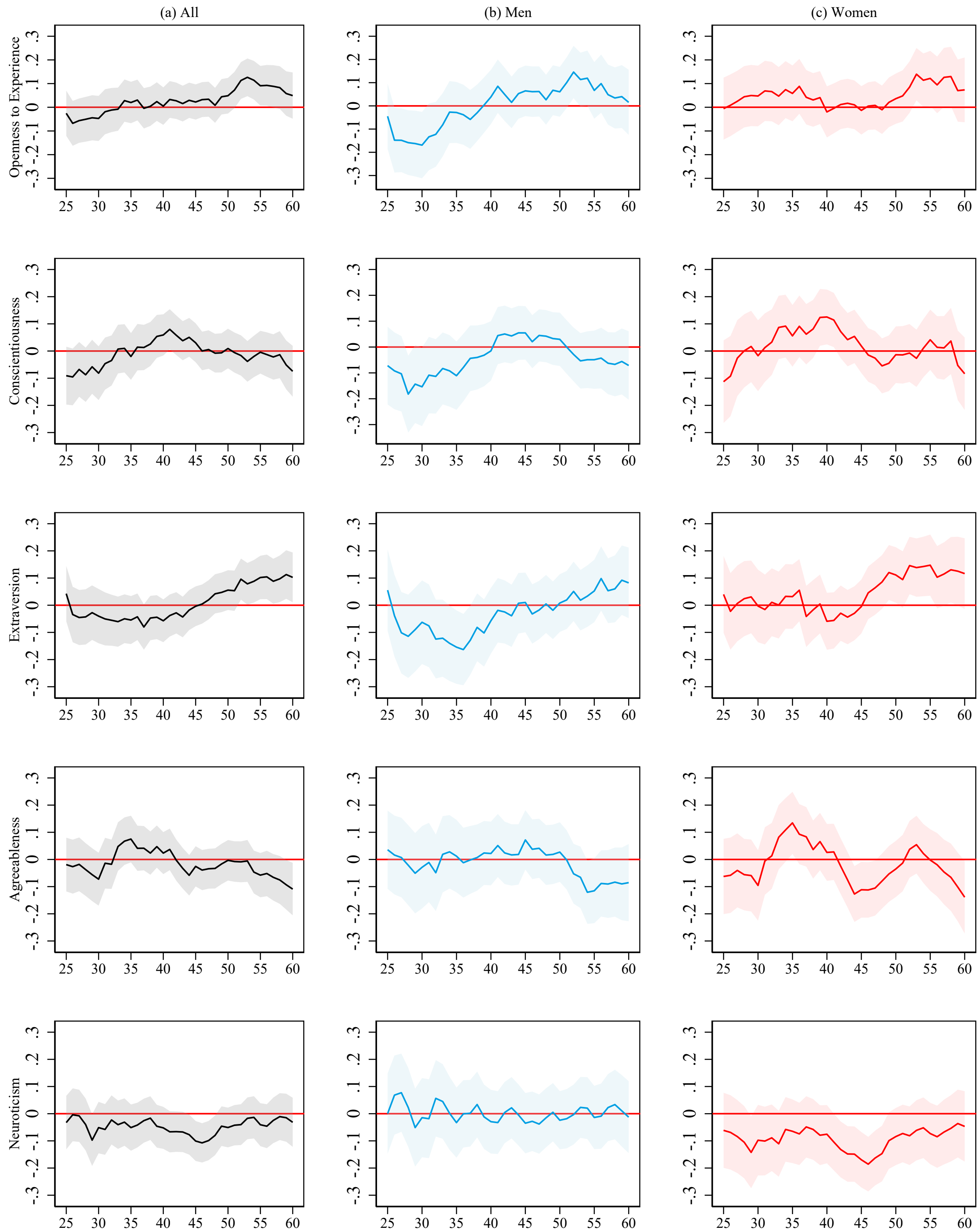
Note: School starting age (in years) is calculated as a difference between the date of a child's school entry and its date of birth. Old for grade indicates school entry in the year of a child's seventh birthday (as opposed to sixth). The solid (dashed) lines represent first (second) order polynomials fitted separately to the data on each side of the cutoff.
Source: NEPS-SC6 (doi:10.5157/NEPS:SC6:12.0.1).

Figure A.5: Personality traits by distance to the cutoff (weekly running variable)



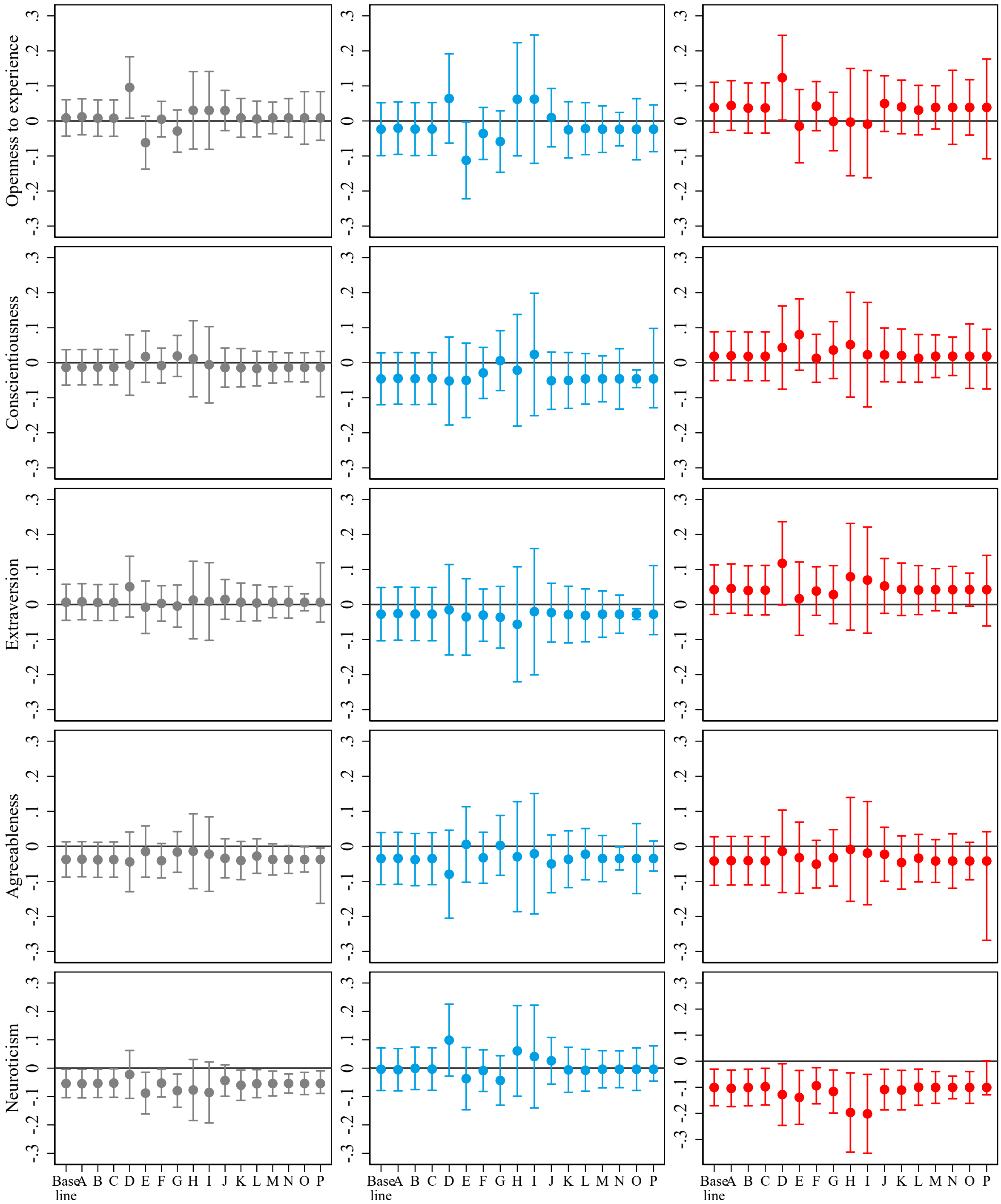
Note: Sample restricted to individuals who were born and enrolled in school in West German states (excl. Berlin). Personality traits are standardized using the mean and standard deviation within each sample.
Source: SOEP (doi:10.5684/soep.core.v38o).

Figure A.6: Age-specific effects on all personality traits



Note: Each subfigure plots 36 point estimates on the *After* dummy. Each estimate comes from a separate linear regression of equation (1). All regressions include linear trends in the running variable fitted separately on either side of the cutoff, cohort fixed effects, survey year fixed effects, and a gender dummy. The shaded areas represent 95% confidence intervals around the respective point estimate obtained from standard errors clustered at the individual level.
Source: SOEP (doi:10.5684/soep.core.v38.1).

Figure A.7: Robustness analysis: average effects on all personality traits



Note: The outcomes are standardized within each estimation sample. Each point estimate on the *After* dummy comes from a separate linear regression of equation (1). All regressions include linear trends in the running variable (month of birth) fitted separately on either side of the cutoff, cohort fixed effects, and survey year fixed effects. If not stated differently, the 95% confidence intervals around the respective point estimate are obtained from standard errors clustered at the individual level.
Source: SOEP (doi:10.5684/soep.core.v38o).