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DISCUSSION PAPER SERIES

IZA DP No. 17948

The Untold Story of Internal Migration in Germany: Life-Cycle Patterns, Developments, and the Role of Education

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ABSTRACT

The Untold Story of Internal Migration in Germany: Life-Cycle Patterns, Developments, and the Role of Education

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 a regression discontinuity design, we find no effect of these policies on internal mobility.

JEL Classification: I26, J61, R23

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, Card et al., 2025), 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 (Jayachandran et al., 2024) 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, presumably also because internal migration is fundamental to the American narrative of "moving to opportunity".[2] 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 about an individual's place of birth,[3] the U.S. decennial censuses began collecting this information in 1935, facilitating research on internal migration patterns (Zimran, 2024).

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, 2018, 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). 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 (e.g., Molloy et al., 2017, Jia et al., 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 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

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 detailed biographical information on residential moves over the life cycle. Specifically, for a representative sample of nearly 13,000 individuals born in Germany between 1944 and 1986, we construct a panel dataset that tracks their geographic mobility at monthly intervals starting from birth until 2020 (i.e., at least until age 34, 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 county) and study them from a life-cycle perspective.

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).

[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), regional characteristics (Haussen and Uebelmesser, 2018a,b) and economic shocks (Jauer et al., 2019).

Importantly, the combination of retrospective details on regional mobility and educational pathways and the availability of fine-grained information on birth dates allows us to exploit institutional aspects of the German school system to investigate the causal effect of education on mobility.

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 (17) 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, internal migration is still quite common from a lifetime perspective; starting from the age of 29 (23), more than a quarter (half) of individuals live in a state (county) other than their birthplace. For comparison, the percentage of individuals residing outside their birth state during prime ages is 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. These differences persist when we condition on parental background.

We then turn to the question of whether there is a causal component in the substantial 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., 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). 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]

[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 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

Using a difference-in-differences and regression discontinuity design, we find no effect of these policy-induced sources of variation in education on internal mobility. The results are remarkably stable over the life cycle and robust to alternative model specifications, sample restrictions, and estimation procedures. In terms of potential mechanisms, we show that the compulsory schooling reform improved reading skills and that the school entry cutoffs affected risk attitudes. However, although the cognitive ability to appropriately assess local disequilibria and the willingness to take the risk of relocation are considered important determinants of mobility decisions (e.g., Sjaastad, 1962, Schultz, 1975a, Jaeger et al., 2010, Dustmann et al., 2023), neither of these channels seems crucial to induce significant mobility changes in our setting. Instead, we provide suggestive evidence that the lack of significant mobility effects is most likely due to insufficient effects on academic qualifications, which in Germany play a crucial role in certifying an individual's knowledge and skills acquired through education.

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 (e.g., Long, 1991, Molloy et al., 2011, Bernard et al., 2014, Champion et al., 2017, Jia et al., 2023, Zimran, 2024). 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 role of education as an individual-level determinant of regional mobility. We build on earlier research using plausibly exogenous sources of variation in education to estimate its impact on internal migration (e.g., Machin et al., 2012, Weiss, 2015, McHenry, 2013, Malamud and Wozniak, 2010). So far, the findings are inconclusive and suggest that both the country-specific context and the margin of educational distribution might be important.[9] We

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] 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. Similarly, Aparicio Fenoll and Kuehn (2017) shows that extended compulsory

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

Finally, this paper complements the extensive research on German data that faces the challenge of measurement error in treatment assignment due to the lack of retrospective regional information in the data. In particular, when evaluating the medium- and long-term effects of past treatments with geographic variation, researchers often lack information on the location of individuals at the time of treatment and use their current location as a proxy. This issue inherently leads to the assumption of negligible internal mobility over the life cycle in Germany. This often applies, but is not limited, to studies that examine how adult outcomes are affected by certain childhood or adolescent experiences such as exposure to war, the socialist GDR regime, food shortages, or school-time interventions.[10] We contribute to the literature by providing first evidence on the magnitude of measurement error from a life-cycle perspective. Our results suggest that the measurement error due to regional mobility may be substantial. We also point to the NEPS as a useful source of information that can facilitate addressing some methodological challenges associated with this error in future research.[11]

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

schooling reduce regional mobility using cross-country data from Europe, but their focus was on the effects of emigration to another country. For the US, Malamud and Wozniak (2010) 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).

[10] See, e.g., Pischke and von Wachter (2008), Kemptner et al. (2011), Riphahn (2012), Cygan-Rehm and Maeder (2013), Jürges (2013), Akbulut-Yuksel (2014), Fuchs-Schündeln and Masella (2016), 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 (forthcoming), Görlitz et al. (2025).

[11] If the measurement error due to regional mobility correlates with the treatment, the estimated effects of the treatment are biased. For the compulsory schooling reform and the school entry cutoffs, we find no significant effect of these treatments on regional mobility. This suggests that if anything, previous studies evaluating the long-run effects of these policies suffer from an attenuation bias. It is beyond our scope to reevaluate previous conclusions or to examine endogenous mobility in relation to other treatments.

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 (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 12,618 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).[12] Nevertheless, retrospective biographical information on early childhood might suffer from a substantial measurement error due to limited recall.

[12] 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.

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 respondents throughout their lives, including the school starting age, the type of secondary school track attended, the highest school degree completed (basic, middle, or high school), and postsecondary education, including college or university graduation. This makes NEPS 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 12,618 individuals, whom we follow over the life cycle starting from birth. Because we observe mobility outcomes beyond age 64 only for a few birth cohorts, we restrict the main sample to ages between 0 and 64. To facilitate computation, we aggregate the panel of approximately 9 million monthly spells into a person-age year panel of nearly 800 thousand 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 German states.

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 still be considered as a sufficiently distant move to make a meaningful difference in local labor market environments and living conditions. Nevertheless, we also test whether our main conclusions hold when we use alternative geographic units such as labor markets and metropolitan areas.

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. Table 1 provides the summary statistics.

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 nearly 2 percent of 7-year-olds moved to another state, and 4.5 percent to another county within the last year. This coincides with the typical school start age. 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 17 percent, respectively. Afterward, the annual mobility rates decline continuously with age until the early fifties. Less than 1.5 (3.3) 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 (12) 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 (see Section 4.1). 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 county fixed effects and 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).[13] 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 enrollment at the beginning of the next school year; children who turn six years of age after the cutoff are 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,[14] 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 enrollment figures. Nevertheless, the NEPS data suggest that, despite this exception, the average compliance is about 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 typically 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 enrollment, children commonly undergo a four-year education in primary

[13] 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).

[14] Many states have explicitly defined exception rules for earlier enrollment. 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.

school.[15] Subsequently (i.e., around the age of 10), based on their academic record, students receive a referral to a particular type of secondary school.[16] Historically and still today, secondary education in Germany distinguishes between the basic track (Hauptschule), intermediate track (Realschule), and high schools (Gymnasium).[17] 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 apprenticeships 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.[18]

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).[19] The primary rationale for these extensions was to en-

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

[16] 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 subjectivity 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 Lüdemann and Schwerdt (2013).

[17] 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.

[18] 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.

[19] 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 (forthcoming), 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.

hance 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, forthcoming). The former 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 simultaneously 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.[20]

Second, we build on established literature showing that the statutory cutoff rules for school enrollment have important consequences for secondary school track placement, which is particularly pronounced and persistent in selective systems featuring early ability tracking.[21] Specifically, being born after the cutoff increases the probability of attending high-ability tracks, which provide eligibility for college education. This

[20] 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.

[21] 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 Öckert (2014) for Sweden; and Oosterbeek et al. (2021) for the Netherlands.

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. 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 year of birth fixed effects. 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'Haultfœuille, 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 Y_i^a is an outcome of individual i 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,[22] 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).[23] 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

[22] Earlier research has documented significant impacts on the entire family (Landersö 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., Landersö 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 Öckert, 2014, Larsen and Solli, 2017).

[23] 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.

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),[24] 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., individuals 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 enrollment 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 enrollment). Therefore, there is a slight difference in the size of the two estimation samples. Nonetheless, the sample means presented in columns 3 and 4 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

[24] The density test yields a p-value of 0.5154. This result does not allow us to reject the hypothesis of a smooth distribution at the conventional significance levels.

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 with 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. 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 additionally control for individual-level characteristics. Column 2 shows a 40-percentage point increase in the probability of being relatively old for grade[25] for children born after the cutoff. 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 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 across the state and county borders measured in adulthood. We begin by 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

[25] Being old for grade is an alternative measure commonly used in recent literature on school starting age (e.g., Landersö 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.

from effect heterogeneity (Wooldridge, 2021). We also show that our main conclusions hold when we use alternative definitions of geographic units, such as labor markets and metropolitan areas, instead of states and counties.

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., 2020b). 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. In Table 4 and Table 5, we focus on effects averaged over the prime working ages (25 to 55). Nevertheless, 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 that is half the size (when compared to the sample mean) 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 1900 and 1964. Again, the corresponding 2SLS estimate from our results implies an effect size half the size (when compared to the sample mean). Thus, the effects for Germany are 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

Since at least Sjaastad (1962), economists have viewed migration as an investment decision, similar to schooling. According to this concept, education can affect individual location choices through several channels. First, education may enhance individuals' ability and/ or willingness to react to disequilibria (Schultz, 1975b), such that they migrate in response to regional differences, e.g., in wages or employment opportunities. This assumes that education increases the individuals' ability to acquire and interpret information accurately, which requires education to improve cognitive abilities. However, this also implies that individuals are willing to take actions that lead to appropriate relocation. Thus, education may also affect mobility through potential effects on risk preferences.[26] Finally, education may affect migration behavior if local labor markets for higher-educated workers become relatively thin. This mechanism requires that education affects educational credentials that are transferable across regions.

We begin with evidence on the skills channel and risk attitudes. The NEPS includes measures of basic linguistic and mathematics competencies in selected waves starting in 2010/11.[27] The reading test consists of five texts and a series of questions related to one of the texts that assess cognitive abilities in retrieving information, drawing text-related inferences, reflection and evaluation. In the listening (or oral language) comprehension test, participants select the correct picture from a set of four pictures for each word presented. This is designed to assess receptive vocabulary, which has been shown to reflect both crystallized intelligence and language ability. In the math test, individuals are challenged by a specific life situation followed by related task(s) assessing the cognitive processes in the areas of quantity, space and shape, change and

[26] Consistent with this argument, previous literature documents that less risk-averse individuals are more likely to migrate (e.g., Jaeger et al., 2010, Roca Paz and Uebelmesser, 2021, Dustmann et al., 2023).

[27] In 2010/11, reading and mathematics skills were assessed for a randomly selected 50% subsample of respondents. The remaining 50% subsample participated in the assessments later (in 2012/13 or 2016/17 for reading and 2016/17 for mathematics). In 2014/15, a listening comprehension test was conducted to measure the receptive vocabulary at the word level. In 2012/13, NEPS also collected data on scientific literacy and ICT literacy for a subsample of respondents, but the available sample sizes are very small and we do not use these assessments. For details, see Fuß et al. (2021).

functional relationships, data and chance (for details, see Weinert et al., 2011). Information on individuals' self-rated risk attitudes was collected on an 11-point Likert scale for five survey years between 2014 and 2020.

The estimated effects of the compulsory schooling reform are reported in Appendix Table A4. The outcomes in Panel A are standardized within each sample. The sample sizes vary depending on data availability. For each individual, we use the first available outcome measurement in the panel. We find that the reform significantly increased the reading competency by about 0.25 standard deviation (SD) and reading speed by 0.17 SD.[28] The effects are substantial compared to an average learning gain over a school year of about a quarter to a third of an SD (Werner and Woessmann, 2023). However, we do not find any statistically significant effects on listening comprehension and math competency. The last column also suggests no effects on risk attitudes. In Panel B, we test for potentially endogenous selection into the samples, which could bias our estimates in Panel A, but do not find any strong evidence that the availability of the outcomes is correlated with the reform status.

For the school entry cutoffs (see Appendix Table A5), we find no statistically significant effects on language and math skills, but the positive point estimate for listening comprehension is relatively large in magnitude. The latest is consistent with Görlitz et al. (2022), who use all NEPS cohorts 1944-1986 to examine the effects of school starting age on cognitive skills.[29] However, individuals born after the cutoff have significantly lower levels of risk affinity. This may explain to some extent why most of the (statistically insignificant) effects on mobility in Table 5 are negative, as higher risk affinity is typically associated with a higher propensity to migrate (e.g., Jaeger et al., 2010, Roca Paz and Uebelmesser, 2021, Dustmann et al., 2023).

Finally, we examine the role of academic credentials (see Appendix Table A6 and Table A7). Generally, for both compulsory schooling reform and school entry cutoffs, the point estimates in columns 1 to 3 suggest a shift away from the basic school degree towards the completion of better school credentials. However, the estimates lack precision. In addition, the last columns show no significant effect on the completion of college education and vocational training. Thus, we do not find strong evidence that longer compulsory schooling and the initial advantages of being born after the cutoff

[28] Using data from the German Socio-Economic Panel, Kamhöfer and Schmitz (2016) found no statistically significant effect of this reform on performance on an ultrashort word fluency test that required respondents to name as many animals as possible in 90 seconds. The NEPS arguably provides a more detailed conceptual framework for measuring verbal cognition (Weinert et al., 2011).

[29] Görlitz et al. (2022) estimate instrumental variable (IV) regressions and find that a one-year increase in school starting age significantly increases listening comprehension by 0.35 SD. Our (insignificant) ITT effect of being born after the cutoff of 0.141 corresponds to a nearly identical IV estimate when divided by the first-stage effect of the cutoffs on school starting age of 0.398 (see column 1, Table 3).

for secondary school track placement translate into better academic credentials. These results are broadly consistent with previous studies (e.g., Pischke and von Wachter, 2008, Cygan-Rehm, 2022, Dustmann et al., 2017).

Taken together, we find positive effects of extended compulsory schooling on reading skills and a negative effect of the school entry cutoffs on risk affinity. However, despite these, we find no significant changes in invividuals' mobility decisions. Thus, these channels seem to be of limited importance for the effects of improved education on regional mobility in Germany. Instead, we argue that the lack of significant mobility effects is most likely due to the insufficient impacts on academic qualifications. This channel may be particularly important in countries such as Germany, where secondary school degrees and postsecondary diplomas play a crucial role in certifying a person's knowledge and skills acquired through education.[30]

5.4 Sensitivity analysis

This section examines the robustness of our findings across alternative model specifications and data choices. The results for the effects of the compulsory schooling reform and the school enrollment cutoffs are presented in Appendix Table A8 and Table 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 changes over time that differed across the states such as the increased supply of secondary schools or universities (e.g., Kamhöfer et al., 2019, Boelmann, 2024, Hertweck and Yasar, 2024). 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

[30] Grenet (2013) makes a similar argument to justify the heterogeneous wage returns to similar compulsory schooling in France and England. He argues that in the European context, the actual quantity of education may be less important than credentials in determining the returns to schooling, i.e, despite effects on cognition, better education can only effectively affect wages if it leads to a significant improvement in academic or vocational credentials.

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 an individual's 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.[31] The alternative estimation procedure supports our main conclusion, although it typically suggests using narrower bandwidths of only about 20 weeks around the cutoff. 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) despite their lower compliance with the 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

[31] We use the robust bias-corrected estimator proposed by Calonico et al. (2020a), 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. 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.

cutoffs" (Panel I). We do this by shifting the actual cutoff date six months to the right. As expected, we find no significant results in this falsification test, supporting the validity of our main estimates.

Finally, we test whether our main conclusions hold when we use alternative definitions of mobility outcomes and geographic units (see Appendix Table A10 to Table A13). For the outcomes, we now count the number of residential cross-state and cross-county moves between ages 25 and 55. In terms of the geographic units, instead of using administrative boundaries between states and counties, we use labor markets and metropolitan areas. The latter may arguably more accurately reflect substantial differences in local residential and economic environments.[32] Similar to our main results, the alternative estimates do not yield any statistically significant effects.

6 Conclusions

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

We begin by documenting some fundamental facts about the extent of internal migration in Germany over 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. In particular, 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 exploit two arguably exogenous sources of variation in education to address the question of whether there is a causal relationship between education and mobility. Specifically, we use a compulsory schooling reform that aimed at increasing educational attainment at the bottom of the ability distribution (e.g., Pischke and von

[32] The common definition of labor markets in Germany corresponds to commuting areas with a daily commuting time of no more than 45 minutes one way (Kropp and Schwengler, 2011). In contrast, the classification of metropolitan areas takes into account common regional characteristics in the areas of politics, economy, science, transportation, and culture (BBSR, 2010). There are 223 labor markets and 15 metropolitan areas.

Wachter, 2008) and the statutory cutoff rules for school entry, which have been shown to increase the probability of attending the highest ability track in secondary school (e.g., Dustmann et al., 2017).

Using the difference-in-differences and regression discontinuity designs, we estimate the effects of education on geographic mobility at different margins of the ability distribution, for the same generation, and within the same context. We find no statistically significant effect of these policy-induced sources of variation on internal mobility. Despite some statistical power issues in our analysis of the compulsory schooling expansion, our point estimates suggest much smaller effect magnitudes for Germany than the positive effects found for similar birth cohorts in Norwegian data (Machin et al., 2012) and the negative effects documented for the US (McHenry, 2013). These countries are considered to have moderate to high migration rates (e.g., Bell et al., 2015). Our results are robust and remarkably stable over the life cycle.

Regarding the potential mechanisms for our findings, we show that the German compulsory schooling reform improved reading skills and that individuals born after the school entry cutoff have lower levels of risk affinity. However, although cognitive abilities to appropriately assess local disequilibria and the willingness to take the risk of a relocation are considered important determinants of mobility decisions (e.g., Sjaastad, 1962, Schultz, 1975a, Jaeger et al., 2010, Dustmann et al., 2023), neither of these channels induces significant mobility changes in Germany. Instead, we argue that the lack of significant mobility effects is most likely due to the insufficient impacts on academic qualifications, which in Germany play a crucial role in certifying a person's knowledge and skills acquired through education.

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

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

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Data statement

This paper uses proprietary data from the National Educational Panel Study (NEPS): Starting Cohort 6 – Adults (doi:10.5157/NEPS:SC6:13.0.0) that cannot be published. However, the data can be requested (e.g., for replication purposes) and analyzed via remote access to the Research Data Centers (FDZ) at the Leibniz Institute for Educational Trajectories (FDZ-LIfBi). From 2008 to 2013, NEPS data were collected as part of the Framework Programme for the Promotion of Empirical Educational Research funded by the German Federal Ministry of Education and Research (BMBF). As of 2014, the NEPS survey is carried out by the Leibniz Institute for Educational Trajectories (LIfBi) at the University of Bamberg in cooperation with a nationwide network. For table A1, we also use proprietary data from the Micro Census 2008 (doi:10.21242/12211.2008.00.00.1.1.0) and 2011 (doi:10.21242/12211.2011.00.00.1.1.0), which can be accessed on-site by signing a data usage agreement with the FDZ of the Statistical Offices of the Federation and the Federal States (DESTATIS).

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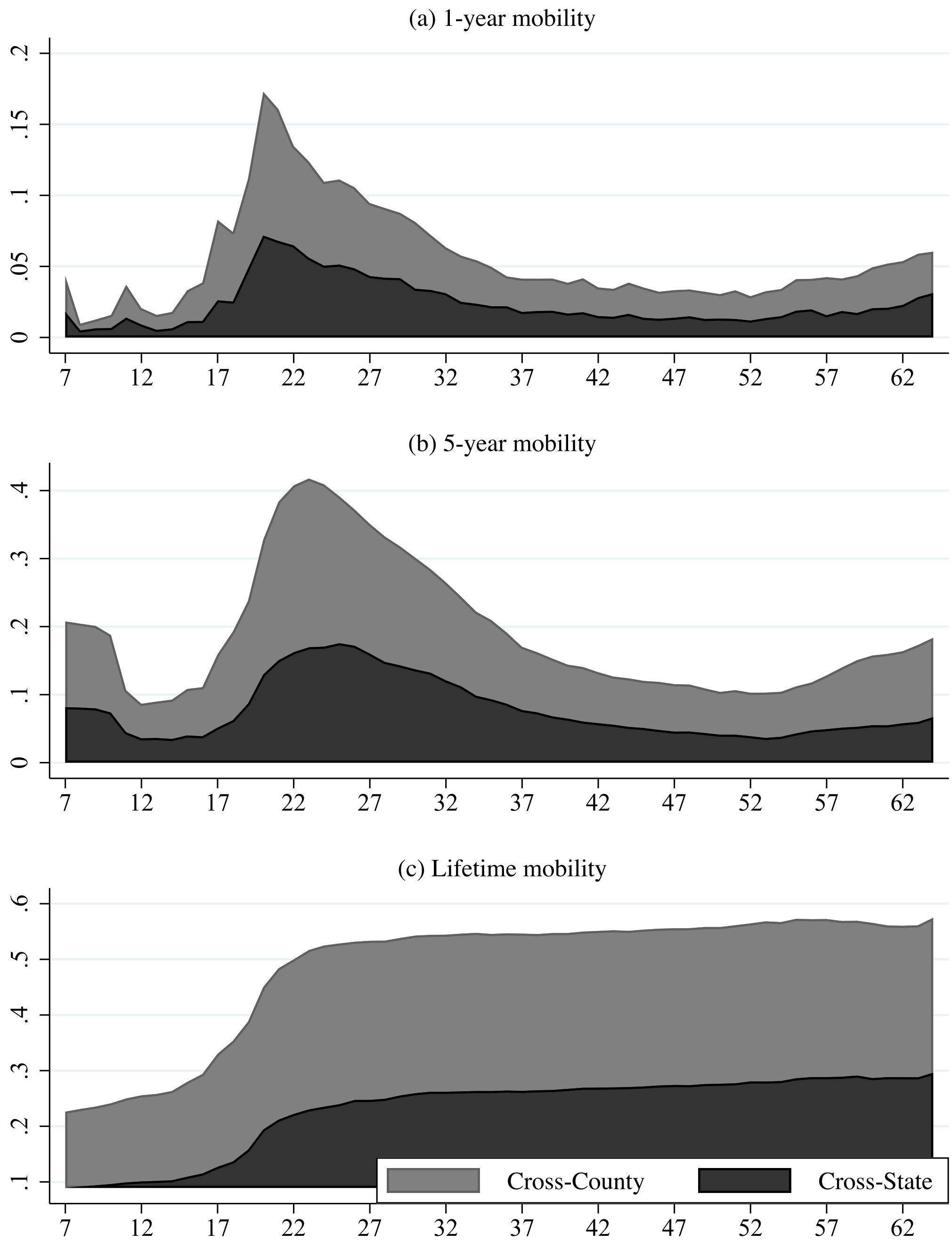
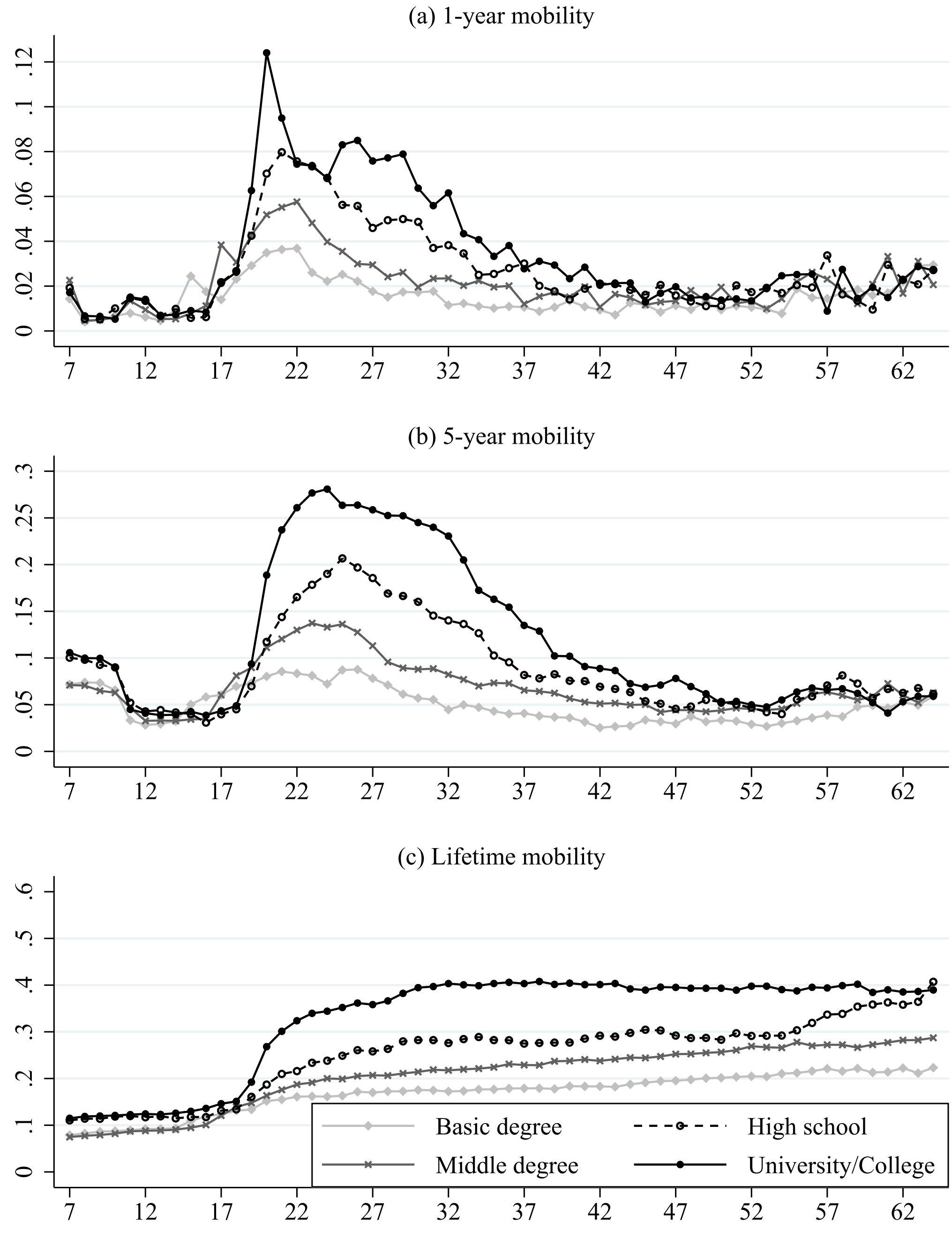
Figure 1: Age-specific cross-state and cross-county mobility rates



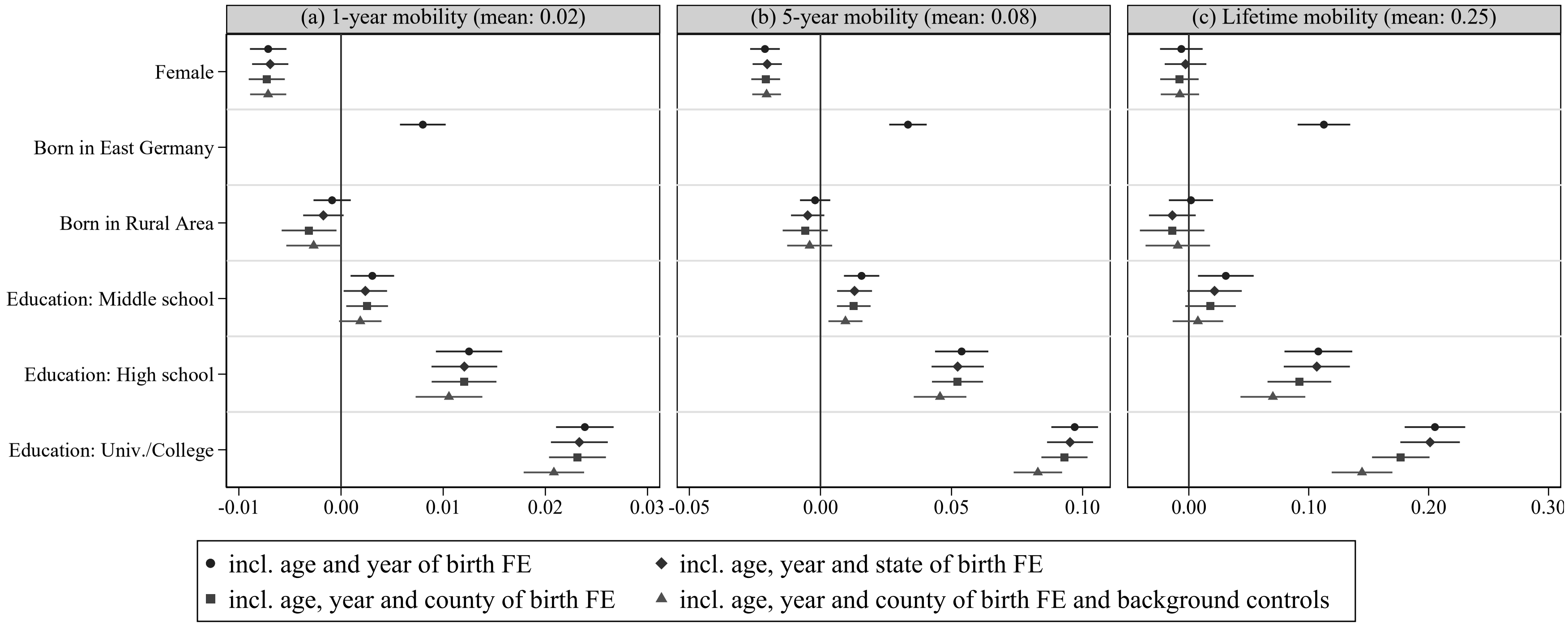
Note: Sample restricted to individuals born in Germany. Data weighted using a cross-sectional weight calibrated to Micro Census 2011.
Source: NEPS SC6:13.0.0.

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



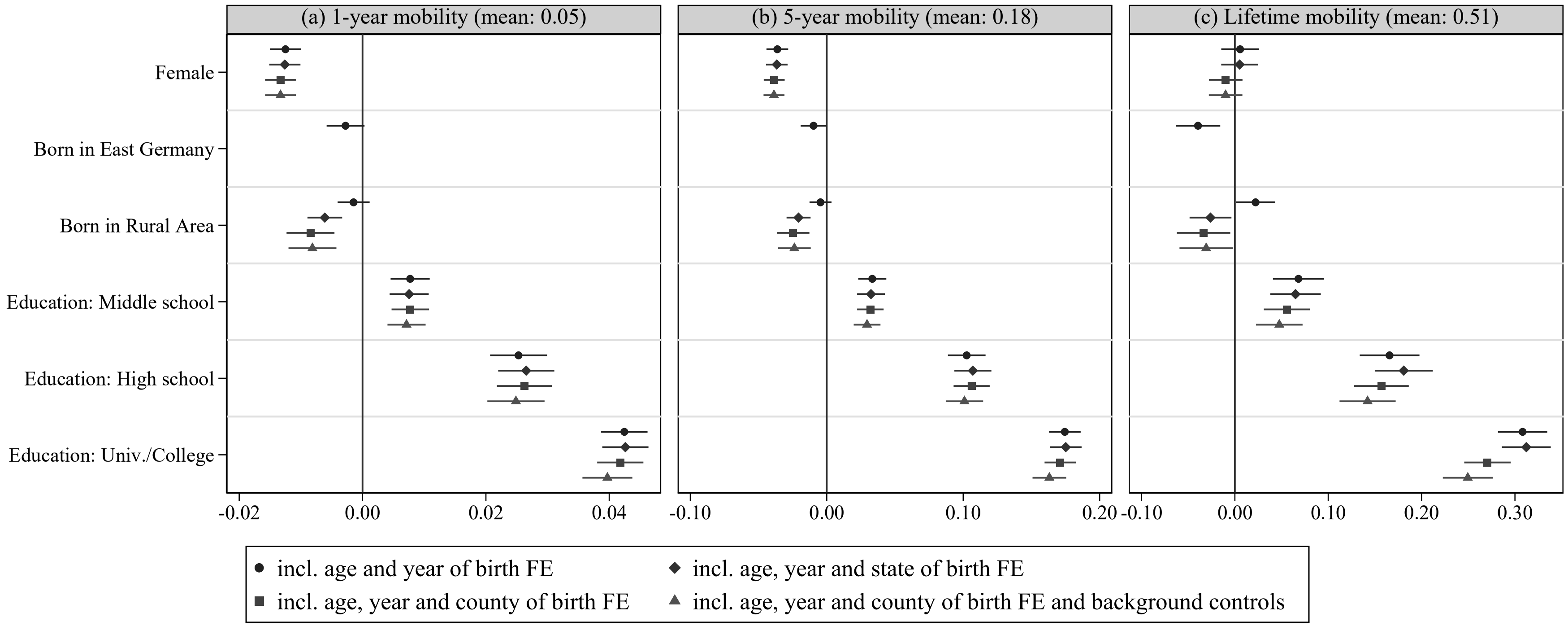
Note: Sample restricted to individuals born in Germany. Data weighted using a cross-sectional weight calibrated to Micro Census 2011.
Source: NEPS SC6:13.0.0.

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, rural place of birth, 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 12,618 individuals. The 95% confidence intervals are based on standard errors clustered at the individual level.
Source: NEPS SC6:13.0.0.

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, rural place of birth, 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 12,618 individuals. The 95% confidence intervals are based on standard errors clustered at the individual level. *Source:* NEPS SC6:13.0.0.

Table 1: Sample Means

	(1) Full sample, cohorts 1944-1986	(2) West Germany, cohorts 1945-1964	(3) Compulsory schooling sample	(4) Enrollment cutoffs sample
Migration measures (individual-level means across ages 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	1955.66	1955.67	1955.55
Month of birth	6.40	6.47	6.47	6.44
Female	0.50	0.52	0.52	0.52
Born in East-Germany	0.74	0.00	0.00	0.00
Born in rural municipality	0.36	0.36	0.36	0.36
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	0.15	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	11.01	11.02	11.00
Parental education: missing	0.02	0.02	0.02	0.02
German parents	0.97	0.98	0.98	0.98
German parents: missing	0.01	0.01	0.01	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	0.43	0.42	0.42	0.42
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
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	0.18	0.15	0.15	0.15
Individuals	12,618	5,295	5,260	4,652

Note: Sample restricted to individuals born in Germany. Mobility outcomes refer to individual-specific means calculated over ages 25-55. Data weighted using a cross-sectional weight calibrated to Micro Census 2011.
Source: NEPS SC6:13.0.0.

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

	(1) Duration of schooling (calend. time)	(2) Years of schooling (in grades)	(3) School starting age (placebo)
Panel A: DD regressions without controls			
Reform	0.590***	0.576***	-0.188
	(0.137)	(0.115)	(0.173)
Panel B: DD regressions with controls			
Reform	0.701***	0.551***	-0.118
	(0.149)	(0.117)	(0.163)
Y-Mean	9.704	10.150	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, rural place of birth, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Robust standard errors in parentheses.
Source: NEPS SC6:13.0.0.

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

	(1) School starting age (in years)	(2) Old for grade	(3) Acad. track attendance
Panel A: RDD regressions without controls			
After	0.398***	0.398***	0.059*
	(0.054)	(0.039)	(0.031)
Panel B: RDD regressions with controls			
After	0.400***	0.394***	0.057**
	(0.052)	(0.038)	(0.028)
Y-Mean	6.417	0.415	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, state fixed effects, birth cohort fixed effects, parental education, and citizenship, maternal age at birth, an individual's birth order, rural place of birth, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Robust standard errors in parentheses.
Source: NEPS SC6:13.0.0.

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, rural place of birth, 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 SC6:13.0.0.

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, state fixed effects, birth cohort fixed effects, parental education and citizenship, maternal age at birth, an individual's birth order, rural place of birth, 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 SC6:13.0.0

The Untold Story of Internal Migration in Germany: Life-cycle Patterns, Developments, and the Role of Education

– Online Appendix (Not for Publication) –

Anton Barabasch, Kamila Cygan-Rehm*, Guido Heineck, & Sebastian Vogler

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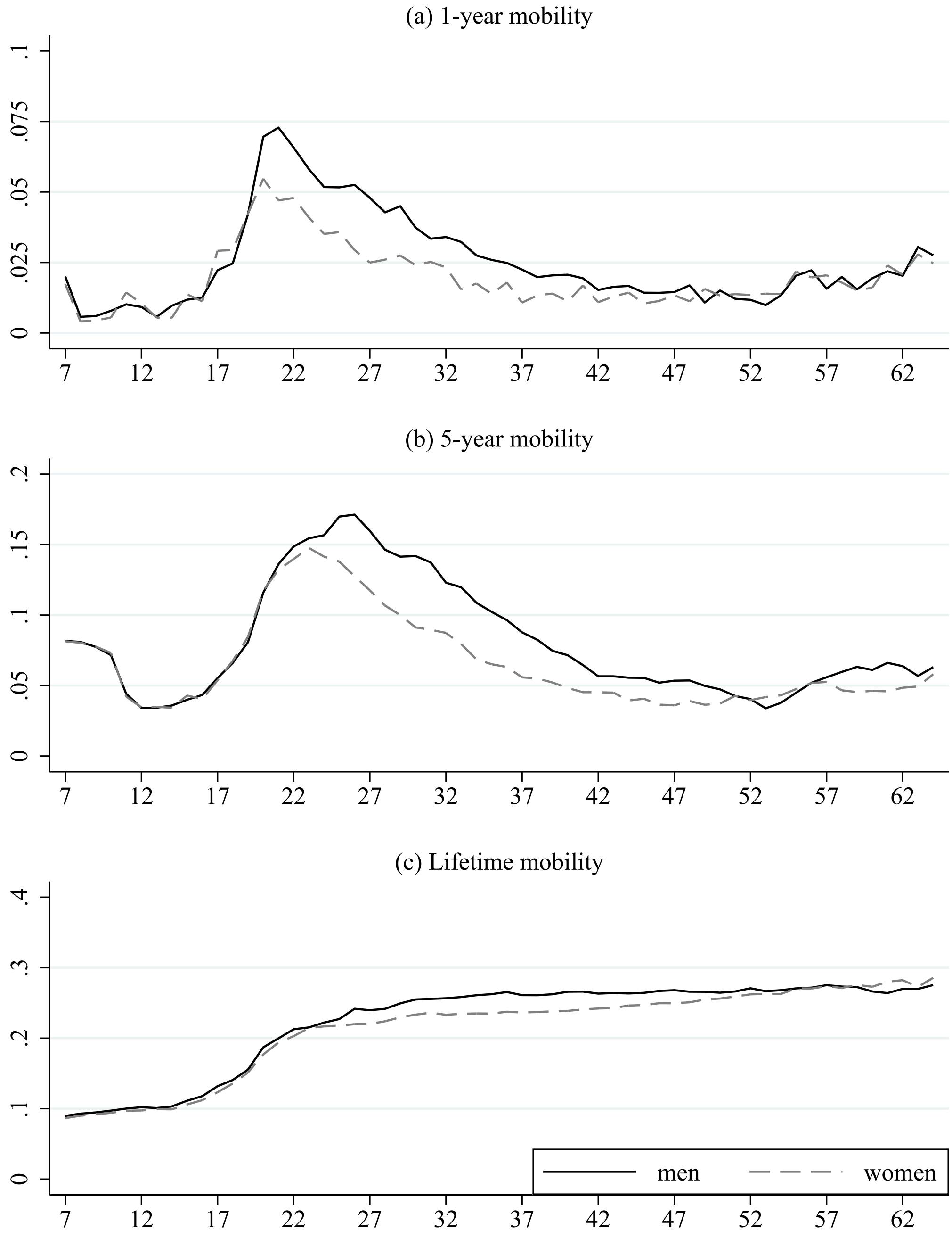
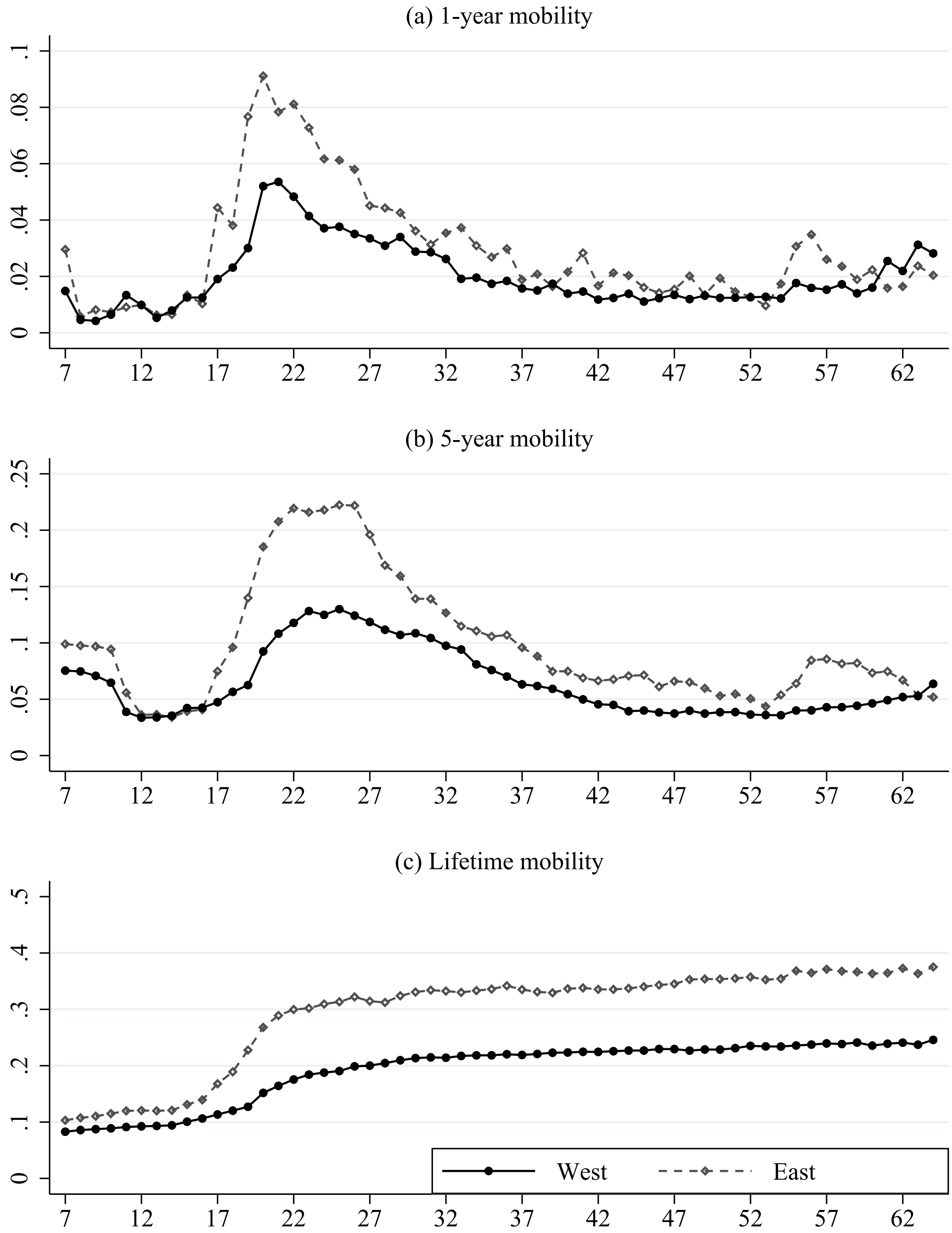
Figure A1: Age-specific cross-state mobility rates by gender



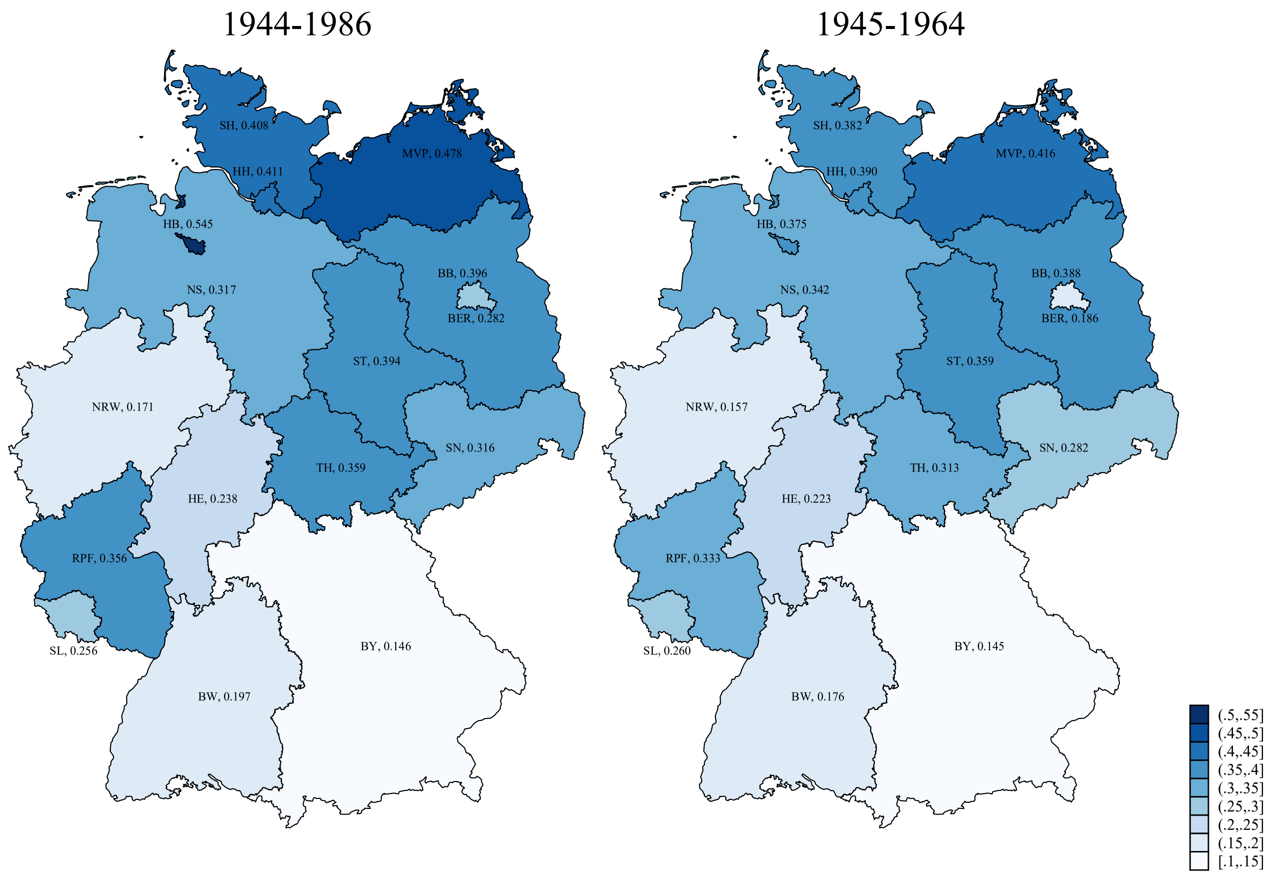
Note: Sample restricted to individuals born in Germany. Data weighted using a cross-sectional weight calibrated to Micro Census 2011. *Source:* NEPS SC6:13.0.0.

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



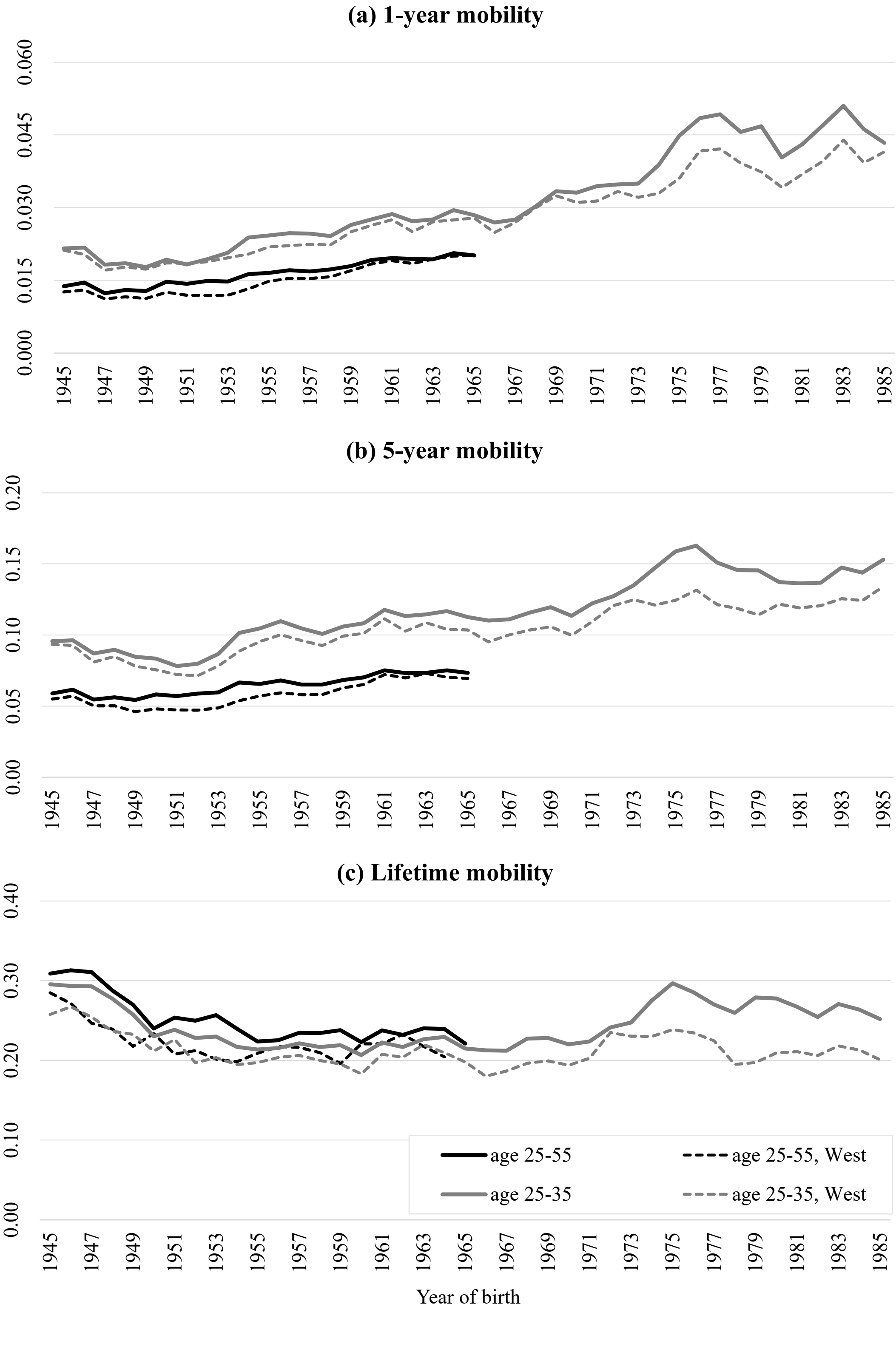
Note: Sample restricted to individuals born in Germany. Data weighted using a cross-sectional weight calibrated to Micro Census 2011. *Source:* NEPS SC6:13.0.0.

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



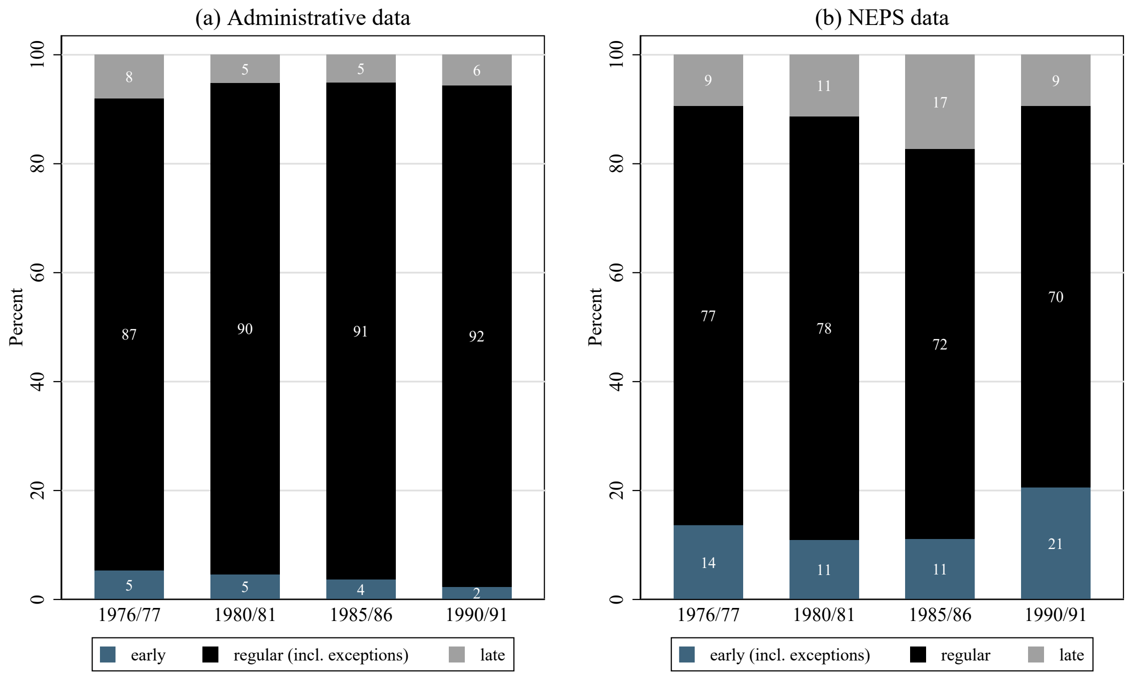
Note: Sample restricted to individuals born in Germany. Mobility is measured at ages 25-55. The numbers next to the states' acronyms indicate the mean cohort- and state-specific lifetime mobility rate. Data weighted using a cross-sectional weight calibrated to Micro Census 2011.
Source: NEPS SC6:13.0.0.

Figure A4: Trends in cross-state mobility over time



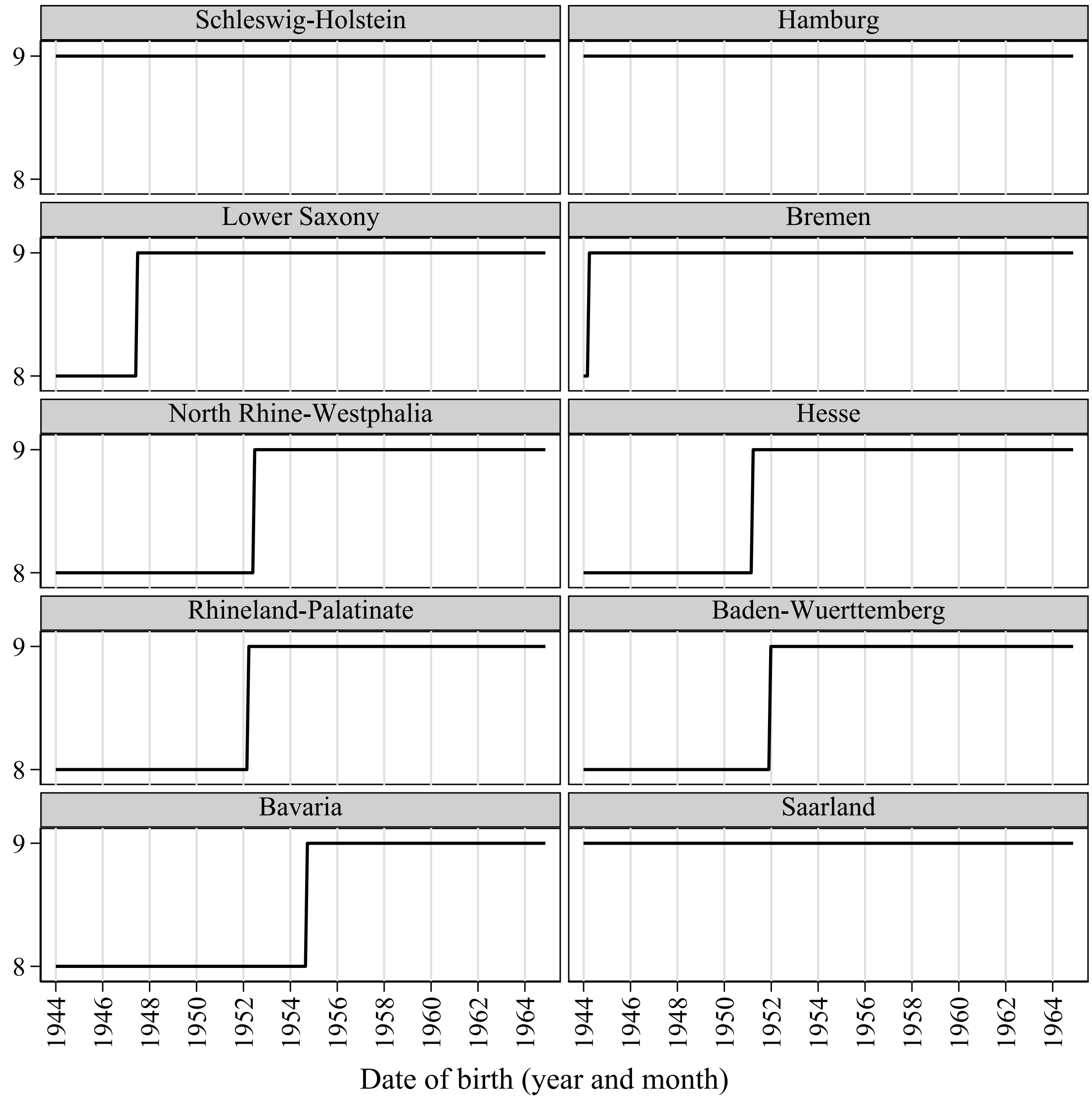
Note: Sample restricted to individuals born in Germany. Data weighted using a cross-sectional weight calibrated to Micro Census 2011. To smooth the data, the trends show three-year moving averages (i.e., including -/+1 year) instead of year-specific means.
Source: NEPS SC6:13.0.0.

Figure A5: School starters by the type of enrollment



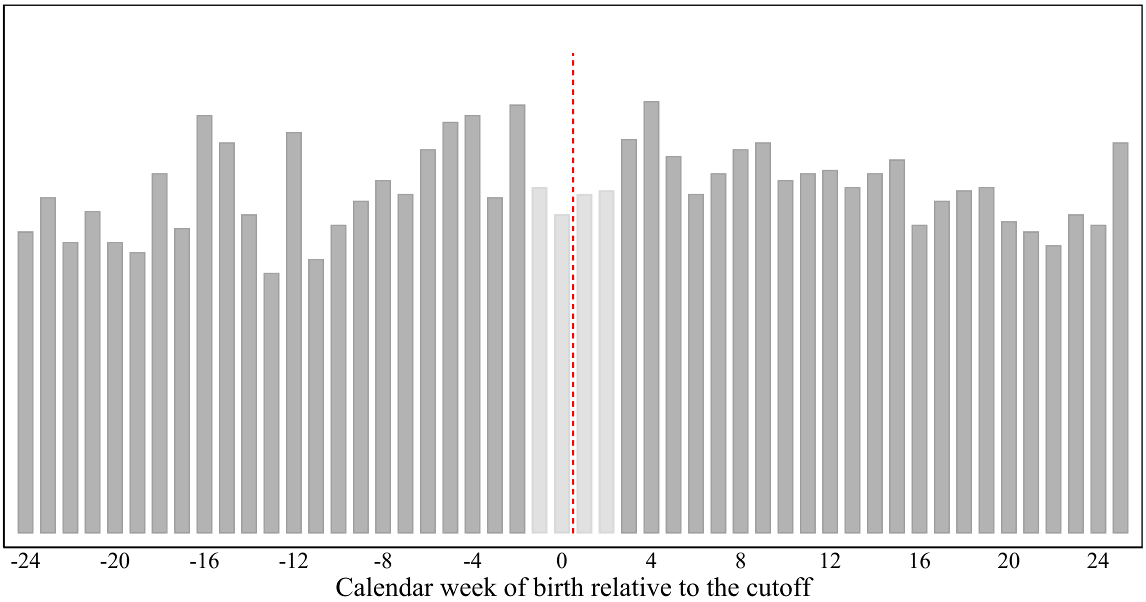
Note: The figures show the relative numbers of students enrolled in a particular school year by the enrollment type. Save for 1990/1, the numbers include only West German states (incl. West Berlin). *Source:* The administrative data are from various years of "Fachserie 11, Reihe 1, Bildung und Kultur, Allgemeinbildende Schulen" published annually by DESTATIS (Federal Statistical Office, Wiesbaden); NEPS SC6:13.0.0.

Figure A6: Compulsory schooling requirement by state and birth cohort



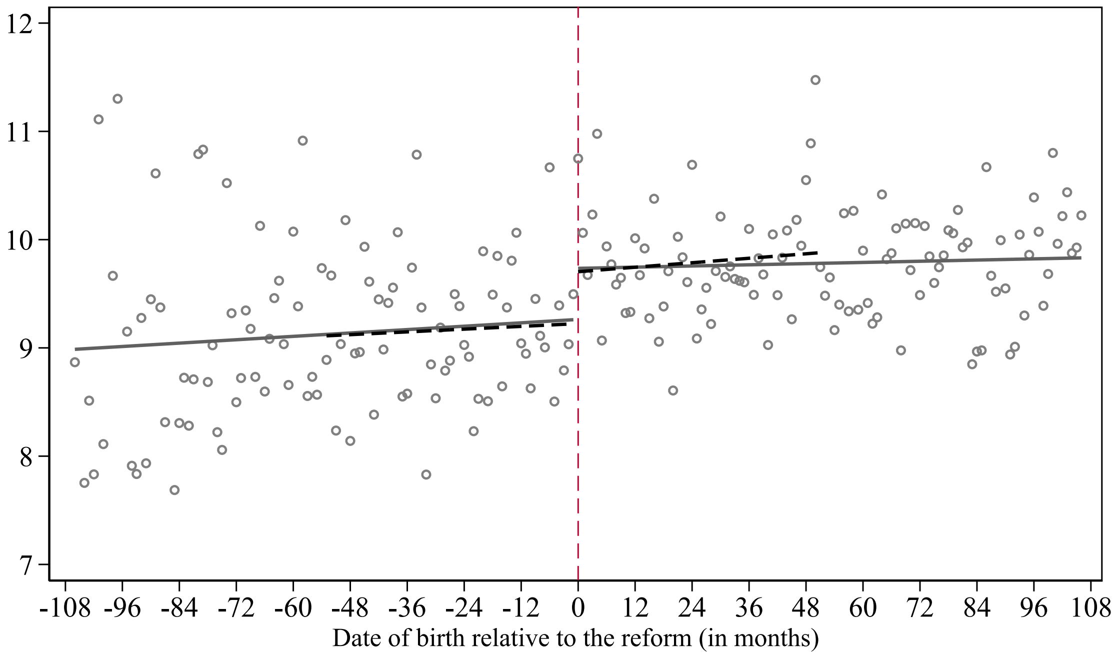
Note: The figure shows the required duration of compulsory schooling depending on the date of birth, which determines the expected year of school enrollment.
Source: State-specific laws from Makrolog (2019). Further details available on request

Figure A7: Distribution of births by the running variable



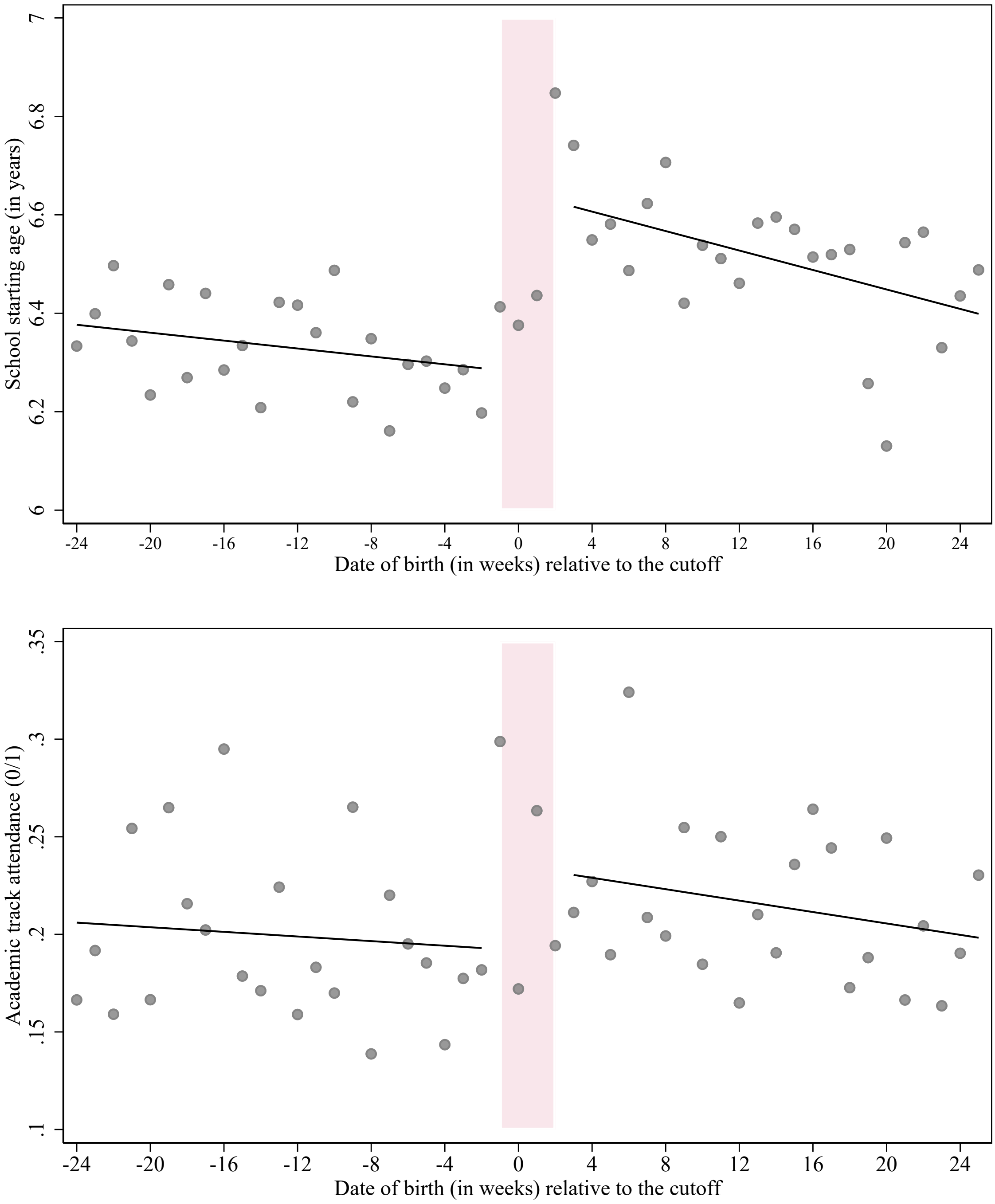
Note: The figure shows the number of individuals in our estimation sample by the calendar week of birth relative to the cutoff for school enrollment. The lighter bars indicate the range of the running variable excluded in our donut-hole RDD regressions (-/+2 points). The density test using the robust inference procedure recommended by Cattaneo et al. (2020) yields a p-value of 0.5154.
Source: NEPS SC6:13.0.0.

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



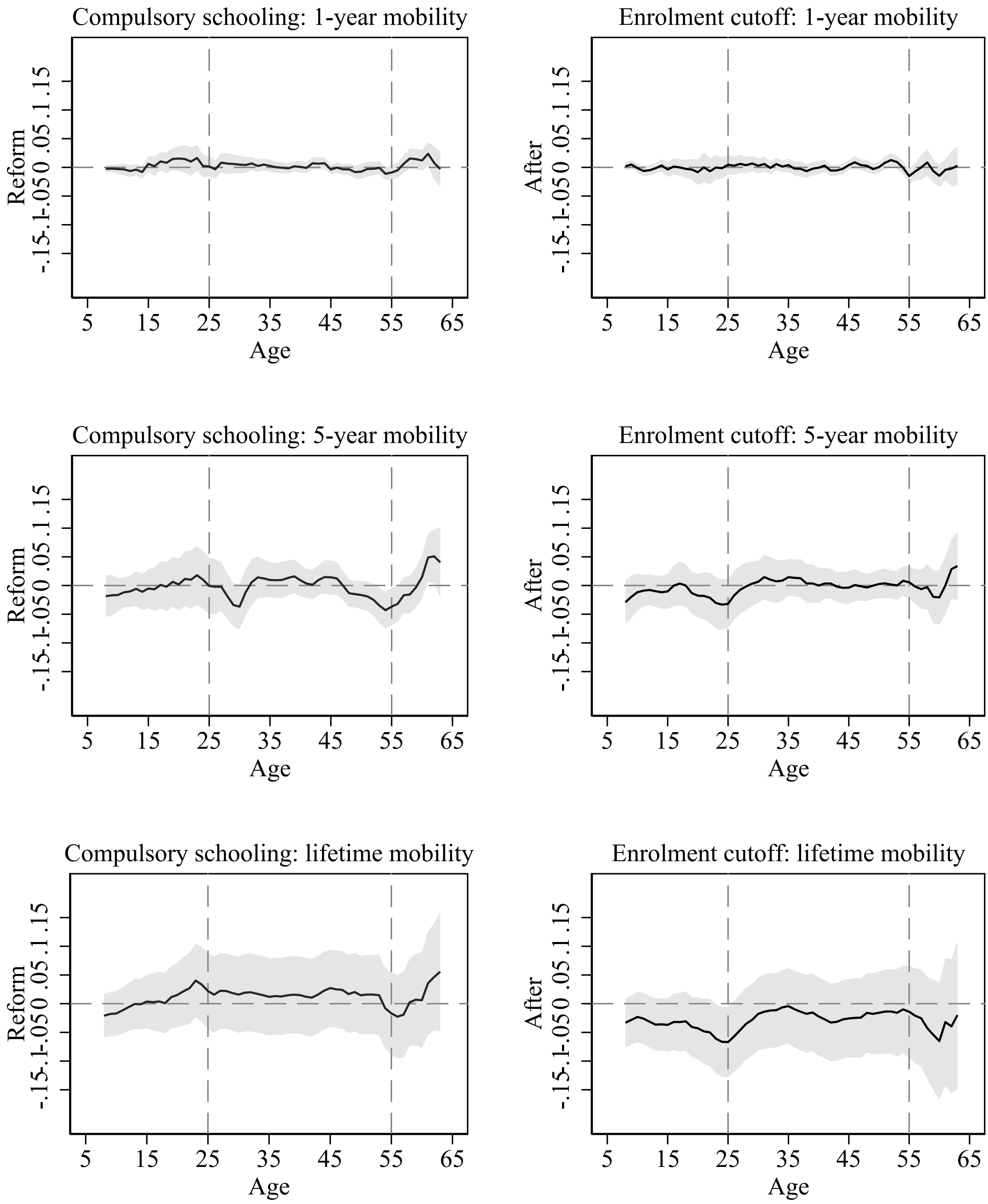
Note: Duration of schooling (in years) is measured in calendar time (not in grades). The variable is calculated as the difference between the date an individual left school and the date he/she entered school. Birth date on the x-axis is measured in months relative to the first birth cohort exposed to 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 SC6:13.0.0.

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 enrollment 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 SC6:13.0.0.

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 Table 4 and Table 5, respectively. To smooth the estimates, each age-specific regression includes observations from a three-year moving window centred on a given age year (i.e. including -/+1 age year).
Shaded areas show 95% confidence intervals based on standard errors clustered at the individual level.
Source: NEPS SC6:13.0.0.

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	42.45	40.91	41.13	42.66	41.04	41.41
Year of birth	1965	1967	1966	1968	1970	1969
Month of birth	6.312	6.310	6.360	6.334	6.348	6.374
Female	0.522	0.507	0.499	0.517	0.495	0.498
High school degree	0.414	0.287	0.320	0.439	0.327	0.340
Individuals	7,936	7,936	232,160	9,430	9,430	220,769

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 SC6:13.0.0. 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	-0.035	-0.014
	(0.040)	(0.047)	(0.042)	(0.012)	(0.040)	(0.044)	(0.044)	(0.011)
German parents (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)
German parents: miss.		0.020	0.069	-0.001		0.012	0.012	-0.003
		(0.095)	(0.083)	(0.004)		(0.098)	(0.098)	(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	-0.049		0.003	0.003	0.023
		(0.009)	(0.008)	(0.037)		(0.009)	(0.009)	(0.041)
Firstborn: miss.		0.013	0.005	0.007		-0.031	-0.031	-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***			0.001	-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 SC6:13.0.0.

Table A3: Average Characteristics of Compliers

Sample	Compulsory Schooling			Enrollment cutoffs		
	Complier	**Non-Complier**	**Diff.**	**Complier**	**Non-Complier**	**Diff.**
Female (0/1)	0.49	0.53	-0.05	0.54	0.49	0.05
Year of Birth	1954.57	1956.17	-1.59	1955.71	1955.18	0.53
State: Schleswig-Holstein	0.04	0.05	-0.01	0.05	0.05	0.00
State: Hamburg	0.03	0.03	0.00	0.03	0.04	-0.01
State: Lower Saxony	0.13	0.13	0.00	0.13	0.14	-0.01
State: Bremen	0.01	0.02	-0.01	0.02	0.03	-0.01
State: North Rhine-Westphalia	0.30	0.27	0.03	0.28	0.28	-0.01
State: Hesse	0.05	0.10	-0.05	0.06	0.09	-0.03
State: Rhineland-Palatinate	0.07	0.07	0.00	0.07	0.08	-0.01
State: Baden-Wuerttemberg	0.14	0.16	-0.02	0.14	0.16	-0.02
State: Bavaria	0.22	0.15	0.07	0.20	0.12	0.09
State: Saarland	0.02	0.02	0.00	0.02	0.02	0.00
Parental education (in yrs)	10.07	11.29	-1.23	11.01	10.97	0.04
Parental education: miss.	0.02	0.02	0.00	0.02	0.02	-0.00
German parents (0/1)	0.97	0.97	0.00	0.98	0.98	0.00
German parents: miss.	0.01	0.01	0.00	0.00	0.01	-0.00
Maternal age at birth (in yrs)	26.64	27.37	-0.73	28.41	27.94	0.47
Maternal age at birth: miss.	0.06	0.03	0.03	0.04	0.06	-0.02
Firstborn (0/1)	0.39	0.44	-0.05	0.42	0.43	-0.01
Firstborn: miss.	0.04	0.08	-0.04	0.06	0.05	0.01
Kindergarten attendance (0/1)	0.46	0.53	-0.07	0.48	0.54	-0.06
Kindergarten attendance: miss.	0.01	0.01	0.00	0.01	0.02	0.00
Born in rural municipality (0/1)	0.41	0.34	0.07	0.36	0.36	0.00
Short school yrs (0/1)	0.24	0.37	-0.13	0.28	0.38	-0.10
No. Individuals	3,579	1,681		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 enrollment 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 SC6:13.0.0

Table A4: Effects of Compulsory Schooling on Cognitive Skills and Risk Affinity

	(1) Reading Competency	(2) Reading Speed	(3) Listening comprehension	(4) Math Competency	(5) Risk Affinity
Panel A: DD Estimate of the Effect on Cognitive Skills					
Reform	0.252***	0.167*	0.017	0.070	-0.037
	(0.085)	(0.092)	(0.092)	(0.119)	(0.084)
Y-Mean	0.000	0.000	0.000	0.000	0.000
Obs./Indiv.	3,411	3,685	3,269	2,260	3,827
Panel B: DD Estimate of the Effect on the Probability of a Missing Outcome					
Reform	0.013	0.020	0.023	-0.029	0.021
	(0.038)	(0.037)	(0.038)	(0.034)	(0.036)
Y-Mean	0.351	0.299	0.379	0.570	0.272
Obs./Indiv.	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 SC6:13.0.0.

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

	(1) Reading Competency	(2) Reading Speed	(3) Listening comprehension	(4) Math Competency	(5) Risk Affinity
Panel A: RDD Estimate of the Effect on Cognitive Skills					
Reform	-0.042	-0.061	0.141	0.046	-0.241**
	(0.097)	(0.101)	(0.106)	(0.126)	(0.101)
Y-Mean	0.000	0.000	0.000	0.000	0.000
Obs./Indiv.	3,021	3,263	2,878	1,975	3,353
Panel B: RDD Estimate of the Effect on the Probability of a Missing Outcome					
Reform	-0.055	-0.063	-0.023	-0.026	-0.042
	(0.043)	(0.043)	(0.042)	(0.036)	(0.040)
Y-Mean	0.351	0.299	0.381	0.575	0.279
Obs./Indiv.	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 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. Robust standard errors in parentheses. *Source:* NEPS SC6:13.0.0.

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

	(1) Basic Degree	(2) Middle Degree	(3) High School	(4) College/ University	(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.			5,259		

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, rural place of birth, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Robust standard errors in parentheses.
Source: NEPS SC6:13.0.0.

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/ University	(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	-0.039	0.018	0.021	0.007	-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, state fixed effects, birth cohort fixed effects, parental education and citizenship, maternal age at birth, an individual's birth order, rural place of birth, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Robust standard errors in parentheses.
Source: NEPS SC6:13.0.0.

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 $\times$ 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. 52,442/1,692)	(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, rural place of birth, 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. Panel D applies the ETWFE by Wooldridge (2021). The placebo reform in Panel G is based on randomly assigned reform dates across states.
Source: NEPS SC6:13.0.0.

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)	(0.015)	(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 in Panel D comprise gender, state fixed effects, birth cohort fixed effects, parental education and citizenship, maternal age at birth, an individual's birth order, rural place of birth, 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. (2020a). 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 SC6:13.0.0

Table A10: Long-Run Effect of Compulsory Schooling on Number of Moves

	Cross-State Mobility			Cross-County Mobility		
	At Least One Move	**Absolute No. of Moves**	**Log (No. of Moves + 1)**	**At Least One Move**	**Absolute No. of Moves**	**Log (No. of Moves + 1)**
Panel A: DD regressions without controls						
Reform	-0.043	0.008	-0.014	-0.024	0.056	0.066
	(0.033)	(0.067)	(0.033)	(0.039)	(0.105)	(0.037)
Panel B: DD regressions with controls						
Reform	-0.058	0.000	-0.023	-0.019	0.077	0.011
	(0.036)	(0.073)	(0.036)	(0.042)	(0.113)	(0.048)
Y-Mean	0.260	0.483	0.258	0.546	1.190	0.587
Obs./Indiv.			5,260			

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, rural place of birth, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Robust standard errors in parentheses.
Source: NEPS SC6:13.0.0

Table A11: Long-Run Effect of Being Born After the Cutoff on Number of Moves

	Cross-State Mobility			Cross-County Mobility		
	At Least One Move	**Absolute No. of Moves**	**Log (No. of Moves + 1)**	**At Least One Move**	**Absolute No. of Moves**	**Log (No. of Moves + 1)**
Panel A: RDD regressions without controls						
After	0.005	0.058	0.019	-0.022	-0.022	-0.024
	(0.036)	(0.072)	(0.036)	(0.042)	(0.114)	(0.048)
Panel B: RDD regressions with controls						
After	0.001	0.045	0.013	-0.020	-0.040	-0.024
	(0.034)	(0.069)	(0.034)	(0.040)	(0.108)	(0.048)
Y-Mean	0.256	0.470	0.252	0.543	1.186	0.584
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 Controls comprise gender, state fixed effects, birth cohort fixed effects, parental education and citizenship, maternal age at birth, an individual's birth order, rural place of birth, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Robust standard errors in parentheses.
Source: NEPS SC6:13.0.0

Table A12: Long-Run Effect of Compulsory Schooling on Regional Mobility: Alternative Geographic Units

	Across Labor Markets			Across Metropolitan Areas		
	1-Year	5-Year	Lifetime	1-Year	5-Year	Lifetime
Panel A: DD regressions without controls						
Reform	0.002	0.002	0.013	0.000	0.000	0.006
	(0.003)	(0.010)	(0.033)	(0.002)	(0.009)	(0.031)
Panel B: DD regressions with controls						
Reform	0.004	0.001	-0.004	-0.000	-0.004	0.004
	(0.003)	(0.011)	(0.036)	(0.002)	(0.009)	(0.033)
Y-Mean	0.029	0.115	0.413	0.021	0.081	0.285
Obs.	159,716					
Indiv.	5,260					

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, rural place of birth, kindergarten attendance, exposure to short school years, and dummies for missing information on each covariate. Robust standard errors in parentheses.
Source: NEPS SC6:13.0.0.

Table A13: Long-Run Effect of Being Born After the Cutoff on Regional Mobility: Alternative Geographic Units

	Across Labor Markets			Across Metropolitan Areas		
	1-Year	5-Year	Lifetime	1-Year	5-Year	Lifetime
Panel A: RDD regressions without controls						
After	0.001	-0.003	-0.042	0.002	-0.002	-0.048
	(0.003)	(0.012)	(0.037)	(0.003)	(0.010)	(0.033)
Panel B: RDD regressions with controls						
After	-0.000	-0.006	-0.050	0.001	-0.004	-0.049
	(0.003)	(0.011)	(0.036)	(0.003)	(0.009)	(0.032)
Y-Mean	0.029	0.114	0.417	0.020	0.080	0.288
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 Controls comprise gender, state fixed effects, birth cohort fixed effects, parental education and citizenship, maternal age at birth, an individual's birth order, rural place of birth, 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 SC6:13.0.0.