The Untold Story of Internal Migration in Germany:

Life-cycle Patterns, Developments, and the Role of Education

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Abstract

This paper examines internal migration from a lifetime perspective using unique data on detailed residential biographies of individuals born in Germany between 1944 and 1986. We first describe life-cycle patterns of internal mobility and potential differences across space, time, and socio-demographic groups. We find substantial differences across the life course, with major location changes around important educational decisions and striking differences across groups, especially by educational attainment. We then investigate causality in the substantial education-mobility gradient. For identification, we exploit two policy-induced sources of variation, each shifting towards better education at a different margin of the ability distribution. Using a difference-in-differences and regression discontinuity design, we find no effect of these policies on internal mobility.

Keywords: regional mobility, internal migration, Germany, education, compulsory schooling, enrollment cutoffs
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1 Introduction

Regional mobility is an essential driver of economic growth and technological development, as both depend on the ability and willingness of workers to relocate to more innovative and productive sectors and labor markets (e.g., Blanchard and Katz, 1992, Caselli and Coleman, 2001, Amior and Manning, 2018). Thus, an important consequence of geographic mobility is a more efficient match between workers and firms (Dauth et al., 2022), but it can also drive spatial income inequalities (e.g., Gaubert et al., 2021). At the individual level, regional mobility is often seen as a means to improve the economic situation and well-being of households and individuals(e.g., Deryugina et al., 2018, Groen et al., 2020)[1]. On the other hand, it can also lead to unintended consequences arising, for example, from the disruptive nature of relocation (Nassal and Paul, 2022) or residential segregation (Derenoncourt, 2022).

Due to its importance on both micro and macro levels, issues related to internal migration have drawn ongoing attention among researchers and policymakers in many countries. The United States is arguably the most prominent example,[2] presumably also because internal migration is fundamental to the American narrative of "moving to opportunity". Over the last decades, the discussion in the US has concentrated on the declining trends in migration rates over time and the substantial changes in the types of destinations by different socio-economic groups (Jia et al., 2023). On the other extreme, surprisingly little is known about internal migration in Europe's largest economy – Germany. This lack of evidence is likely due to severe limitations in the data available at the national level that would allow tracking individuals' location over time. For example, while none of the modern German censuses included a question on an individual's place of birth [3], the decennial censuses in the U.S. collected this information from 1935 onwards, which allows for the study of internal migration patterns with relative ease (Zimran, 2022).

Data limitations have made it challenging to establish even fundamental facts about the extent, patterns, and determinants of internal migration in Germany, with few exceptions. First, based on aggregate administrative data[4], we know that approximately

[1] These studies document positive long-run effects of regional mobility on earnings and employment by using arguably exogenous variation in reallocation. Regarding other outcomes, for example, Kling et al. (2005) show that migration affects crime behavior, and Finkelstein et al. (2021) document positive effects on life expectancy. There is also evidence showing that the effects of migration carry over to the next generation (Chetty and Hendren, 2018a,b, Nakamura et al., 2022, Baran et al., 2023).

[2] See, e.g., Borjas (2006), Saks and Wozniak (2011), Molloy et al. (2011), Bayer and Juessen (2012), Jia et al. (2023), Peri and Zaiour (2023).

[3] After World War II, Germany conducted censuses in 1950, 1961, 1970, 1987, 2011, and 2022. Place of birth is recorded only as an indicator of being born abroad.

[4] The records come from local Residents' Registration Offices and are centrally collected and annually

1-2 percent (3-4 percent) of the German population officially changes their residential address by moving across state (county) borders each year. These numbers are slightly lower for women than for men but have remained relatively constant over time since 1991 (e.g., Sander, 2017, Stawarz and Rosenbaum-Feldbrügge, 2020, BiB, 2020). However, the administrative data do not allow for following individuals over time or linking with other national data sources, thereby making it impossible to capture any long-term movements or life-cycle patterns[5].

Second, the large migration flows from the former socialist German Democratic Republic (GDR) to the Federal Republic of Germany (FRG) after the Fall of the Berlin Wall in 1989 have spawned extensive research on the extent, specifics, determinants, and consequences of this particular phenomenon.[6] Many of these studies draw on individual data from the German Socio-Economic Panel (SOEP), which asks the respondents whether they resided in East or West Germany in 1989. However, beyond moves across the former East-West border, regional mobility in Germany has been considered negligible and gained little attention in research.[7]

This paper fills the gap in the literature by presenting a comprehensive and detailed analysis of regional mobility patterns in Germany from a lifetime perspective. Apart from the well-documented East-West and gender gaps, our focus is on the role of education, as it has been long recognized as the key factor for understanding why some individuals move across regions and others do not (known as the "positive skill selection"). For this purpose, we use data from the National Education Panel Study: Starting Cohort Adults (NEPS-SC6). The unique feature of these data is the availability of both detailed biographical information on residential moves and educational paths over the life cycle. Specifically, for a representative sample of nearly 13,000 individuals born in Germany between 1944 and 1986, we construct a balanced panel that tracks their geographic mobility at monthly intervals starting from birth until 2020 (i.e., until the age of 34 to 76, depending on the birth cohort). Given the longitudinal nature of the NEPS, we can construct different mobility measures in terms of the time horizon (i.e., 1-year, 5-year, and lifetime migration) and the geographic units (state and

published by the Federal Statistical Office (Destatis). The data include information on the absolute number of population inflows and outflows from a given region within a given calendar year. Relating the number of movers to the respective population size in a given region and calendar year yields an aggregate 1-year migration rate

[5] Additionally, the included characteristics of the movers are limited to gender, age, and citizenship, which hampers research on the patterns and determinants of internal migration in Germany.

[6] See, e.g., Burda (1993), Werding (2002), Hunt (2006), Uhlig (2006), Fuchs-Schündeln and Schündeln (2009), Rainer and Siedler (2009), Sander (2014), Rosenbaum-Feldbrügge et al. (2022), Riphahn and Sauer (2024), Siedler (2010).

[7] Other notable exceptions include the few papers that study specific determinants of internal mobility, such as risk attitudes (Jaeger et al., 2010, Bauernschuster et al., 2014).

county) and study them from a life-cycle perspective. Importantly, the combination of retrospective regional details and fine-grained information on birth dates enables us to exploit institutional aspects of the German state-specific school system to investigate the causal impact of education.

We begin by presenting some basic facts on the extent of internal migration in Germany across the life cycle. Contrary to the common conjecture that regional mobility in Germany is generally low, we find substantial differences across the life course, space, time, and socio-demographic groups. Specifically, major location changes occur around school start and the transition to post-secondary education. The 1-year migration rate peaks at the age of 20, with 7 (15) percent of individuals moving across state (county) borders within a year. This declines to less than 1 (3) percent during prime ages. Despite the relatively low short-term propensities to move, migration is still quite common from a lifetime perspective; starting from the age of 35, more than a quarter (half) of individuals live in a state (county) other than their birthplace. The percentage of individuals residing outside their birth state during prime ages is indeed lower than nearly 40 percent in the US (e.g., Jia et al., 2023). However, in contrast to the declining trend in the US, there are no clear changes in lifetime migration in Germany over time despite slight increases in short-term migration rates. Beyond East-West and gender differences in age-mobility profiles, we observe striking disparities by educational attainment. Most of the differences persist when we condition on parental background.

We then turn to the question of whether there is a causal component in the significant education-mobility gradient. We do so by exploiting two arguably exogenous sources of variation, each inducing a shift at a different margin of the educational distribution. First, we exploit a post-World War II compulsory schooling reform that aimed to increase the duration of schooling for students at the bottom of the ability distribution (e.g.,). Second, for the same school cohorts, we study the mobility responses to statutory cutoff rules for school enrollment, which have been shown to increase the probability of attending the highest ability track in secondary school (e.g., Dustmann et al., 2017). The German setting is ideal for studying the effects of school entry and leaving laws because (unlike e.g., in the US or UK) there is no mechanic relationship between school starting age and compulsory schooling requirements. Considerable research examined the effects of both policies on adult outcomes, but there is so far no empirical evidence on their potential effects on regional mobility.[8] Using a difference-

[8]The compulsory schooling reform has been used to estimate wage returns to schooling (e.g., Pischke and von Wachter, 2008, Kamhöfer and Schmitz, 2016, Cygan-Rehm, 2022), and its various nonmonetary effects including political participation (Siedler, 2010, Bömmel and Heineck, 2023), health (Kemptner et al., 2011, Begerow and Jürges, 2022), fertility (Cygan-Rehm and Maeder, 2013), and intergenerational

in-differences and regression discontinuity design, we find no effect of these policies on internal mobility.

This paper contributes to several literatures. First, it is related to the descriptive evidence on internal mobility patterns in the United States and other countries, including cross-country comparisons (e.g., Long, 1991, Molloy et al., 2011, Bernard et al., 2014, Champion et al., eds, 2017, Jia et al., 2023). Unlike prior research, which predominantly focuses on aggregate mobility trends or specific life stages, our paper provides a comprehensive analysis across the entire life cycle. Although age-mobility profiles in cross-sectional data are well documented, it is essential to follow the same individuals over time to distinguish them from overall time trends. However, this is usually limited by data availability. Using large-scale longitudinal data on detailed residential biographies, we demonstrate that despite relatively low aggregate propensities to move, internal migration in Germany is substantial at specific life stages. This highlights the importance of taking a lifetime perspective to gain a more nuanced understanding of mobility patterns and their broader implications.

Second, this paper is closely related to the literature on the individual-level determinants of regional mobility. Specifically, we build on earlier research using plausibly exogenous sources of variation in education to estimate its impact on internal migration. So far, the findings are inconclusive. For example, while Machin et al. (2012) and Weiss (2015) find a positive effect exploiting changes in compulsory schooling laws in Norway and eight other European countries, respectively, McHenry (2013) documents the opposite for the US[9]. Similarly, for the US, Malamud and Wozniak (2012) find a negative estimate, although insignificant, when they instrument years of schooling by quarter of birth. However, the effect becomes positive when they use the variation in college attendance resulting from draft-avoidance behavior during the Vietnam War (Malamud and Wozniak, 2012). These results suggest that both the country-specific context and the margin of educational distribution might be important. We extend this literature by providing evidence from a country that is relatively less mobile compared to the US and northern European countries (e.g., Bell et al., 2015). A unique feature of our work is that we use two distinct sources of variation that induce a shift in

effects (Piopiunik, 2014, Margaryan et al., 2021, Huebener, 2022). The cutoffs for school entry have been shown to affect the secondary school track placement (e.g., Puhani and Weber, 2008, Mühlenweg and Puhani, 2010). Dustmann et al. (2017) use the German cutoff rules to estimate the effects of tracking on wages. Görlitz et al. (2022) document a persistent impact on vocabulary skills measured when individuals are in their late 50s.

[9]Similarly, Aparicio Fenoll and Kuehn (2017) shows that extended compulsory schooling reduce regional mobility using cross-country data from Europe. However, their focus was on the effects of emigration to another country. Since the transferability of educational attainment may vary across different countries, the effects on internal and international migration may differ

education at different margins of the educational distribution for the same generation. This enables us to compare the effects at different margins within the same context.

Finally, this paper complements the extensive research on German data that faces the challenge of a measurement error in the absence of retrospective regional information in the data. Specifically, when evaluating the mid- and long-term effects of earlier treatments with geographic variation, researchers often lack information on individuals' location at the time of the treatment. This issue inherently leads to the assumption of negligible mobility between the treatment occurrence and the outcome measurement. This often applies, but is not limited, to studies examining how adult outcomes are affected by regional shocks during childhood such as war-time experiences, food shortages, or school-time interventions.[10] We contribute to the literature by providing first evidence on the magnitude of the measurement error from a life-cycle perspective. Our results indicate that the measurement error might be substantial, which then raises the question of whether it is random to particular treatments. For the compulsory schooling reform and school entry cutoffs, we do not find any significant effect on regional mobility, which confirms that existing results on other responses to these policies, if anything, suffer from an attenuation bias. While it is beyond our scope to reassess prior conclusions or examine endogenous mobility in relation to other treatments, we draw attention to a dataset that can facilitate addressing these methodological challenges in future research.

The paper is structured as follows: Section 2 describes the data and mobility measures. Section 3 presents a descriptive analysis of internal mobility patterns and developments from a life-cycle perspective. Section 4 discusses our empirical approach to identify the causal link between education and regional mobility. Section 5 reports the main results and discusses the potential mechanisms. Finally, Section 6 provides concluding remarks.

2 Data

2.1 The National Educational Panel Study - Starting Cohort Adults (NEPS – SC6)

We use individual-level data from the German National Educational Panel Study (NEPS) (see Blossfeld and Roßbach, 2019). Specifically, we focus on the Starting Cohort Adults

[10] See, e.g., Pischke and von Wachter (2008), Kemptner et al. (2011), Cygan-Rehm and Maeder (2013), Jürges (2013), Akbulut-Yuksel (2014), Kamhöfer and Schmitz (2016), Dustmann et al. (2017), Bach et al. (2019), Margaryan et al. (2021), Bömmel and Heineck (2023), Huebener (2022), Dehos and Paul (2023), Cygan-Rehm (2025), Görlitz et al. (2025).

(SC6), which provides a representative sample of adults born between 1944 and 1986. The study initially began in 2007/2008 with a sample of individuals born between 1956 and 1986. In 2009/2010 (second wave), the sample was expanded to include the 1944-1955 birth cohorts, and the survey has been conducted annually since then. A sample refreshment followed in 2011. Since we are interested in regional mobility across the lifespan, we focus on individuals born in Germany, yielding 12,612 individuals. For each of them, we use information provided during all interviews conducted between 2007/8 and 2020.

The NEPS is a unique source of detailed regional information at different stages of the life cycle. Specifically, it provides information on place of birth, retrospective residential biographies, educational trajectories including the location of schools and post-secondary institutions attended, and labor market biographies including the location of employers. The biographical information is collected at the first interview of a given respondent. The biographical data are stored in episode-split monthly spells and are subject to rigorous plausibility checks (for details, see Rompczyk and Kleinert (2017)). After the first interview, we use the current place of residence provided at each subsequent interview, i.e., typically once a year between 2007 and 2020. Regional information is available at the state and county levels.

Using the different sources of regional information, we can follow a given individual across space in monthly intervals starting from birth until the most recent interview (in 2020 at the latest).[11] Nevertheless, retrospective biographical information on early childhood might suffer from a substantial measurement error due to limited recall. Thus, save for the place of birth, we do not use regional information before the age of 6 for the main analyses with the assumption that most respondents might not remember their residential histories before the school entry. For 11 percent of the monthly spells, the regional information is missing, mostly because the (non-retrospective) information on the place of residence collected during the consecutive interviews is only valid for the month of the interview. We fill the unobserved monthly spells vertically by carrying forward the location from the last observed spell.

The NEPS also collects comprehensive data on the educational paths of its respon-

[11]Retrospective residential biographies were not collected in the second wave of NEPS, i.e., in 2007/8 when birth cohorts 1944 and 1955 entered the sample. We impute a missing place of residence in a given calendar month using the available regional information from the remaining biographical sources such as educational spells, training spells, employment spells, and interview history. The measurement error should be negligible, as children in Germany are typically assigned to schools in their district. Regarding the match between the place of residence and place of work, we validated using social security data (Antoni et al., 2019) that more than 95 percent of workers from these cohorts did not commute across state borders in the early 2000s, i.e., before their first NEPS interview. Unfortunately, the place of residence is not available in the administrative records for earlier calendar years.

dents throughout their lives, making it an ideal dataset for studying the relationship between education and regional mobility. The availability of fine-grained information on birth dates, measured in calendar weeks, is also advantageous for our purposes. This enables us to precisely assign the treatment while utilizing institutional aspects of the German educational system to investigate causality. Finally, the dataset also contains a rich set of family background characteristics such as parental education, migration history, maternal age at childbirth, and the number of siblings.

A brief comparison of the NEPS with 2008 and 2011 cross-sections from the German Micro Census (see Appendix Table A1) using similar sample restrictions reveals that the sociodemographic composition of the two datasets is comparable with one exception: better-educated individuals are slightly overrepresented in the NEPS. We address this issue by applying cross-sectional weights calibrated to the 2011 Micro Census throughout. We use the weights for this calendar year because the NEPS provides the largest number of individuals after the sample update in 2011. However, our results do not change substantially if we alternatively use unweighted data.

Our main sample consists of 12,612 individuals, whom we follow over the life cycle starting from birth. Because we observe mobility outcomes beyond age 70 only for a few birth cohorts, we restrict the main sample to ages between 0 and 69. To facilitate computation, we aggregate the panel of approximately 9.5 million monthly spells into a person-age year panel of nearly 803,000 observations. For the descriptive analysis in Section 3, we use the entire sample. To identify causality in Section 4, we exploit institutional features of the West German school system after World War II, thereby restricting the estimation samples to individuals born between 1945 and 1964 in West Germany. Table 1 provides the summary statistics.

2.2 Mobility measures

To define specific measures for internal mobility, researchers typically decide on the geographic units of origin and destination and the time period in which individuals must move between the two (Molloy et al., 2011). These choices are often determined by data limitations, which is less of an issue in our data. We start with the state boundaries, which is the most common approach to define long-distance migration that leads to an appreciable change in the local economic environment (Jia et al., 2023). We then turn to the county level, which can be still considered as a sufficiently distant move to make a meaningful difference in local labor market environments and living conditions.

Regarding the time dimension, to measure the most recent moves, we compare an individual's geographic unit at a particular age to the corresponding unit twelve months or five years (i.e., exactly 60 months) ago. We also compare the current residential unit to an individual's place of birth, which is a common proxy for lifetime mobility. Note that following earlier literature, we determine all these measures solely by comparing the starting and ending months of the relevant time frame and, thereby, ignore the potential moves across geographic units over the intervening months. For example, an individual who lived in the same state at the age of 40 and exactly five years earlier will be classified as a nonmigrant even if this individual resided in a different state for a substantial time in between.

3 Descriptive analysis of internal mobility patterns over the life cycle

We begin with a plot of age-specific migration patterns across state and county borders in Figure 1. The top panel (a) shows that the annual migration rates are relatively high around the school start, typically at the age of 6 to 7, and fall immediately thereafter. Nearly 3.5 percent of 7-year-olds move to another state, and 8 percent to another county within a year. During compulsory schooling (i.e., approximately until the age of 15), the propensity to migrate across the state or county borders remains relatively low, at 1 or 2 percent, respectively). However, we do observe a slight increase at the age of 10, which typically coincides with the transition from primary to secondary school. A much larger increase is visible between the ages of 15 and 19, when adolescents typically decide on their post-secondary education. Both the cross-state and cross-county mobility rates peak at the age of 20, reaching 7 and 15 percent, respectively. Afterward, the annual mobility rates decline continuously with age until the early fifties. Less than 1.5 (3) percent of 45-55-year-olds move to a different state (county) annually. The slight increase thereafter may be a potential consequence of early retirement regulations (see e.g., Riphahn and Schrader, 2021).

Figure 1 (b) displays the percentage of individuals who have relocated across states or counties within the past five years. The life-cycle patterns closely resemble those for annual migration rates, but the 5-year rates are two to three times higher and slightly shifted to the right. As a result, the 5-year migration rates peak in the mid-twenties at nearly 18 and 40 percent for the cross-state and cross-county measures, respectively. Thereafter, both measures decrease substantially and level off at the age of 50-55, when only about 5 (11) percent of individuals relocate to a different state (county) within a 5-year period.

Figure 1 (c) shows the lifetime migration rate, which is the proportion of individuals living outside their birth state at a given age. Despite the relatively low short-term propensity to move, migration is still quite common from a long-term perspective. Specifically, almost 10 (25) percent of children born in Germany start school in a different state (county) than where they were born, and more than a quarter (half) of adults end up living in another state (county). As expected, there is a sharp increase in lifetime mobility between ages 15 and 20, followed by a plateau from age 25 onwards. Nevertheless, a comparison across the various migration measures in all three subfigures shows that lifetime migration rates may not reflect recent residential choices.

Appendix Figure A1 splits the cross-state mobility rates by gender. We do not observe any gender-specific differences during childhood and adolescence. However, starting from the age of 20, German men score somewhat higher on all considered mobility measures. During prime working ages, the mobility rates for men and women nearly converge, which might reflect the family formation and, consequently, joint mobility decisions.

For various reasons, life-cycle mobility may also vary across space and change over time. Appendix Figures A2-A4 illustrate some of the most striking differences. For example, Figure A2 confirms substantial variation across the former East-West German border: individuals born in former East Germany are more likely to have moved across states at any life stage, according to any migration measure considered. The disparities emerge towards the end of compulsory schooling and become most pronounced when individuals are in their early and mid-twenties. The corresponding gap in lifetime migration rates is large, with a difference of over 10 percentage points at age 20, and it widens further as individuals age. The East-West differences largely reflect the extensive migration flows from East to West German states after the Fall of the Berlin Wall (e.g., Hunt, 2006). However, the map in Appendix Figure A3 reveals that in addition to the East-West gaps, there are also substantial North-South disparities.

Figure A4 displays the trends in cross-state mobility for adults over time. We plot the average rates for ages 25-35, which we observe for all included birth cohorts, and for ages 25-55, which we can calculate only for individuals born until 1965 ("baby boomers"). Generally, we observe slightly increasing trends in 1-year and 5-year mobility rates over time, with acceleration for the most recent birth cohorts. This is mostly driven by the East Germans as the trends are less steep when we exclude them from the sample (dashed lines). The lifetime rates exhibit a U-shaped pattern over time. Again, the most recent increase can be attributed to East Germans, as the trend flattens when we omit them (dashed lines). The relatively high lifetime mobility of individuals born in the 1940s is entirely due to unusually high migration rates experienced in early childhood by the end of World War II and in its aftermath (not shown), which shifts their lifetime migration trajectory upward.

Generally, the life-cycle patterns (see, Figure 1, A1, and A2) suggest that much of the internal mobility in Germany coincides with periods of important educational decisions and tends to be low outside of these. Figure 2 provides more insights into the role of education in shaping the life-cycle profiles in cross-state mobility. It demonstrates that individuals with higher levels of education are more likely to move across states, regardless of the measure of migration used. The educational gradient solidifies in late adolescence, but some disparities are noticeable even before the age of 10, when ability tracking occurs. This suggests that some of the differences may also be due to selection on parental background. In Section 4, we test the extent to which the link between an individual's educational attainment and migration is causal.

Although many of these characteristics are correlated with one another, differences among groups are similar when estimated in a multiple OLS regression framework that includes age years fixed effects, year of birth fixed effects, and all of the considered socio-demographic characteristics. In Figure 3, we plot the estimates for cross-state mobility. The regressions are run on a sample restricted to ages between 25 and 55, but they remain very similar for alternative age restrictions. The results confirm significant gender gaps in short-term mobility, which dissipate in terms of lifetime mobility. Irrespective of the specific measure, East Germans exhibit a larger probability of moving. Interestingly, in Figure 4, we observe that the East-West gap reverses for cross-county mobility. In terms of magnitudes, the most striking differences in both figures are related to educational attainment. Some of the differences become slightly smaller when we condition on family background characteristics but do not disappear entirely.

4 Identifying the causal link between education and mobility

4.1 Institutional background

Education in Germany is generally free from primary school up to university level. Before school entry, children may attend a voluntary kindergarten. Formally, German kindergartens are not an integral part of the education system, but they rather serve as formal childcare facilities from the age of three until a child's school start (Bauernschuster and Schlotter, 2015)[12]. This differs from the situation, e.g., in the United States, where kindergarten entry marks the beginning of formal education. As for compulsory schooling in Germany, it typically starts at the age of six or seven. Specifically, children who turn six before a certain cutoff date are scheduled for school enrolment at the beginning of the next school year; children who turn six years of age after the cutoff are

[12] Kindergarten is typically not free of charge although publicly subsidized. For more information on the German childcare system, see, e.g., Spiess (2008), Wrohlich (2008), Bauernschuster and Schlotter (2015).

admitted to school one year later. The exact cutoff dates might vary across federal states because educational policies are under their responsibility (see,.g., Helbig and Nikolai, 2015). During the period under study, June 30th was the most prevalent cutoff.

Although the cutoff dates are not strictly binding[13], the majority of parents comply with the standard regulations. Official statistics indicate that around 90 percent of children start school on time, and this trend has remained fairly constant over time (see Appendix Figure A5). However, the actual compliance with the sharp cutoff dates is somewhat lower, as the official statistics include school starters under an early-exception rule in the regular enrolment figures. Nevertheless, the NEPS data suggest that, despite this exception, the average compliance is 75 percent (see Appendix Figure A5). The administrative data suggest that beyond the early-exception rule, early enrollment is rather rare, with only 2-5 percent of children starting school before they are of compulsory age. Comparing the shares of early enrollments across the two data sources implies that approximately 10-15 percent of parents utilized the statutory exceptions for early enrollment. The administrative data suggest that only 5-8 percent of children begin school with a delay. Although the shares are slightly higher in the NEPS data, redshirting is not a widespread practice in Germany.

Upon enrolment, children commonly undertake a four-year education in primary school.[14] Subsequently (i.e., around the age of 10), based on their academic record, students receive a referral to a particular type of secondary school[15]. Historically and still today, secondary education in Germany distinguishes between the basic track (Hauptschule), intermediate track (Realschule), and high schools (Gymnasium)[16]. These tracks substantially differ in duration and academic curricula, thereby preparing children for different professional careers. Specifically, the duration of the basic track is determined by the effective compulsory schooling law (i.e., it lasted until the eighth or ninth grade in the period under study). The basic track aims to prepare students for apprentice-

[13]Many states have explicitly defined exception rules for earlier enrolment. Their specifics differ across states and over time (Kamb and Tamm, 2023), but typically children born within three months after the cutoff date can apply for early enrollment. There is little room for additional exemptions. However, parents and authorities can retain some flexibility when the legal framework conflicts with child-specific factors, such as intellectual and emotional maturity. However, these cases are subject to complex administrative procedures and therefore, rare

[14]Save for the city-states of Hamburg, Bremen, and Berlin, where primary school comprises six grades.

[15]The exact tracking criteria differ by state. Usually, primary school teachers provide a recommendation that should exclusively reflect a student's cognitive abilities. In practice, this might involve some subjectiveness and considering a student's socioeconomic background. In several states, the recommendation is non-binding, yet in practice, the vast majority of parents comply. Details are provided, e.g., in L"udemann and Schwerdt (2013).

[16]There are alternative school types, including comprehensive schools without tracking (Gesamtschule) and schools for children with special needs (Sonderschule, Förderschule). However, the vast majority of cohorts considered in this study participated in the traditional tripartite system

ships in blue-collar occupations. The intermediate track continues until grade ten and qualifies students for apprenticeships or training in white-collar professions. A high school certificate after grade 12 or 13 entitles the student to pursue academic education in universities or colleges. Among individuals born in the 1940/50s, approximately 50 percent completed the basic track, 30 percent graduated from the middle track, and 20 percent obtained a high school diploma. Since then, the importance of the basic track has continuously declined and of high school increased.[17]

Regardless of the secondary school track attended, students are obligated to stay in school for a minimum number of years. Thus, unlike in the US or UK, the length of compulsory schooling in Germany is grade-based (and not age-based), i.e., it does not depend on when an individual started schooling or intends to drop out. While the centralized education system during the Nazi regime stipulated at least eight years of compulsory schooling, between 1946 and 1969, all states of the former Federal Republic of Germany (West Germany) extended its duration to nine years. Bavarian students born in September 1954 were the last birth cohort not affected by the extensions (see Appendix Figure A6)[18]. The primary rationale for these extensions was to enhance the physical and psychological readiness of students for mature vocational and labor market choices (LeSchinsky and Roeder, 1980). In several states, the extension of compulsory schooling was accompanied by a shift in the start of the school year from spring to autumn, which caused two shortened school years (Cygan-Rehm, 2025). On the other side of the Iron Curtain, the socialist German Democratic Republic (East Germany) centrally stipulated ten years of compulsory education since the 1950s (Helbig and Nikolai, 2015). Due to substantial differences between the former West and East Germany up until the Reunification in 1990 such as distinct educational systems and mobility patterns, we subsequently focus on West German states (excl. Berlin).

4.2 Empirical Approach

Our aim in this Section is to investigate the existence of a causal link between education and regional mobility. As outlined in Section 3, a major empirical challenge is that unobserved factors such as personality traits or parental background may simultane-

[17] For example, among the most recent birth cohorts in the NEPS-SC6 (i.e., born in the first half of the 1980s), we observe only 20 percent of individuals graduating from the basic track, and the share of high school graduates more than doubled to 45 percent.

[18] There are some inconsistencies in the literature regarding the exact timing of these extensions in some states (e.g., Pischke and von Wachter, 2008, Cygan-Rehm and Maeder, 2013, Piopiunik, 2014). The data behind Figure A6 largely follow LeSchinsky and Roeder (1980) and Cygan-Rehm (2025), who validated the reform's timing using the original state laws, official statistics on the actual ninth-grade attendance, and historical documents. All this leads us to believe that the information on the reform's timing is very accurate.

ously determine education and mobility. Consequently, it remains unclear whether the positive correlation between educational attainment and mobility is due to selection or a direct effect of education.

To address the endogeneity issue, we employ two distinct sources of plausibly exogenous variation that have been documented to steer individuals toward higher education at different levels of educational distribution. First, we exploit compulsory schooling reforms that intend to shift educational attainment at the lower end of the education distribution. Specifically, we use the staggered extensions of compulsory schooling from eight to nine years across the West German states in the 1950s and 1960s. Extensive research using this reform to identify the effects of education on other outcomes has consistently shown that this reform significantly increased the duration of education among affected individuals.[19]

Second, we build on established literature showing that the statutory cutoff rules for school enrolment have important consequences for secondary school track placement, which is particularly pronounced and persistent in selective systems featuring early ability tracking.[20] Specifically, being born after the cutoff increases the probability of attending high-ability tracks, which provide eligibility for college education. This implies a shift towards better education at relatively high levels of ability distribution.

Regarding the compulsory schooling extensions, our empirical approach exploits the variation in the exposure to the reform across states and birth cohorts. Specifically, we estimate reduced-form regressions of the following form

$$Y_{ist}^{a} = \alpha^{a}\text{Reform}_{st} + \pi_{s}^{a} + \pi_{t}^{a} + X_{ist}'\gamma^{a} + \epsilon_{ist}^{a}, \tag{1}$$

where Y_{ist}^{a} is a mobility outcome of individual i from state s and birth cohort t. We define birth cohorts at a monthly level by using information on an individual's year and month of birth. Our main outcomes comprise of 1-year, 5-year, and lifetime mobility indicators measured across both state and county borders. When assessing the reform's impact on regional mobility over the life cycle, the outcomes are measured at a particular age or age range a. The key explanatory variable of interest is the dummy variable $Reform$, which indicates the exposure to nine years of compulsory schooling instead of eight. All regressions include state π_s and birth cohort π_t fixed effects.

[19] See, e.g., Pischke and von Wachter (2008), Kamhöfer and Schmitz (2016), Cygan-Rehm (2022) for wage returns, Kemptner et al. (2011), Begerow and Jürges (2022) for health responses, Cygan-Rehm and Maeder (2013) for fertility effects, Siedler (2010), Bömmel and Heineck (2023) for political outcomes, and Piopiunik (2014), Margaryan et al. (2021), Huebener (2022) for intergenerational transmission.

[20] See, e.g., Bedard and Dhuey (2006) for the US, Puhani and Weber (2008), Mühlenweg and Puhani (2010), Dustmann et al. (2017) for Germany, Fredriksson and "Ockert (2014) for Sweden; and Oosterbeek et al. (2021) for the Netherlands

The cohort fixed effects correspond to a set of indicators for each unique combination of year and month of birth between February 1945 and December 1964 (with January 1945 being the omitted reference category). While in the main analysis, we do not include any further covariates, for sensitivity tests, we additionally control for individual characteristics such as gender and family background in the vector X. Finally, the unobserved heterogeneity is captured by the error term ϵ_i^a.

Given the reduced-form nature of equation 1, the estimate of α reflects an intention-to-treat (ITT) effect of the exposure to extended compulsory schooling. We do not employ an instrumental variable (IV) approach as prior research shows that the reform affected diverse adult outcomes beyond just the schooling duration such as health, fertility, social attitudes, and labor market outcomes (e.g., Kemptner et al., 2011, Cygan-Rehm and Maeder, 2013, Margaryan et al., 2021, Cygan-Rehm, 2022). This gives rise to econometric and interpretation challenges for an IV design. Thus, it is important to note that compulsory schooling extensions can potentially affect long-run mobility patterns through various channels.

The coefficient of interest α is identified within a staggered difference-in-differences (DD) framework using temporal variation across cohorts and spatial variation across states. Given that we include a full set of state and birth cohort fixed effects, our model specification represents a two-way fixed-effects (TWFE) design. The key assumption is that in the absence of the reform, all states would have followed similar trends in outcomes over time (the "parallel trends" assumption). Thus, the empirical strategy would fail if other state-specific differences could have been correlated with the reform and regional mobility patterns.

Although the parallel trends assumption is inherently untestable, we perform several empirical exercises to support its plausibility. First, we validate whether the reform status is not related to predetermined characteristics. These balancing tests (see, Appendix Table A2, columns 1 - 4) yield no systematic correlation patterns between the treatment variable and a wide range of observable characteristics, except for the exposure to short school years. This is not surprising since several states implemented compulsory schooling reform during the short school years. To address concerns that the parallel policy change may confound our results, in Section 5.4, we demonstrate that controlling for short school years does not change our main findings. To further strengthen the argument that there were no other unobserved factors disproportionately affecting states over time, we estimate extended model specifications that include aggregate proxies for state-specific schooling quality and state-specific trends in the relevant outcomes. Taken together, these validity checks strongly support the "as good as" random treatment assignment.

Nevertheless, recent research questions the validity of staggered DD designs even if the parallel trends assumption holds (e.g., De Chaisemartin and d'Haultfoeuille, 2020, Callaway and Sant'Anna, 2021, Goodman-Bacon, 2021, Sun and Abraham, 2021). The main argument is that using always-treated and/or earlier-treated groups as comparison groups for later-treated groups might lead to bias if the treatment effect varies across regions or over time. To ensure that treatment effect heterogeneity does not bias our main results from a conventional TWFE estimation, we demonstrate in Section 5.4 that our findings are robust to excluding always-treated states from the sample. Alternatively, we also use an extended TWFE estimator proposed by Wooldridge (2021), which flexibly allows for treatment effect heterogeneity.

Regarding the second source of plausibly exogenous variation in education, we adopt the approach by Dustmann et al. (2017), in which they leverage the quasi-random shift between secondary school tracks induced by the German cutoff rules for school entry to study the long-run effects of tracking on wages. Specifically, we apply a regression discontinuity design (RDD) by estimating the following reduced-form equation

$$Y_i^a = \beta^a \text{After}_i + f^a(w_i) + Z_i'\delta^a + \epsilon_i^a, \tag{2}$$

where is an outcome of individual at a specific age (range) a. The explanatory variable of interest is the indicator $After$, which equals one for individuals born up to six months after the cutoff date and zero for those born up to six months before the cutoff. The running variable corresponds to an individual's birth date measured in calendar weeks. We normalize to zero for the last week before the cutoff so that it measures the relative distance from an individual's birthdate to the relevant cutoff date for school entry. As a result, it ranges from -24 to 25. $f^a(w_i)$ denotes a control function in the running variable (week of birth), which is discrete. In our preferred specification, we define f as a linear function of the running variable with different slopes on either side of the cutoff. Nonetheless, in Section 5.4, we also report results from a quadratic specification and a non-parametric approach by using local linear regressions (Cattaneo et al., 2020). Again, for sensitivity checks, we extend the model specification by including the vector of individual characteristics Z_i, which might vary depending on the exact specification. ϵ_i^a is an error term.

The coefficient of interest β^a measures the ITT effect of being born after the cutoff on regional mobility at particular ages. Several studies for Germany indicate that students who were born after the cutoff date, and are thus relatively older upon school entry, have a significantly increased likelihood of attending Gymnasium, the highest secondary school track (e.g., Puhani and Weber, 2008, Mühlenweg and Puhani, 2010, Dustmann et al., 2017, Görlitz et al., 2022). Some of these studies also find a persistent effect on high school completion, but there seems to be no effect on university graduation. Nonetheless, we focus on reduced-form estimates as related literature has shown that the cutoffs have effects on various outcomes, not only academic achievement.[21] but parallel literature suggests that the relatively older school entrants are overrepresented in highly competitive professional environments (e.g., Tukiainen et al., 2019).

The main identification assumption is that $f^a(w_i)$ is a continuous and smooth function with no other discontinuity at the cutoff aside from a relatively later school entry. Before examining this assumption in detail, it is important to note that we do not observe the precise day of birth but rather the calendar week, which introduces some measurement error in the running variable and the dummy $After$ for individuals born exactly in the calendar week of the relevant cutoff (i.e., for weeks 0 and 1 relative to the cutoff)[22]. For this reason, but also to mitigate potential concerns that near the cutoff, the compliance could be potentially selective or that parents may have timed the exact birth date of their child, we exclude observations born up to two calendar weeks before and after the cutoff from the main analysis. This approach results in a "donut hole" RDD; a technique that has been widely used in the literature to make discontinuity analyses less sensitive to potential peculiarities in the immediate vicinity of the cutoff (e.g., Barreca et al., 2011).

In Appendix Figure A7, we show that the distribution of individuals in our sample is relatively smooth around the cutoff. Based on this graphical inspection and a density test based on the robust inference procedure recommended by Cattaneo et al. (2020)[23], we do not find any strong evidence of a non-random heaping around the cutoff. Reassuringly, the predetermined characteristics are also balanced around the cutoff (see Appendix Table A2, columns 5 - 8), which supports the argument of no endogenous selection into the treatment.

In both empirical strategies, the estimates of α and β measure the local effects of plausibly exogenous shifts in education on regional mobility for compliers, i.e., indi-

[21] Earlier research has documented significant impacts on the entire family (Landers"o and Heckman, 2017), special education service uptake (e.g., Dhuey and Lipscomb, 2010), high school leadership (Dhuey and Lipscomb, 2008), teenage fertility (e.g., Black et al., 2011), and crime commitment at young ages (e.g., Landers"o and Heckman, 2017). Regarding labor market performance, most studies (if anything) find negligible effects on earnings and employment (e.g., Fertig and Kluve, 2005, Black et al., 2011, Fredriksson and "Ockert, 2014, Larsen and Solli, 2017)

[22] Alternatively, we could manually assign individuals born exactly in the calendar week of the cutoff to one side or the other using the month of birth. In Section 5.4, we show that our results are robust when we do this.

[23] The density test yields a p-value of 0.5882. This result does not allow us to reject the hypothesis of a smooth distribution at the conventional significance levels.

viduals who comply with compulsory schooling laws or the administrative cutoffs for school entry, respectively. In Appendix Table A3, we compare the average characteristics of the compliers and non-compliers.

To ensure the availability of long-term mobility biographies in our data, both estimation samples are limited to individuals born in West Germany between 1945 and 1964. To assign the exposure to compulsory schooling extensions ($Reform$), we use an individual's date of (year and month) and the state of residence at the age of 14 (i.e., in the eighth grade). As the cutoff dates for school enrolment can also vary by state, the treatment variable $After$ is determined by the individual's date of birth and the state of residence at the age of 6 (i.e., at the time of school enrolment). Therefore, there is a slight difference in the size of the two estimation samples. Nonetheless, the sample means presented in columns 2 and 3 of Table 1 indicate that the sociodemographic composition of both samples is virtually identical.

5 Results

5.1 Compliance with the policies and immediate effects on educational outcomes

In this Section, we provide empirical evidence on the extent of compliance with compulsory schooling extensions and the statutory cutoffs for school enrollment among the relevant cohorts. We also study their immediate effects on educational outcomes. We begin by estimating the first-stage effect of the compulsory schooling reform. Table 2 shows the results from DD estimations of equation (1), where all regressions include state and birth date fixed effects. In Panel A, we use our main model specification without covariates. Column 1 implies that the reform increased the time spent in school by almost 0.6 years, on average. This is consistent graphical evidence in Appendix Figure A8 showing that the average duration of schooling increases discontinuously after the reform's implementation.

In Panel B, we include controls for family background characteristics and other policy changes, which leads to an even larger estimate. This is mainly due to controlling for the exposure to the parallel introduction of shorter school years in some states, which affected schooling duration in the opposite direction. Thus, in column 2, we alternatively measure schooling duration in terms of grades (rather than calendar years). The effect is similar in magnitude and less sensitive to the inclusion of covariates. Column 3 confirms that the reform significantly affected the probability of attending school for more than eight years. To support the internal validity of our results, the last column shows no effects on school starting age. This is not surprising and can be viewed as a placebo test because the reform affected students at least eight years after their school entry.

An average increase in years of schooling of nearly 0.6 is plausible given that compulsory schooling requirements were mostly binding for students attending the basic track in secondary school, which refers to approximately 50 percent of the cohorts under study. The estimate is also in line with earlier findings although its magnitude varies considerably across studies, depending on the data, schooling measure, and exact sample restrictions from 0.2 (e.g., Pischke and von Wachter, 2008)to more than 0.9 (e.g., Kamhöfer and Schmitz, 2016). Our estimate is very similar to Siedler (2010), Kemptner et al. (2011), Margaryan et al. (2021), Bömmel and Heineck (2023), Huebener (2022), Kemptner et al. (2011).

Next, we shed more light on compliance with school enrollment cutoffs. In Section 4.1, we argued that most parents adhere to the regulations, but not all comply with the sharp cutoffs due to legal exceptions for early enrollment. The top panel of Appendix Figure A9 illustrates the relationship between the cutoff and the timing of school entry. We observe a relatively smooth downward trend in school starting age for individuals born before the cutoff, followed by a substantial discontinuity of approximately 0.4 years after the cutoff. Column 1 of Table 3 confirms the estimated magnitude of the discontinuity in a regression framework. Panel A reports the result from RDD estimations of equation (2), which includes linear trends in the running variable fitted separately on both sides of the cutoff. In Panel B, we add the same set of covariates as in Table 2. Column 2 shows a 40-percentage point increase in the probability of being relatively old for grade[24] for children born after the cutoff. In column 3, we examine whether the extent of compliance differs on both sides of the cutoff. The results indicate that children born after the cutoff have a 16-percentage point lower probability of enrolling in the year they are expected to, according to the sharp cutoff rule. This finding supports the idea that some parents take advantage of the statutory exceptions for early enrollment. However, the remarkable stability of the point estimates across the panels strongly suggests that compliance is not systematically correlated with background characteristics.

Finally, in the last column of Table 3, we examine the mid-run consequences of the cutoff rules for secondary school track placement. The point estimate indicates that being born after the cutoff increases the probability of being tracked to the academic track (Gymnasium) by at least 5 percentage points. The bottom panel of Appendix

[24] Being old for grade is an alternative measure commonly used in recent literature on school starting age (e.g., Landers"o et al., 2020). We define old for grade as an indicator that a child enters school in the year of its seventh instead of sixth birthday.

Figure A9 provides graphical evidence for this effect, whose magnitude is large compared to the sample mean of 20 percent. These conclusions hold regardless of whether we use a first or second-order polynomial to approximate the underlying trends in the running variable on either side of the cutoff. Our estimates generally confirm earlier findings for Germany from more recent birth cohorts (e.g., Puhani and Weber, 2008, Mühlenweg and Puhani, 2010, Dustmann et al., 2017, Görlitz et al., 2022).

5.2 Long-run effects on regional mobility

In this Section, we present our main results on the effects of both policies on regional mobility measured in adulthood. We begin with estimating the average effects at ages 25-55. For this purpose, we pool the data on age-specific outcomes and cluster the standard errors at the individual level to account for repeated occurrences of each individual in the age-year panel.

Table 4 summarizes our main findings on the effects of compulsory schooling extensions estimated within a DD framework. Each point estimate comes from a separate linear regression of a specific mobility measure on the Reform dummy as in Equation (1). All regressions include state and birth date fixed effects. As in Table 2 for educational outcomes, in addition to our main specification (Panel A), we also report the results from an extended specification that includes a rich set of covariates (Panel B). Reassuringly, both panels yield very similar results. In particular, all point estimates are relatively small in magnitude and statistically insignificant. Thus, despite the substantial effect on schooling duration, the reform did not significantly affect individuals' mobility behavior. This holds for both cross-state and cross-county mobility.

In Section 5.4, we demonstrate that these findings are robust to alternative specifications and sample restrictions such as augmented models that make the parallel trends assumption more plausible, excluding the always-treated states (Goodman-Bacon, 2021), and an alternative TWFE estimator that accounts for the potential bias from effect heterogeneity (Wooldridge, 2021).

Next, we turn to the estimated discontinuities at the cutoff for school enrollment. Table 5 shows the results of the RDD regressions of Equation (2). Each coefficient comes from a separate linear regression of a given mobility outcome on the After dummy. All regressions include linear trends in the running variable separately fitted on either side of the cutoff. Again, the specifications without and with additional covariates (Panels A and B, respectively) yield nearly identical results. None of the point estimates is statistically significant and none of them implies a positive effect on mobility. In contrast, most of the estimates are negative, and the results for lifetime mobility suggest relatively large reductions in interstate mobility of 8-9 percent and cross-county mobility of 5-6 percent if compared to the respective sample means. However, given the imprecision of the estimates, we are reluctant to draw any strong conclusions about the potentially adverse effects.

In Section 5.4, we show that the results remain remarkably robust in various standard sensitivity analyses such as a non-donut specification, models with a more flexible function in the running variable, and narrowing the bandwidths around the cutoff to the preferred bandwidth by optimizing the coverage error rate (Calonico et al., 2020a). We also run non-parametric local polynomial regressions (Cattaneo et al., 2020).

Taken together, our results consistently suggest that, despite some positive effects on educational outcomes in adolescence, school entry and exit laws do not significantly increase regional mobility in Germany. While in Tables 4 and 5 we focus on effects averaged over the prime working ages (25 to 55), by estimating age-specific regressions we find that the effects are very stable over nearly the entire life cycle (see Appendix Figure A10). The fact that we do not find any significant effects of the compulsory schooling reform on mobility outcomes measured before the age of 15 (left panel), when it hit the affected individuals, also supports the validity of our empirical design.

Previous studies that used compulsory schooling laws to identify the causal effect of education on internal mobility within a two-stage-least-squares (2SLS) approach have produced inconclusive results. For example, using Norwegian data for birth cohorts from 1947 to 1958, Machin et al. (2012) found that an additional year of schooling increases the 1-year cross-county migration rate by 15 percent. Scaling our reduced-form effect for this specific outcome by the first stage yields a 2SLS estimate of approximately 3.5 percent relative to the sample mean. This effect is substantially lower and statistically insignificant. In contrast, McHenry (2013) found that one year of schooling reduces the 5-year cross-state migration rate by 9 percent for the US cohorts born between 1990 and 1964. The corresponding 2SLS estimate from our results would imply a 3.5 percent reduction in mobility when compared to the sample mean. Again, the effect for Germany is much lower and statistically insignificant. As for the school entry cutoffs, to the best of our knowledge, there is so far no evidence of their potential consequences for geographic mobility.

5.3 Potential mechanisms (to be completed)

Since at least Sjaastad (1962) work, economists have viewed migration as an investment decision, similar to schooling. Following this concept, education can affect an individual's location choices through several channels. First, education may enhance individuals' ability to react to disequilibria (Schultz, 1975), such that they migrate in response to regional differences, e.g., in wages or employment opportunities. This assumes that increased education enhances individuals' ability to acquire and interpret information accurately or/and the willingness take actions that result in appropriate relocation. Thus, this channel requires that increased education leads to an improvement in cognitive abilities or/and risk attitudes.

Second, education may impact migration behavior if local labor markets for higher-educated workers become relatively thin. This mechanism requires that education affects educational credentials that are transferable between regions. This may be particularly important in countries like Germany, where secondary school degrees and post-secondary diplomas play a crucial role in certifying a person's knowledge and skills acquired from education.

Regarding the potential impact of the German compulsory schooling reform on cognitive skills, Kamhöfer and Schmitz (2016) found no statistically significant effect using an ultra-short word fluency test available in the SOEP 2006. The test required respondents to name as many animals as possible within 90 seconds. For enrollment cutoffs, Görlitz et al. (2022) found long-lasting imprints on skills using more comprehensive measures available in the NEPS data. Specifically, they show that individuals born after the cutoff score significantly higher on receptive vocabulary, where respondents assigned pictures to a single word given by the interviewer by choosing from four possibilities. However, there was no effect on math skills or text comprehension, i.e., the ability to draw text-related conclusions, reflect, and assess.

Earlier evidence suggests that both policies do not enhance individuals' capacity to accurately interpret information in Germany, which is consistent with no impact on geographic mobility through the skill channel. To validate these conclusions, we will reassess the earlier findings in the future, using our sample restrictions and different skill measures in NEPS, such as reading and mathematics competencies and risk attitudes (for details, see, e.g., Weinert et al., 2011)....

Next, we examine the role of academic credentials. The estimated effects of compulsory schooling reform and enrollment cutoffs are reported in Appendix Tables A6 and A7, respectively. Generally, the point estimates in columns 1 through 3 suggest a shift away from the basic school degree towards the completion of higher degrees, but the estimates lack precision. Additionally, the last column shows no significant effect on college graduation. Thus, we find that extended compulsory schooling and the benefits of being born after the cutoff for secondary school track placement do not translate into better academic credentials. Taken together, we do not find any meaningful consequences of the two policies for important channels through which education could impact regional mobility.

5.4 Sensitivity analysis

This section examines the robustness of our findings across alternative model specifications and data choices. The results for the effects compulsory schooling reform and school enrollment cutoffs are presented in Appendix Tables A8 and A9, respectively. For comparability, the top panel of each table reproduces the baseline results.

Regarding the effects of the compulsory schooling reform (see Appendix Table A8), our results are almost unchanged when we control for potentially different trends in school quality across states, approximated by the state-specific student-teacher ratio (Panel A). Alternatively, we include state-specific year of birth fixed effects (Panel B), which flexibly capture any cohort-specific developments in outcomes across the states. The stability of our results from the extended model specifications support the parallel trends assumption.

Next, we test the robustness of our results to the exclusion of always-treated states (Panel C), which may bias the conventional TWFE estimator that uses them as a control group (Goodman-Bacon, 2021). Despite the smaller sample size, our conclusions still hold. Our results are also robust to the use of the extended TWFE estimator proposed by Wooldridge (2021) (Panel D). Both sensitivity tests suggest that treatment effect heterogeneity is not a major issue in our main analysis relying on the conventional TWFE estimator.

We also test the robustness of our results to alternative data choices. Specifically, in Panel E we assign treatment using state of residence at age 12 instead of 14. Reassuringly, the estimates remain consistent with our baseline results, suggesting that potentially endogenous mobility prior to the implementation of the reform does not affect our results. For our main analysis, we use sample weights to account for the over-representation of better-educated individuals in the NEPS data. However, the estimates do not substantially change if we omit the weights from the regressions (Panel F). Finally, we conduct a falsification test by estimating the effects of a "placebo reform" (Panel G). We do this by randomly assigning implementation dates across states. Again, the results provide confidence in the internal validity of our empirical design.

Table A9 in the Appendix summarizes the results of the sensitivity analysis for the mobility responses to the school enrollment cutoffs. First, we show that our results remain nearly identical when we include additional covariates. For example, in Panel A, we control for the student-to-teacher-ratio measured at the age of 6. Given that the enrollment cutoffs are state-specific and may be based on different calendar months, in Panel B, we add cutoff month fixed effects. This specification captures potential seasonality effects in the cutoff rules, but yields nearly identical estimates.

Next, we perform some standard sensitivity analyses for RDD designs. For example, we estimate models with a more flexible function in the running variable by adding quadratic trends in the week of birth (Panels C and D). Apart from slightly larger point estimates in some cases and lower precision, the alternative specifications lead to similar conclusions. A similar pattern emerges when we estimate the mobility effects non-parametrically (Panel E) using local polynomial regressions. Specifically, we use the robust bias-corrected estimator proposed by Calonico et al. (2020b), which flexibly estimates the underlying trends in outcomes on either side of the cutoff, selects the optimal bandwidths in a data-driven manner, and provides bias-corrected inference.[25] The non-parametric estimation supports our main conclusion, although it typically suggests optimal bandwidths of only about 20 weeks around the cutoff. In Panel F, we show that applying the optimal bandwidths to our parametric regressions also does not affect our baseline results (Panel F). The results are also robust to the inclusion of individuals with birth dates within the donut hole (Panel G), who generally have lower compliance with the enrollment cutoffs.

We also test the robustness to omitting the sample weights (Panel H), which yields results consistent with our baseline estimates. Finally, we estimate the effects of "placebo cutoffs" (Panel I). We do this by shifting the actual cutoff date six months to the left. As expected, we find no significant results in this falsification test, supporting the validity of our main estimates.

6 Conclusions

Geographic mobility is an important determinant of economic outcomes at both the macro and the micro level. Germany is commonly viewed as a country with low internal mobility rates, but the patterns and determinants of this phenomenon have received little attention in research. We focus on the role of education, as it has been long recognized as the key factor for understanding why some individuals move across regions and others do not. Using unique data on detailed residential biographies and educational paths of individuals born in Germany between 1944 and 1986, we provide a comprehensive and detailed analysis of regional mobility patterns in Germany and

[25] We use 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.

investigate causality in the education-mobility gradient.

We begin by documenting some fundamental facts on the extent of internal migration in Germany across the life cycle. Contrary to the common conjecture that regional mobility in Germany is generally low, we find substantial differences across the life course, space, time, and socio-demographic groups. Specifically, major location changes occur around important educational decisions. Beyond regional and gender differences in age-mobility profiles, the most striking disparities occur by educational attainment.

We then turn to the question of whether there is a causal link between education and mobility. We do so by exploiting two arguably exogenous sources of variation, each inducing a shift at a different margin of the educational distribution. First, we exploit a compulsory schooling reform that aimed to increase the duration of schooling for students at the bottom of the ability distribution (e.g., Pischke and von Wachter, 2008). Second, for the same school cohorts, we study the mobility responses to statutory cutoff rules for school enrollment, which have been shown to increase the probability of attending the highest ability track in secondary school (e.g., Dustmann et al., 2017). This enables us to compare the potential effects on internal mobility at different margins of the ability distributions, for the same generation, and within the same context. Using the difference-in-differences and regression discontinuity designs, we find no effect on internal mobility. We also shed light on the potential mechanisms for this finding. We conclude that none of the two policies had a meaningful impact on important channels through which education could impact regional mobility such as cognitive skills and academic credentials.

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

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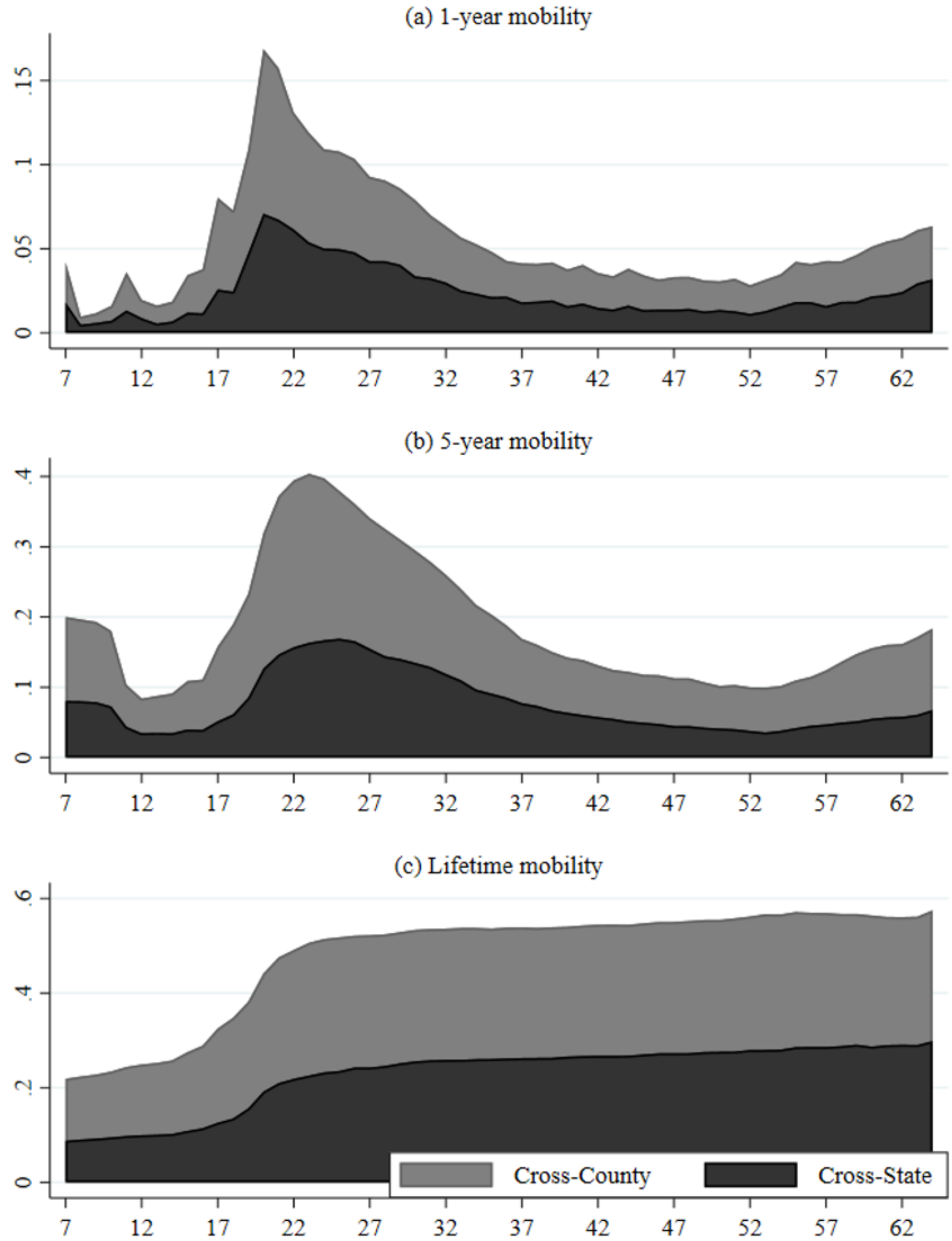
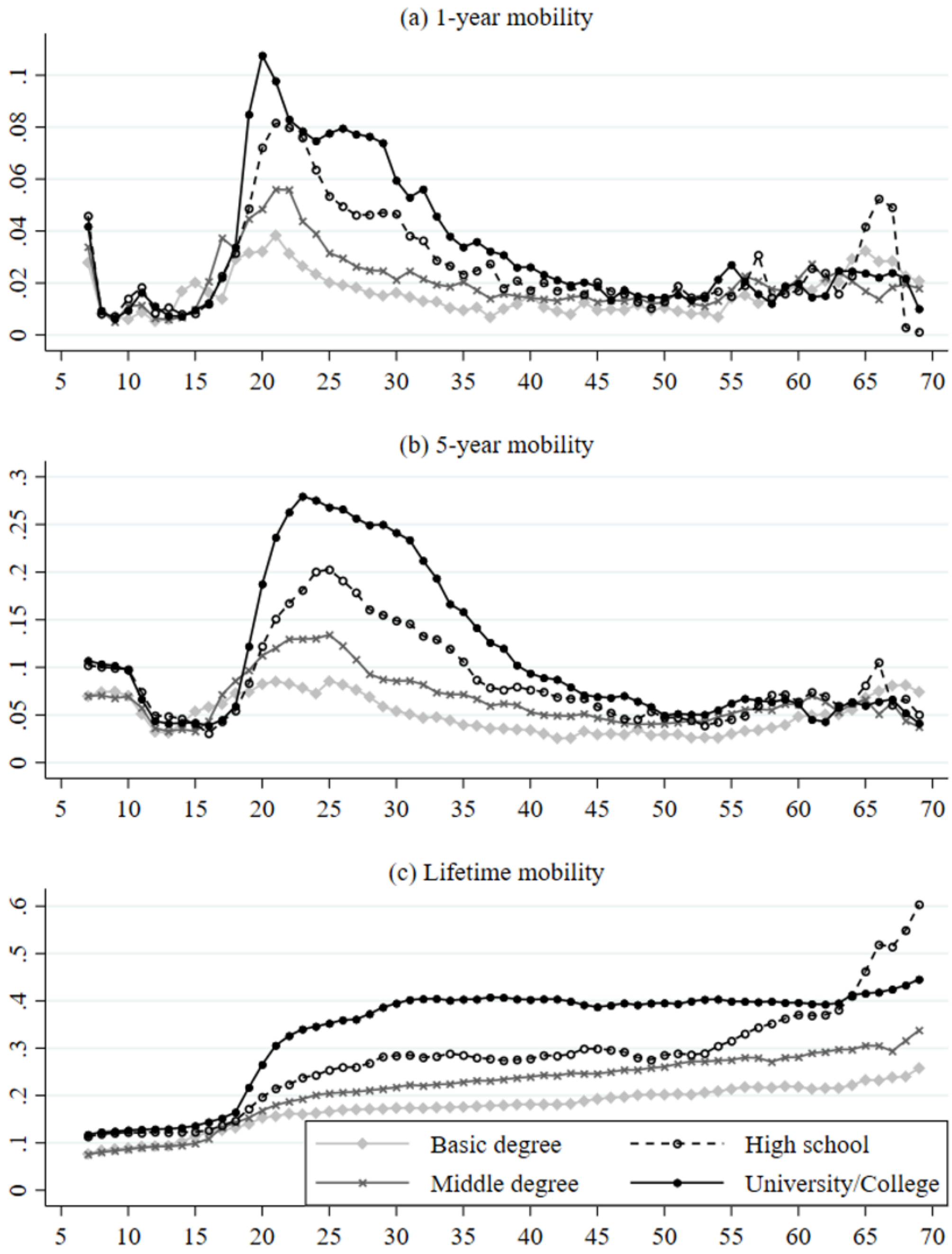
Figure 1: Age-specific cross-state and cross-county mobility rates



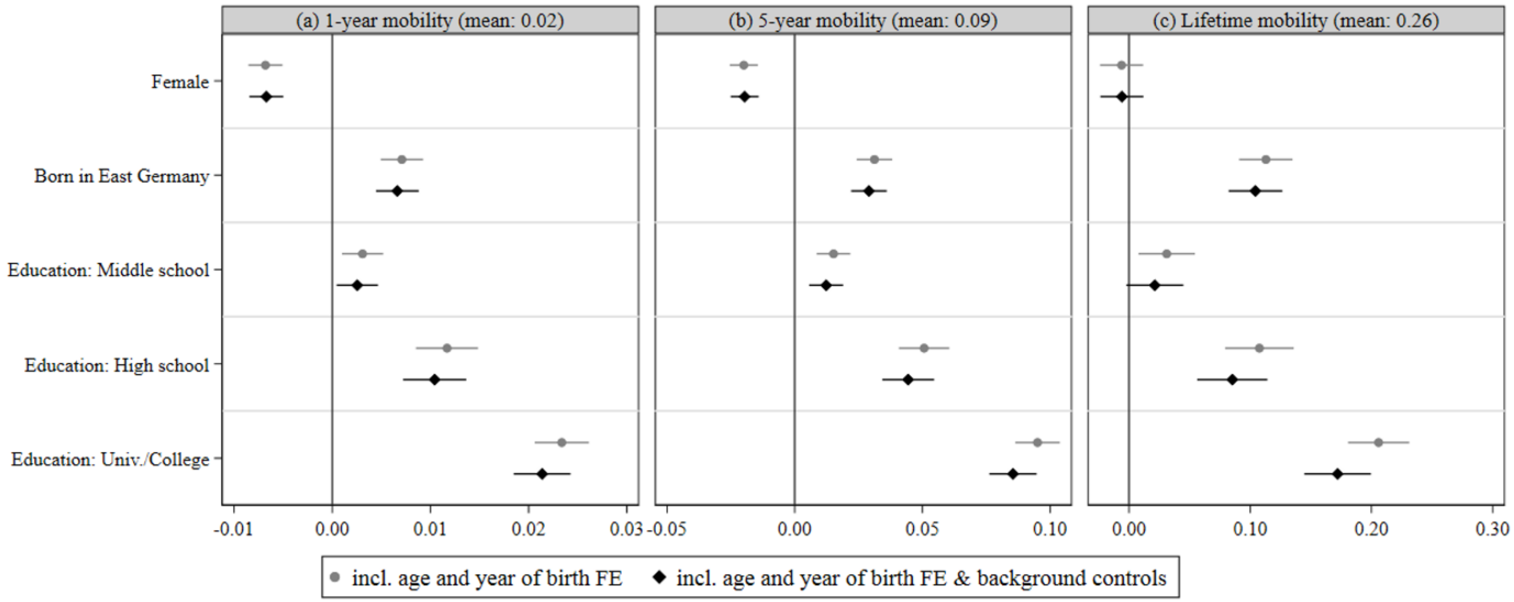
Note: Sample restricted to individuals born in Germany. Data weighted using a cross-sectional weight calibrated to Micro Census 2011.
Source: NEPS SC6:12.1.0.

Figure 2: Age-specific cross-state and cross-county mobility rates



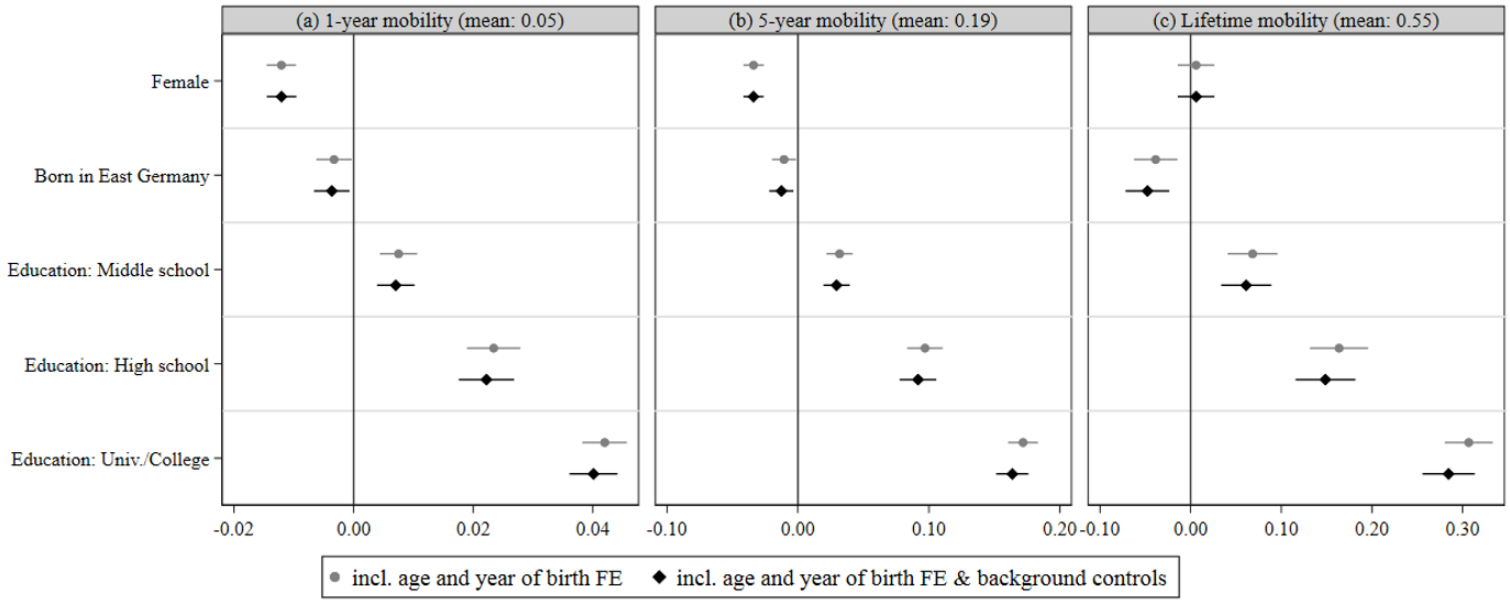
Note: Sample restricted to individuals born in Germany. Data weighted using a cross-sectional weight calibrated to Micro Census 2011.
Source: NEPS SC6:12.1.0.

Figure 3: Determinants of cross-state mobility in adulthood



Note: Sample restricted to individuals born in Germany and age years 25-55. The figure plots the estimates from pooled OLS regressions of various mobility measures (shown in separate panels) on indicators for gender (reference is male), being born in East German states (reference is West), and educational attainment (reference is basic degree). All regressions include age and year of birth fixed effects (FE). The extended specifications (black diamonds) additionally include individual background characteristics such as parental education and citizenship, maternal age at birth, an individual's birth order, kindergarten attendance, and dummies for missing information on each covariate. All regressions use a cross-sectional survey weight calibrated to Micro Census 2011. The estimation sample consists of 319,068 person-age year observations on 12,612 individuals. The 95*Source:* NEPS SC6:12.1.0.

Figure 4: Determinants of cross-county mobility in adulthood



Note: Sample restricted to individuals born in Germany and age years 25-55. The figure plots the estimates from pooled OLS regressions of various mobility measures (shown in separate panels) on indicators for gender (reference is male), being born in East German states (reference is West), and educational attainment (reference is basic degree). All regressions include age and year of birth fixed effects (FE). The extended specifications (black diamonds) additionally include individual background characteristics such as parental education and citizenship, maternal age at birth, an individual's birth order, kindergarten attendance, and dummies for missing information on each covariate. All regressions use a cross-sectional survey weight calibrated to Micro Census 2011. The estimation sample consists of 319,068 person-age year observations on 12,612 individuals. The 95 percent confidence intervals are based on standard errors clustered at the individual level. *Source:* NEPS SC6:12.1.0.

Table 1: Sample Means

	Full sample, cohorts 1944-1986	West Germany, cohorts, 1945-1964	Compulsory schooling sample	Enrollment cutoffs sample
Migration measures (at age 25-55)				
1-year cross-state	0.02	0.02	0.02	0.02
1-year cross-county	0.05	0.04	0.04	0.04
5-year cross-state	0.08	0.06	0.06	0.06
5-year cross-county	0.18	0.15	0.15	0.15
Lifetime cross-state	0.25	0.22	0.22	0.22
Lifetime cross-county	0.51	0.53	0.53	0.53
Socio-demographic characteristics				
Year of birth	1964.78	1955.67	1955.55	1955.55
Month of birth	6.40	6.47	6.44	6.44
Female	0.50	0.52	0.52	0.52
Born in East-Germany	0.74	0.00	0.00	0.00
State: Schleswig-Holstein	0.03	0.05	0.05	0.05
State: Hamburg	0.02	0.03	0.03	0.03
State: Lower Saxony	0.10	0.13	0.13	0.13
State: Bremen	0.01	0.02	0.02	0.02
State: Nordrhein-Westphalia	0.21	0.28	0.28	0.28
State: Hesse	0.07	0.08	0.07	0.07
State: Rheinland-Palatinate	0.05	0.07	0.07	0.07
State: Baden-Wurttemberg	0.11	0.15	0.14	0.14
State: Bavaria	0.14	0.18	0.18	0.18
State: Saarland	0.01	0.02	0.02	0.02
Parental education (in years)	11.64	11.02	11.00	11.00
Parental education: missing	0.02	0.02	0.02	0.02
Non-German parent	0.97	0.98	0.98	0.98
Non-German parent: missing	0.01	0.01	0.00	0.00
Maternal age at birth (in years)	27.53	28.28	28.28	28.28
Maternal age at birth: missing	0.05	0.04	0.04	0.04
Firstborn	0.49	0.52	0.52	0.52
Firstborn: missing	0.09	0.06	0.06	0.06
Kindergarten attendance	0.70	0.50	0.50	0.50
Kindergarten attendance: missing	0.02	0.01	0.01	0.01
Extended compulsory schooling	0.90	0.76	0.76	0.76
Exposed to short school years	0.12	0.32	0.31	0.31
Born in rural municipality	0.36	0.36	0.36	0.36
Educational outcomes				
School starting age (in years)	6.58	6.44	6.42	6.42
Academic track attendance	0.21	0.21	0.21	0.21
Duration of schooling (in years)	9.90	9.74	9.73	9.73
Highest school degree: basic	0.32	0.46	0.47	0.47
Highest school degree: middle	0.40	0.31	0.31	0.31
Highest school degree: high school	0.29	0.23	0.22	0.22
College/University degree	0.18	0.15	0.15	0.15
Individuals	12,612	5,260	4,652	4,652

Note: Sample restricted to individuals born in Germany. Mobility outcomes refer to individual-specific means calculated over ages 25-55.
Source: NEPS SC6:12.1.0.

Table 2: Immediate Effects of Compulsory Schooling Reform on Educational Outcomes

	(1) Duration of schooling	(2) Yrs of schooling (in grades)	(3) More than 8 yrs of schooling	(4) School starting age (placebo)
Panel A: DD regressions without controls				
Reform	0.590***	0.576***	0.374***	-0.188
	(0.137)	(0.115)	(0.031)	(0.173)
Panel B: DD regressions with controls				
Reform	0.703***	0.551***	0.401***	-0.118
	(0.149)	(0.117)	(0.033)	(0.163)
Y-Mean	9.704	10.150	0.791	6.441
Obs./Indiv.		5,259		

Note: Sample restricted to individuals born in West Germany between 1945 and 1964. The outcomes are measured at the individual level. Each cell is based on a separate linear regression of Equation (1) using a cross-sectional weight calibrated to Micro Census 2011. All regressions include state and birth date fixed effects. Controls comprise gender, parental education and citizenship, maternal age at birth, an individual's birth order, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Robust standard errors in parentheses.
Source: NEPS SC6:12.1.0.

Table 3: Immediate Effects of Being Born After the Cutoff on Educational Outcomes

	(1) School starting age (in years)	(2) Old for grade	(3) Enrollment year as expected	(4) Acad. track attendance
Panel A: RDD regressions without controls				
After	0.398***	0.398***	-0.169***	0.059*
	(0.054)	(0.039)	(0.040)	(0.031)
Panel B: RDD regressions with controls				
After	0.400***	0.394***	-0.163***	0.057**
	(0.052)	(0.038)	(0.039)	(0.028)
Y-Mean	6.417	0.415	0.704	0.206
Obs./Indiv.		4,652		

Note: Sample restricted to individuals born in West Germany between 1945 and 1964. The outcomes are measured at the individual level. Each cell is based on a separate linear regression of Equation (2) using a cross-sectional weight calibrated to Micro Census 2011. All regressions include linear trends in the running variable (week of birth) that are allowed to vary on both sides of the cutoff. Controls comprise gender, parental education and citizenship, maternal age at birth, an individual's birth order, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Robust standard errors in parentheses.
Source: NEPS SC6:12.1.0.

Table 4: Long-Run Effect of Compulsory Schooling Reform on Regional Mobility

	Cross-State Mobility			Cross-County Mobility		
	1-Year	**5-Year**	**Lifetime**	**1-Year**	**5-Year**	**Lifetime**
Panel A: DD regressions without controls						
Reform	0.000	-0.002	0.020	0.002	0.001	0.005
	(0.002)	(0.007)	(0.029)	(0.003)	(0.012)	(0.033)
Panel B: DD regressions with controls						
Reform	0.000	-0.005	0.017	0.003	-0.002	-0.023
	(0.002)	(0.008)	(0.031)	(0.004)	(0.013)	(0.036)
Y-Mean	0.016	0.061	0.221	0.039	0.154	0.527
Obs.			159,716			
Indiv.			5,260			

Note: Sample restricted to individuals born in West Germany between 1945 and 1964. The outcomes are measured at ages from 25 through 55. Each cell is based on a separate linear regression of Equation (1) using a cross-sectional weight calibrated to Micro Census 2011. All regressions include state and birth date fixed effects. Controls comprise gender, parental education and citizenship, maternal age at birth, an individual's birth order, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Standard errors in parentheses are clustered at the individual level.
Source: NEPS SC6:12.1.0.

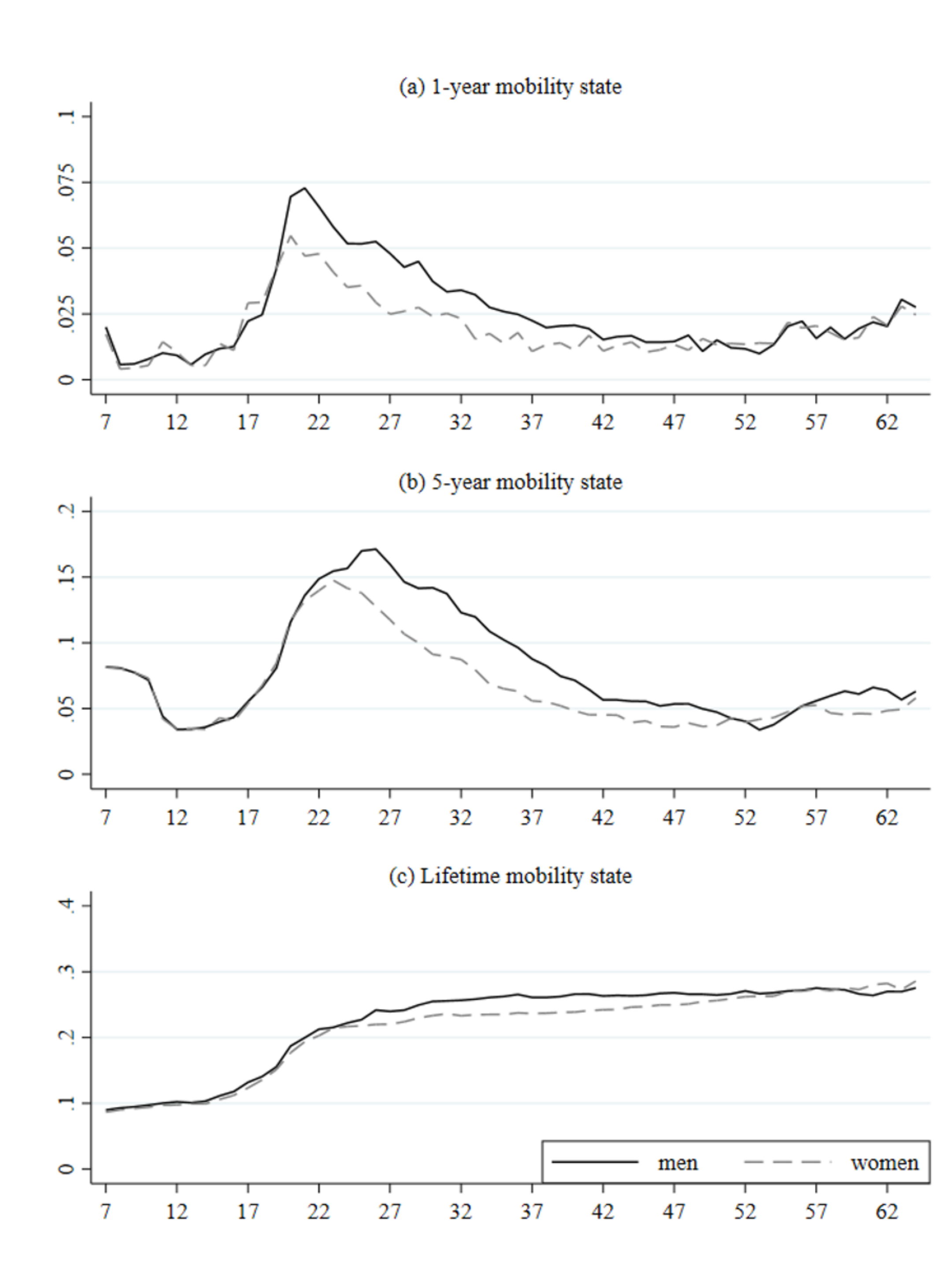
Table 5: Long-Run Effect of Being Born After the Cutoff on Regional Mobility

	Cross-State Mobility			Cross-County Mobility		
	1-Year	**5-Year**	**Lifetime**	**1-Year**	**5-Year**	**Lifetime**
Panel A: RDD regressions without controls						
After	0.002	0.002	-0.023	-0.001	-0.014	-0.031
	(0.002)	(0.008)	(0.032)	(0.004)	(0.013)	(0.038)
Panel B: RDD regressions with controls						
After	0.001	0.001	-0.025	-0.002	-0.015	-0.030
	(0.002)	(0.008)	(0.030)	(0.004)	(0.012)	(0.037)
Y-Mean	0.015	0.060	0.224	0.039	0.154	0.531
Obs.			140,414			
Indiv.			4,652			

Note: Sample restricted to individuals born in West Germany between 1945 and 1964. The outcomes are measured at ages from 25 through 55. Each cell is based on a separate linear regression of Equation (2) using a cross-sectional weight calibrated to Micro Census 2011. All regressions include linear trends in the running variable (week of birth) that are allowed to vary on both sides of the cutoff. Controls comprise gender, parental education and citizenship, maternal age at birth, an individual's birth order, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Standard errors in parentheses are clustered at the individual level.
Source: NEPS SC6:12.1.

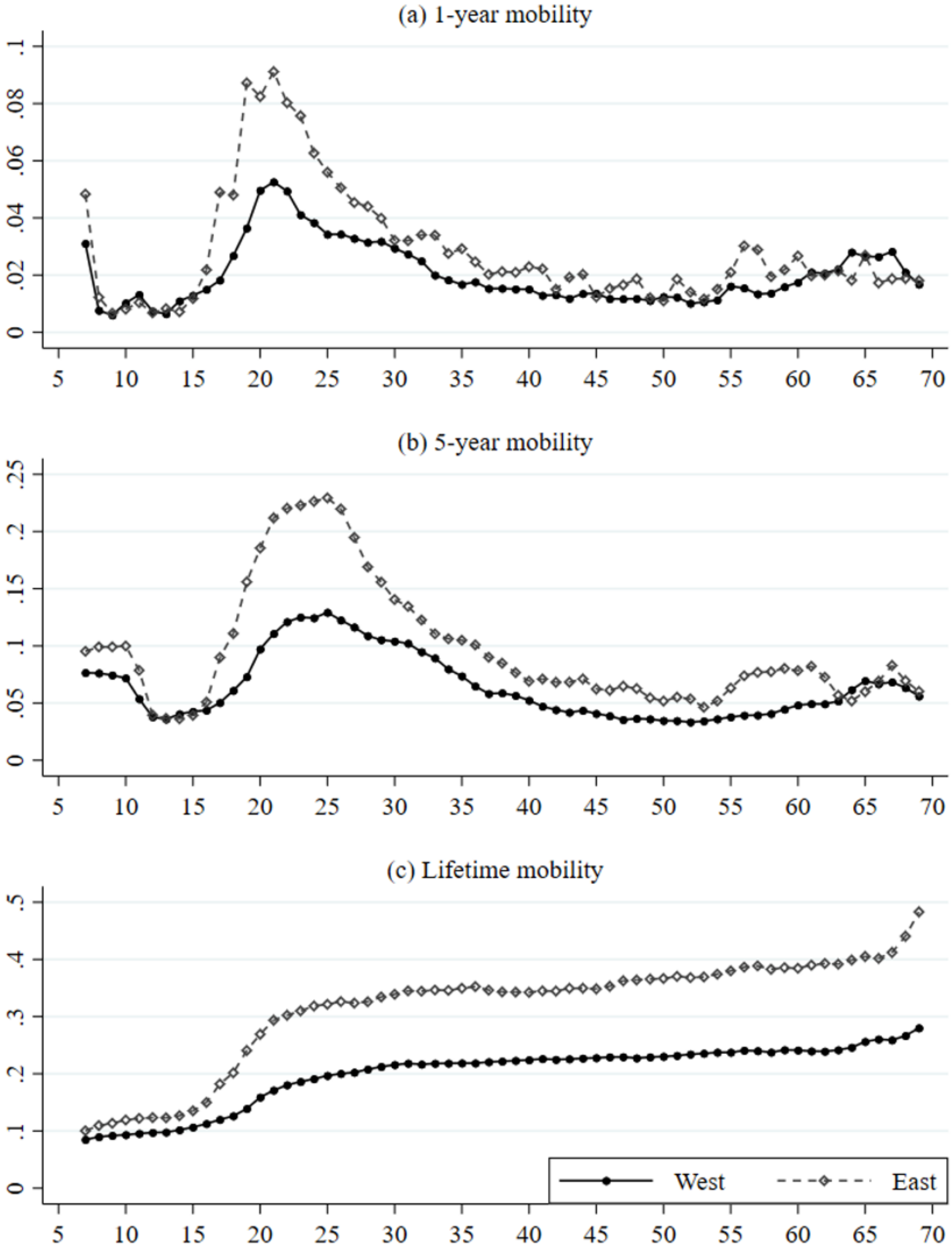
Appendix

Figure A1: Age-specific cross-state and cross-county mobility rates



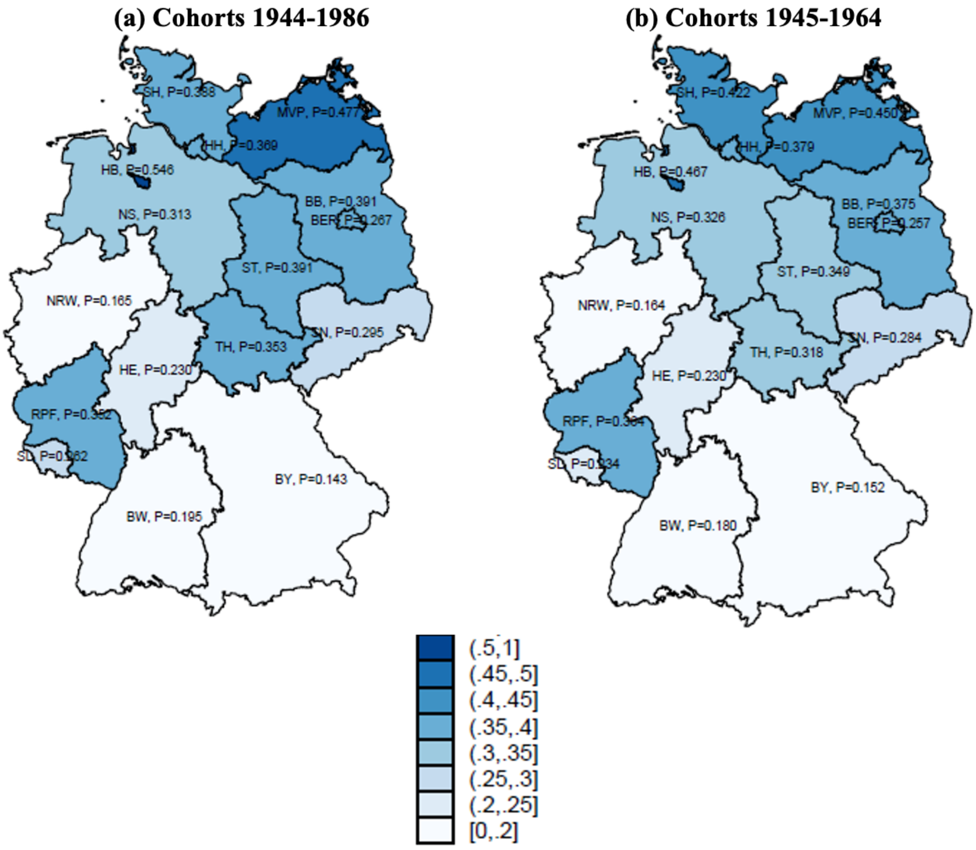
Note: Sample restricted to individuals born in Germany. Data weighted using a cross-sectional weight calibrated to Micro Census 2011. *Source:* NEPS SC6:12.1.0.

Figure A2: Age-specific cross-state mobility rates by the region of births



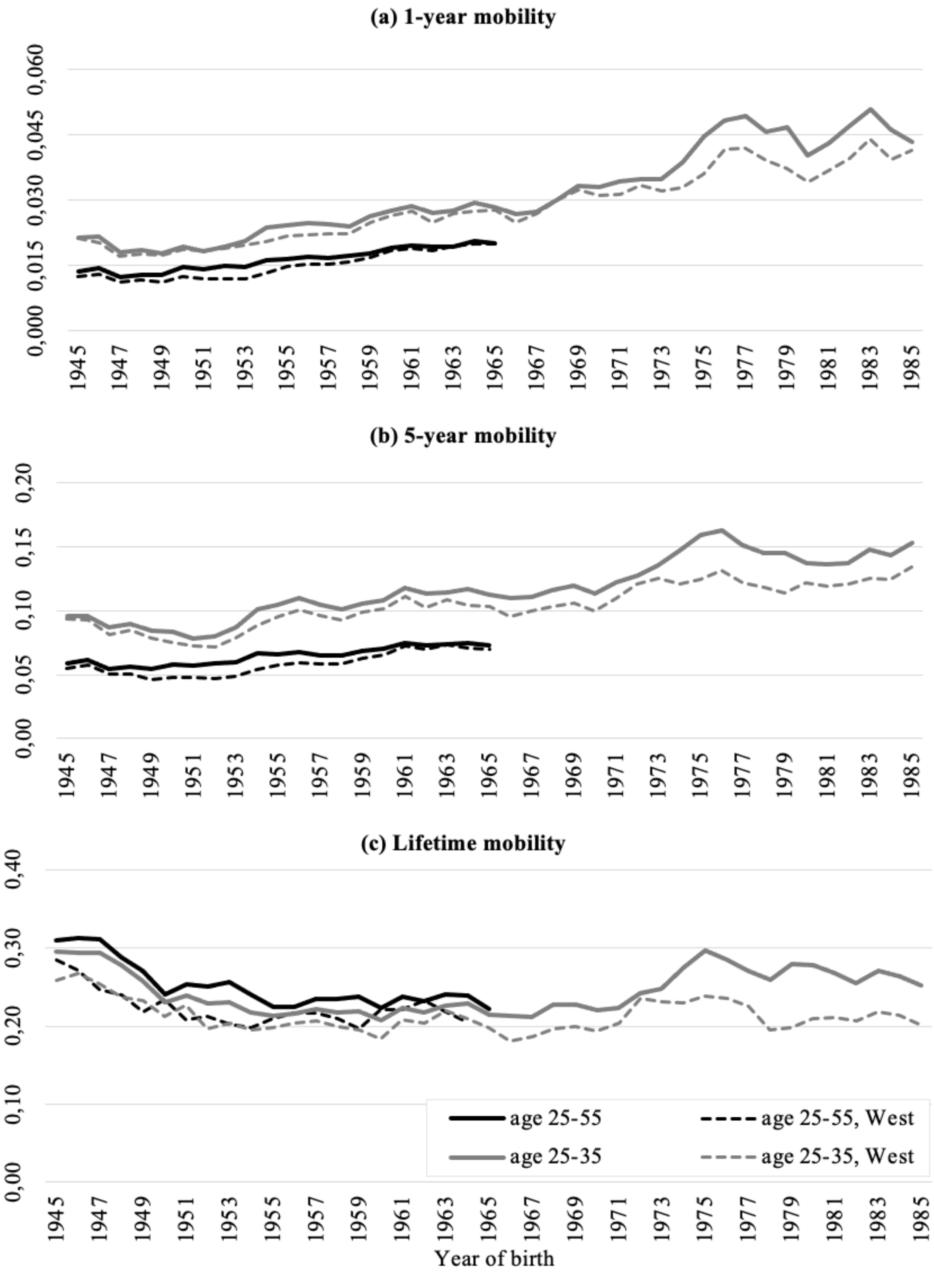
Note: Sample restricted to individuals born in Germany. Data weighted using a cross-sectional weight calibrated to Micro Census 2011. *Source:* NEPS SC6:12.1.0.

Figure A3: Cross-state mobility rates in adulthood by birth cohort and state of birth



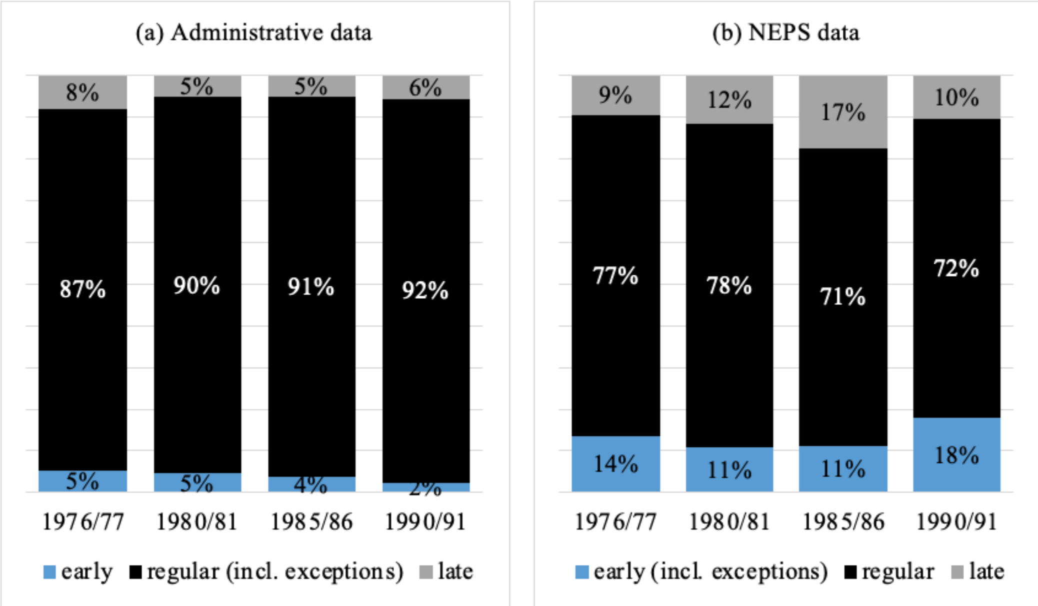
Note: Sample restricted to individuals born in Germany. Mobility is measured at ages 25-55. Data weighted using a cross-sectional weight calibrated to Micro Census 2011.
Source: NEPS SC6:12.1.0.

Figure A4: Trends in cross-state mobility over time



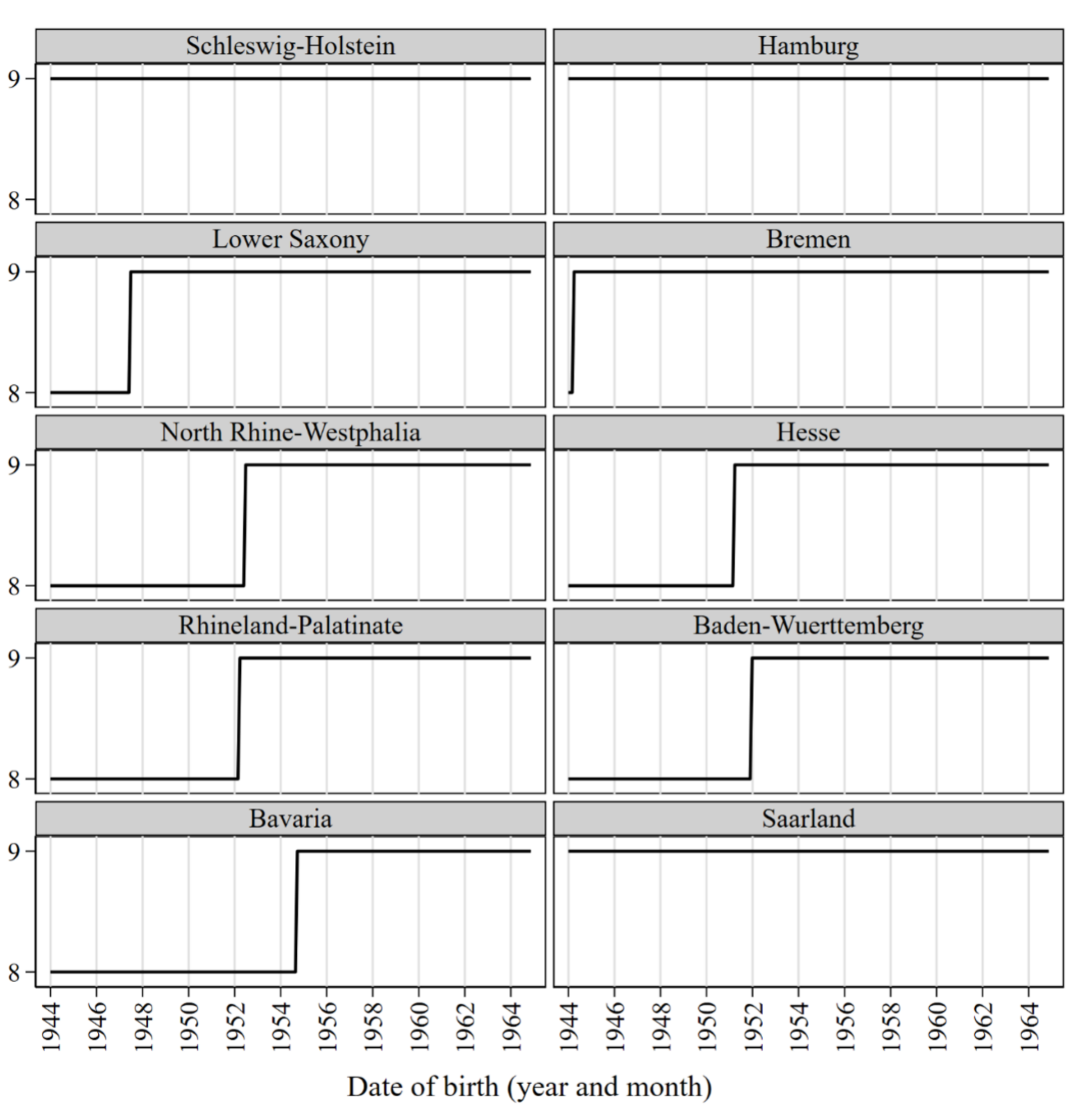
Note: Sample restricted to individuals born in Germany. Data weighted using a cross-sectional weight calibrated to Micro Census 2011. To smooth the data, the trends show three-year moving averages (i.e., including -/+1 year) instead of year-specific means.
Source: NEPS SC6:12.1.0.

Figure A5: School starters by the type of enrollment



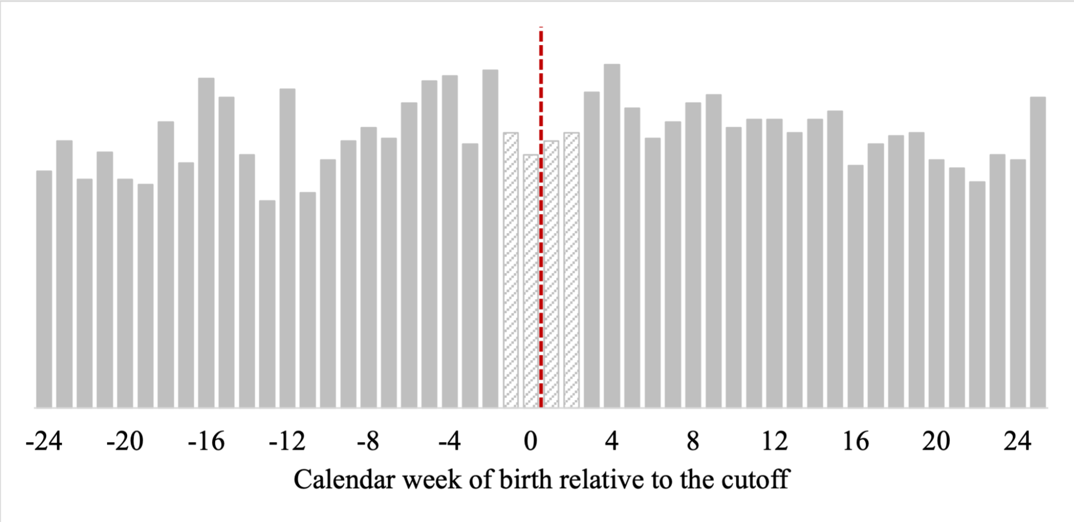
Note: The figures show the relative numbers of students enrolled in a particular school year by the enrollment type. Save for 1990/1, the numbers include only West German states (incl. West Berlin). *Source:* The administrative data are from various years of "Fachserie 11, Reihe 1, Bildung und Kultur, Allgemeinbildende Schulen" published annually by DESTATIS (Federal Statistical Office, Wiesbaden); NEPS SC6:12.1.0.

Figure A6: Compulsory schooling requirement by state and birth cohort



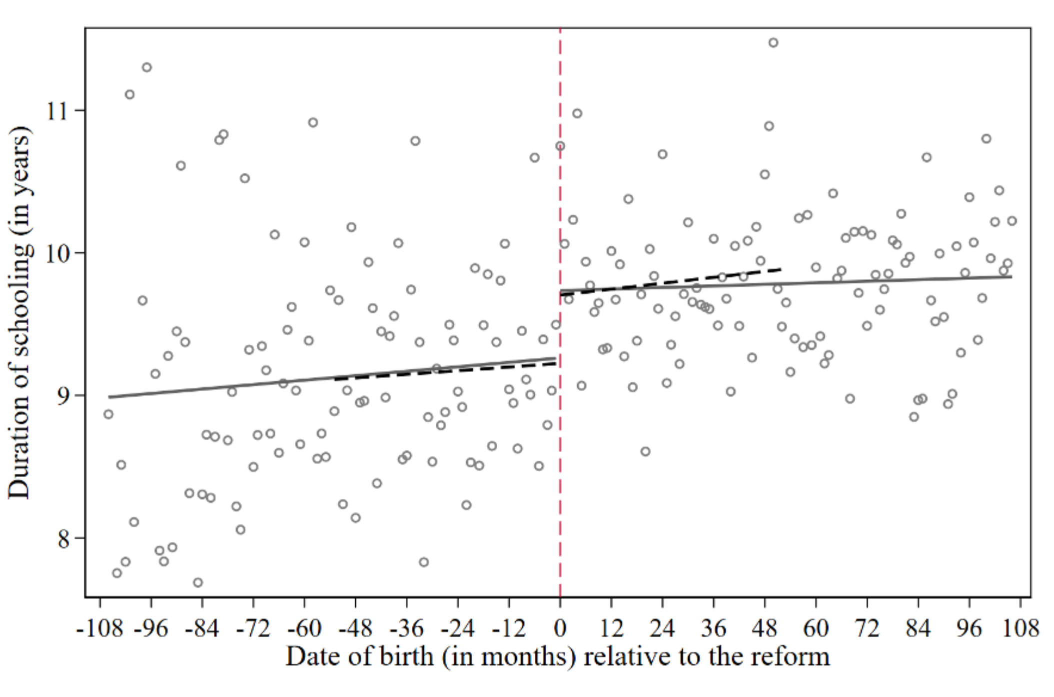
Note: The figure shows the required duration of compulsory schooling depending on the date of birth, which determines the expected year of school enrollment.
Source: State-specific laws from Makrolog (2019). Further details available on request

Figure A7: Distribution of births by the running variable



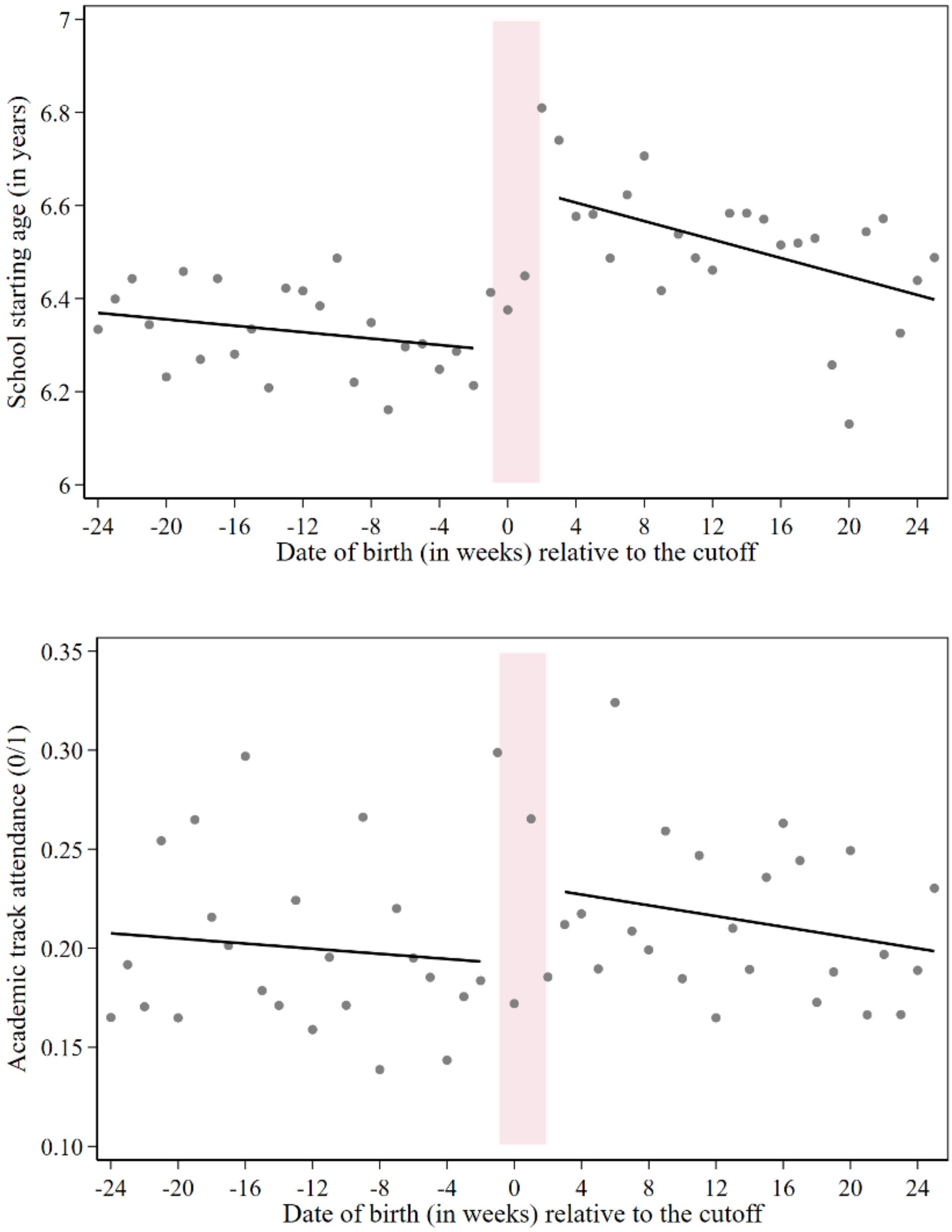
Note: The figure shows the number of individuals in our estimation sample by the calendar week of birth relative to the cutoff for school enrolment. The lighter bars indicate the range of the running variable excluded in our donut-hole RDD regressions (-/+2 points). The density test using the robust inference procedure recommended by Cattaneo et al. (2020) yields a p-value of 0.5882.
Source: NEPS SC6:12.1.0.

Figure A8: Average duration of schooling by birth cohort relative to the first cohort affected by compulsory schooling extensions



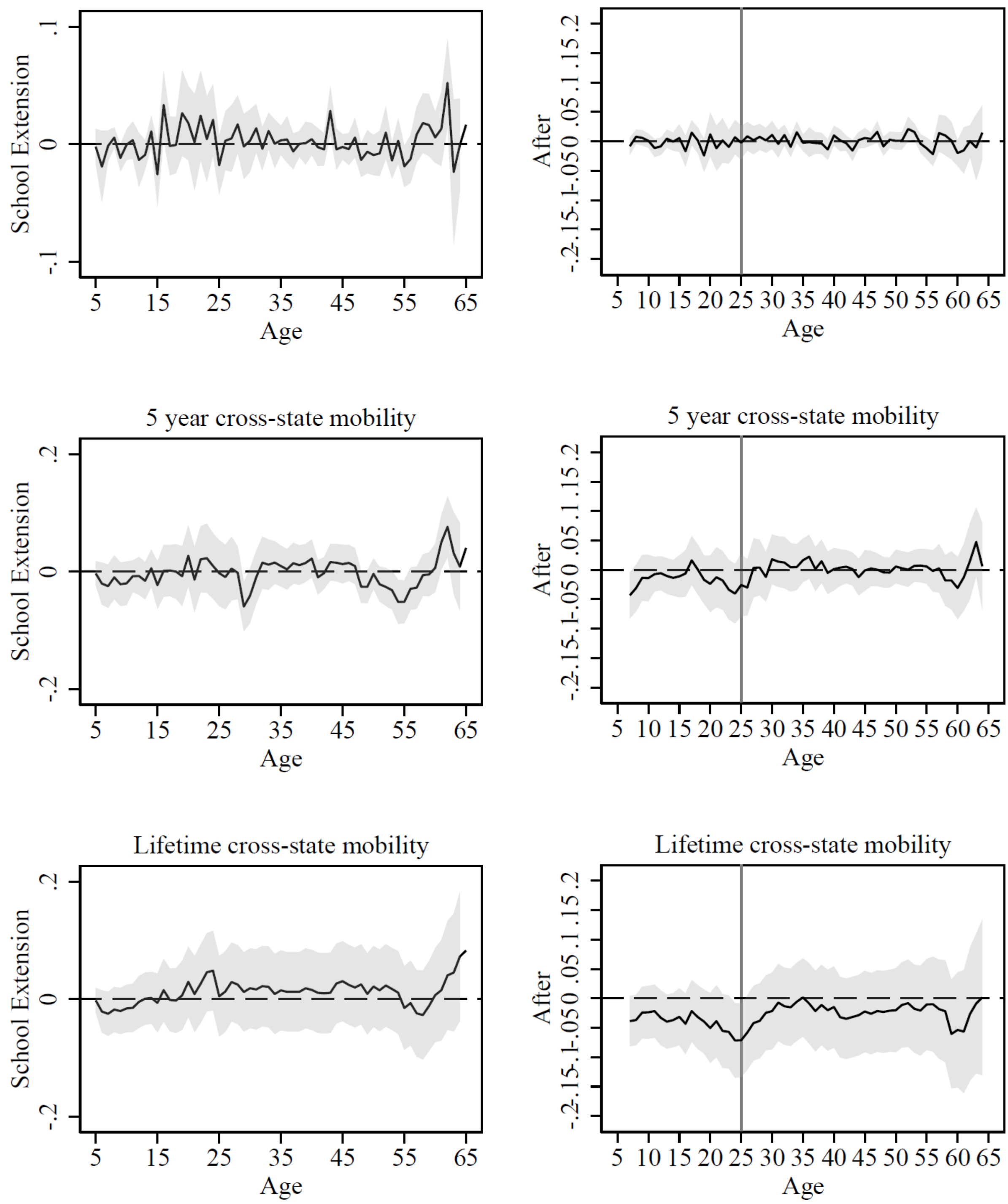
Note: Duration of schooling (in years) is measured in calendar time (not in grades). The variable is calculated as the difference between the date an individual left school and the date he/she entered school. Birth date on the x-axis is measured in months relative to the first birth cohort exposed to nine instead of eight years of compulsory schooling in the individual's state of residence at age 14. The vertical line marks the first affected cohort. The horizontal black solid lines correspond to linear trends fitted separately for cohorts born 9 years (i.e., 108 months) before and after the reform. The horizontal grey dashed lines correspond to linear trends fitted separately for cohorts born 4.5 years (i.e., 54 months) before and after the reform. The data are unbalanced across the relative date of birth, i.e., the further away from the reform's introduction, the fewer observations are available for calculating the means.
Source: NEPS SC6:12.1.0.

Figure A9: Being born after the cutoff and short-term educational outcomes



Note: School starting age (in years) is calculated as the difference between the date of an individual's school entry and his/her date of birth. Academic track attendance is an indicator of whether an individual attended the academic track in secondary school The date of birth on the x-axis is measured in calendar weeks relative to the cutoff for school enrolment in the individual's state of residence at age 6. The shaded area marks the donut hole of +/- 2 weeks around the cutoff.
Source: NEPS SC6:12.1.0.

Figure A10: Life-cycle effects on cross-state mobility



Note: The left panel plots the age-specific estimates of Equation 1 and the right panel of Equation 2. Each estimate is from a separate linear regression of the outcome at a given age on the *Reform* or the *After* dummy, respectively. For details on the model specifications, see Tables 4 and 5, respectively. Shaded areas show 95% confidence intervals based on standard errors clustered at the individual level. *Source:* NEPS SC6:12.1.0.

Table A1: Comparison of Cross-Sectional Samples from the NEPS and Micro Census

	2008			2011		
	NEPS unweighted	**NEPS weighted**	**Micro Census**	**NEPS unweighted**	**NEPS weighted**	**Micro Census**
Age	42.49	41.23	41.56	43.28	41.66	41.89
Year of birth	1965	1966	1966	1968	1970	1970
Month of birth	6.334	6.346	6.356	6.355	6.358	6.365
Female	0.519	0.509	0.501	0.517	0.497	0.498
High school degree	0.412	0.297	0.316	0.449	0.332	0.342
Individuals	9,508	9,508	226,109	8,428	8,428	208,553

Note: Samples restricted to ages 25–55 in calendar years 2008 and 2011. Thus, the sample means for the year 2008 are based on birth cohorts 1953–1983 and for the year 2011 on birth cohorts 1956–1986. The cross-sectional weights in the NEPS are calibrated to the Micro Census sample as of a respective calendar year.
Source: NEPS SC6:12.1.0. Micro Census 2008 and 2011.

Table A2: Balancing Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	**Compulsory Schooling**				**Enrollment cutoffs**			
	Dependent variable: Reform (0/1)			Bivariate Correlation	Dependent variable: After (0/1)			Bivariate Correlation
Female (0/1)	0.008	0.007	0.004	0.037	-0.007	-0.007	-0.007	-0.037
	(0.008)	(0.008)	(0.007)	(0.038)	(0.009)	(0.009)	(0.009)	(0.042)
Parental education (in yrs)	0.001	0.001	0.001	0.002	0.001	0.001	0.001	0.218
	(0.002)	(0.002)	(0.002)	(0.203)	(0.002)	(0.002)	(0.002)	(0.224)
Parental education: miss.	0.046	0.066	0.020	0.011	-0.035	-0.035	-0.038	-0.014
	(0.040)	(0.047)	(0.042)	(0.012)	(0.040)	(0.044)	(0.044)	(0.011)
Non-German parent (0/1)		0.023	0.047	0.010		-0.006	0.006	-0.002
		(0.040)	(0.034)	(0.014)		(0.036)	(0.036)	(0.011)
Non-German parent: miss.		0.020	0.069	-0.001		0.012	0.012	-0.003
		(0.095)	(0.083)	(0.004)		(0.098)	(0.099)	(0.005)
Maternal age at birth (in yrs)		-0.001	-0.001	0.216		0.000	0.000	0.389
		(0.001)	(0.001)	(0.662)		(0.001)	(0.001)	(0.505)
Maternal age at birth: miss.		-0.051	-0.036	-0.019		0.007	0.007	-0.009
		(0.032)	(0.029)	(0.016)		(0.032)	(0.032)	(0.017)
Firstborn (0/1)		-0.013	-0.013	0.042		-0.003	-0.003	0.012
		(0.009)	(0.008)	(0.038)		(0.009)	(0.009)	(0.042)
Firstborn: miss.		0.013	0.005	0.007		-0.034	-0.034	-0.034
		(0.013)	(0.014)	(0.012)		(0.028)	(0.028)	(0.029)
Kindergarten attendance (0/1)		-0.005	-0.008	-0.024		-0.007	-0.008	-0.035
		(0.008)	(0.008)	(0.034)		(0.009)	(0.009)	(0.042)
Kindergarten attendance: miss.		-0.035	-0.026	-0.008		-0.016	-0.016	-0.003
		(0.025)	(0.029)	(0.006)		(0.041)	(0.041)	(0.011)
Born in rural municipality (0/1)		0.006	0.004	0.028		0.013	0.013	0.061
		(0.009)	(0.009)	(0.035)		(0.009)	(0.009)	(0.040)
Short school yrs (0/1)			0.359***	0.451***			0.000	-0.006
			(0.017)	(0.018)			(0.010)	(0.039)
F-Statistic	0.723	0.786	42.900		0.882	0.626	0.581	
p-value	0.538	0.654	0.000		0.450	0.822	0.871	
Individuals	5,260				4,652			

Note: Sample restricted to individuals born in West Germany between 1945 and 1964. Data weighted using a cross-sectional weight calibrated to Micro Census 2011. The regressions in columns 1–4 include state and (monthly) birth date fixed effects. The regressions in columns 5–8 include linear trends in the running variable (week of birth) that are allowed to vary on both sides of the cutoff. Results in columns 4 and 8 come from separate regressions of the covariate, reported in each row, on the Reform or After dummy, respectively. Robust standard errors in parentheses. The F-Statistics and the p-value below are from tests of a joint significance of all covariates in a given column.
Source: NEPS SC6:12.1.0.

Table A3: Average Characteristics of Compliers

Sample	Compulsory Schooling			Enrollment cutoffs		
	Complier	**Non-Complier**	**Diff.**	**Complier**	**Non-Complier**	**Diff.**
Female (0/1)	0.485	0.493	-0.008	0.54	0.49	0.05
Year of Birth	1954.28	1955.55	-1.267	1955.71	1955.18	0.53
State: Schleswig-Holstein	0.043	0.055	-0.012	0.05	0.05	0.00
State: Hamburg	0.027	0.022	0.006	0.03	0.04	-0.01
State: Lower Saxony	0.132	0.097	0.035	0.13	0.14	-0.01
State: Bremen	0.010	0.030	-0.02	0.02	0.03	-0.01
State: North Rhine-Westphalia	0.301	0.298	0.103	0.28	0.28	-0.00
State: Hesse	0.056	0.095	-0.039	0.06	0.09	-0.03
State: Rhineland-Palatinate	0.074	0.094	-0.02	0.07	0.08	-0.01
State: Baden-Wuerttemberg	0.128	0.177	-0.05	0.14	0.16	-0.02
State: Bavaria	0.202	0.207	-0.005	0.20	0.12	0.08
State: Saarland	0.028	0.026	0.002	0.020	0.02	0.02
Parental education (in yrs)	9.910	9.972	-0.061	11.01	10.97	0.04
Parental education: miss.	0.030	0.048	-0.018	0.02	0.02	0.00
Non-German parent (0/1)	0.008	0.038	-0.029	0.98	0.98	0.00
Non-German parent: miss.	0.008	0.000	0.008	0.00	0.01	-0.01
Maternal age at birth (in yrs)	26.409	25.350	1.059	28.41	27.94	0.47
Maternal age at birth: miss.	0.066	0.084	-0.018	0.04	0.06	-0.02
Firstborn (0/1)	0.569	0.629	-0.061	0.52	0.51	0.01
Firstborn: miss.	0.052	0.033	0.019	0.06	0.05	0.01
Kindergarten attendance (0/1)	0.472	0.469	0.003	0.48	0.54	-0.06
Kindergarten attendance: miss.	0.013	0.027	-0.014	0.01	0.02	-0.01
Born in rural municipality (0/1)	0.406	0.406	0.000	0.36	0.36	0.00
Short school yrs (0/1)	0.278	0.476	-0.198	0.28	0.38	-0.10
No. Individuals	1,637	148		3,355	1,297	

Note: Sample restricted to individuals born in West Germany between 1945 and 1964. Data weighted using a cross-sectional weight calibrated to Micro Census 2011. All individuals who enter school in the year they are supposed to according to the school enrolment law are defined as compliers in the school starting age sample. For the compulsory schooling extension sample, compliers are defined as individuals that achieved only basic schooling degree, were affected by the reform and attended more than 8 years of schooling or individuals that achieved only basic schooling degree, were not yet affected by the reform and attended more than 7 years of schooling.
Source: NEPS SC6:12.1.

Table A4: Effects of Being Born After the Cutoff on Cognitive Skills and Risk Affinity

	(1) Reading Competency	(2) Reading Speed	(3) Self-Assess. Reading	(4) Math Competency	(5) Self-Assess. Math	(6) Risk Affinity
Panel A: DD Estimate of the Effect on Cognitive Skills						
Reform	0.252***	0.167*	0.102	0.070	0.024	-0.037
	(0.085)	(0.092)	(0.097)	(0.119)	(0.130)	(0.084)
Y-Mean	0.000	0.000	0.000	0.000	0.000	0.000
Obs./Indiv.	3,411	3,685	3,323	2,260	2,232	3,827
Panel B: DD Estimate of the Effect on the Probability of a Missing Outcome						
Reform	0.013	0.020	0.013	-0.029	-0.031	0.023
	(0.038)	(0.037)	(0.038)	(0.034)	(0.034)	(0.040)
Y-Mean	0.427	0.382	0.445	0.682	0.688	0.320
Obs./Indiv.	5,260	5,260	5,260	5,260	5,260	5,260

Note: Sample restricted to individuals born in West Germany between 1945 and 1964. All outcomes are standardized. Each cell is based on a separate linear regression of Equation (1) using a cross-sectional weight calibrated to Micro Census 2011. All regressions include state and birth date fixed effects. Robust standard errors in parentheses.
Source: NEPS SC6:12.1.0.

Table A5: Effects of Being Born After the Cutoff on Cognitive Skills and Risk Affinity

	(1) Reading Competency	(2) Reading Speed	(3) Self-Assess. Reading	(4) Math Competency	(5) Self-Assess. Math	(6) Risk Affinity
Panel A: RDD Estimate of the Effect on Cognitive Skills						
Reform	-0.042	-0.061	-0.027	0.046	0.052	-0.241
	(0.097)	(0.101)	(0.100)	(0.126)	(0.132)	(0.101)
Y-Mean	0.000	0.000	0.000	0.000	0.000	0.000
Obs./Indiv.	3,020	3,263	2,984	1,975	1,952	3,353
Panel B: RDD Estimate of the Effect on the Probability of a Missing Outcome						
Reform	-0.055	-0.063	-0.066	-0.026	-0.028	-0.042
	(0.043)	(0.043)	(0.042)	(0.036)	(0.036)	(0.040)
Y-Mean	0.425	0.379	0.442	0.685	0.690	0.295
Obs./Indiv.	4,652	4,652	4,652	4,652	4,652	4,652

Note: Sample restricted to individuals born in West Germany between 1945 and 1964. All outcomes are standardized. Each cell is based on a separate linear regression using a cross-sectional weight calibrated to Micro Census 2011. All regressions include linear trends in the running variable (week of birth) that are allowed to vary on both sides of the cutoff. Robust standard errors in parentheses.
Source: NEPS SC6:12.1.0.

Table A6: Long-Run Effects of Compulsory Schooling on Educational Attainment

	(1) Basic Degree	(2) Middle Degree	(3) High School	(4) College/Univ. Degree	(5) Vocational Education
Panel A: DD Regressions Without Controls					
Reform	-0.029	0.027	0.002	-0.019	0.013
	(0.037)	(0.035)	(0.025)	(0.022)	(0.034)
Panel B: DD Regressions With Controls					
Reform	-0.023	0.031	-0.008	-0.027	0.015
	(0.038)	(0.038)	(0.027)	(0.024)	(0.037)
Y-Mean	0.465	0.309	0.226	0.148	0.719
Obs./Indiv.			5,529		

Note: Sample restricted to individuals born in West Germany between 1945 and 1964. The outcomes are measured at the individual level. Each cell is based on a separate linear regression of Equation (1) using a cross-sectional weight calibrated to Micro Census 2011. All regressions include state and birth date fixed effects. Controls comprise gender, parental education and citizenship, maternal age at birth, an individual's birth order, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Robust standard errors in parentheses.
Source: NEPS SC6:12.1.0.

Table A7: Long-Run Effects of Being Born After the Cutoff on Educational Attainment

	(1) Basic Degree	(2) Middle Degree	(3) High School	(4) College/Univ. Degree	(5) Vocational Education
Panel A: RDD Regressions Without Controls					
After	-0.043	0.016	0.027	0.013	-0.024
	(0.042)	(0.038)	(0.030)	(0.023)	(0.037)
Panel B: RDD Regressions With Controls					
After	-0.038	0.019	0.018	0.006	-0.026
	(0.039)	(0.037)	(0.028)	(0.023)	(0.037)
Y-Mean	0.469	0.308	0.223	0.145	0.723
Obs./Indiv.			4,652		

Note: Sample restricted to individuals born in West Germany between 1945 and 1964. The outcomes are measured at the individual level. Each cell is based on a separate linear regression of Equation (2) using a cross-sectional weight calibrated to Micro Census 2011. All regressions include linear trends in the running variable (week of birth) that are allowed to vary on both sides of the cutoff. Controls comprise gender, parental education and citizenship, maternal age at birth, an individual's birth order, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Robust standard errors in parentheses.
Source: NEPS SC6:12.1.0.

Table A8: Robustness Analysis - Compulsory Schooling Reform

	Cross-State Mobility			**Cross-County Mobility**		
	1-Year	**5-Year**	**Lifetime**	**1-Year**	**5-Year**	**Lifetime**
Baseline (Obs. 159,716/5,260)	0.000	-0.002	0.020	0.002	0.001	0.005
	(0.002)	(0.008)	(0.029)	(0.003)	(0.012)	(0.033)
A: Incl. student-teacher-ratio	-0.000	-0.003	0.021	0.002	0.002	0.005
(Obs. 159,716/5,260)	(0.002)	(0.008)	(0.029)	(0.003)	(0.012)	(0.034)
B: Incl. year of birth x state FE	0.001	0.004	-0.007	0.000	-0.008	-0.025
(Obs. 159,716/5,260)	(0.002)	(0.007)	(0.027)	(0.003)	(0.012)	(0.034)
C: Always-treated states excluded	0.002	0.008	0.027	0.005	0.012	-0.000
(Obs. 143,042/4,711)	(0.003)	(0.009)	(0.034)	(0.004)	(0.013)	(0.038)
D: Extended TWFE estimator (ETWFE)	0.003	0.004	-0.039	-0.002	-0.007	-0.023
(Obs. 52,442)	(0.002)	(0.010)	(0.041)	(0.005)	(0.017)	(0.049)
E: Earlier treatment assignment (age 12)	-0.000	-0.003	0.015	0.002	0.003	0.005
(Obs. 159,596/5,256)	(0.002)	(0.007)	(0.029)	(0.003)	(0.012)	(0.033)
F: Unweighted regressions	-0.000	0.001	0.002	0.001	0.004	0.005
(Obs. 159,716/5,260)	(0.002)	(0.007)	(0.025)	(0.003)	(0.011)	(0.028)
G: Placebo reform	-0.001	-0.005	-0.012	0.000	0.003	-0.029
(Obs. 159,716/5,260)	(0.002)	(0.006)	(0.023)	(0.003)	(0.010)	(0.027)

Note: Sample restricted to individuals born in West Germany between 1945 and 1964. The outcomes are measured at ages from 25 through 55. Each cell is based on a separate linear regression of Equation (1) using a cross-sectional weight calibrated to Micro Census 2011. All regressions include state and birth date fixed effects. Controls comprise gender, parental education and citizenship, maternal age at birth, an individual's birth order, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. The standard errors in parentheses are clustered at the individual level, except in Panel D, where we use the ETWFE by Wooldridge (2021) and the default clustering at the state level. The ETWFE is applied to data aggregated into state $\times$ cohort cells and weighted by the number of observations in each cell. The placebo reform in Panel G is based on randomly assigned reform dates across states.
Source: NEPS SC6:12.1.0.

Table A9: Robustness Analysis - Being Born After the Cutoff

	Cross-State Mobility			Cross-County Mobility		
	1-Year	**5-Year**	**Lifetime**	**1-Year**	**5-Year**	**Lifetime**
Baseline (Obs. 140,414/4,652)	0.002	0.002	-0.023	-0.001	-0.014	-0.031
	(0.002)	(0.002)	(0.032)	(0.004)	(0.013)	(0.038)
A: Incl. student-teacher-ratio	0.002	0.002	-0.022	-0.001	-0.014	-0.031
(Obs. 140,414/4,652)	(0.002)	(0.008)	(0.032)	(0.004)	(0.013)	(0.038)
B: Incl. cutoff-month FE	0.002	0.001	-0.025	-0.001	-0.014	-0.032
(Obs. 140,414/4,652)	(0.002)	(0.008)	(0.032)	(0.004)	(0.013)	(0.038)
C: Incl. quadratic trends	0.001	-0.002	-0.067	-0.004	-0.022	-0.078
(Obs. 140,414/4,652)	(0.004)	(0.015)	(0.056)	(0.007)	(0.023)	(0.070)
D: Incl. quadratic trends & controls	-0.000	-0.005	-0.067	-0.005	-0.021	-0.056
(Obs. 140,414/4,652)	(0.004)	(0.014)	(0.054)	(0.006)	(0.022)	(0.069)
E: Non-parametric approach	0.001	-0.000	-0.059	-0.004	-0.022	-0.067
(Obs. 140,414/4,652)	(0.004)	(0.008)	(0.056)	(0.007)	(0.023)	(0.070)
F: Limited bandwidths (20 weeks)	0.001	-0.001	-0.027	-0.003	-0.021	-0.044
(Obs. 115,137/3,818)	(0.003)	(0.009)	(0.036)	(0.004)	(0.015)	(0.043)
G: Incl. donut-hole	0.000	-0.000	-0.023	-0.002	-0.016	-0.037
(Obs. 152,347/5,045)	(0.002)	(0.007)	(0.027)	(0.003)	(0.011)	(0.033)
H: Unweighted regressions	0.003	0.007	-0.005	0.000	-0.006	-0.012
(Obs. 140,414/4,652)	(0.002)	(0.008)	(0.025)	(0.003)	(0.011)	(0.029)
I: Placebo cutoff	0.002	0.001	-0.010	0.001	-0.013	-0.032
(Obs. 140,414/4,652)	(0.002)	(0.009)	(0.034)	(0.004)	(0.014)	(0.040)

Note: Sample restricted to individuals born in West Germany between 1945 and 1964. The outcomes are measured at ages from 25 through 55. Each cell is based on a separate linear regression of Equation (2) using a cross-sectional weight calibrated to Micro Census 2011. All regressions include linear trends in the running variable (week of birth) that are allowed to vary on both sides of the cutoff. Controls comprise gender, parental education and citizenship, maternal age at birth, an individual's birth order, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. For the non-parametric approach in Panel E, we use the robust bias-corrected estimator proposed by Calonico et al. (2020b).Panel G limits the bandwidths to 20 weeks on either side of the cutoff. The placebo cutoff in Panel I implies a shift of the actual cutoff by 6 months to the left. Standard errors in parentheses are clustered at the individual level.
Source: NEPS SC6:12.1.