The Effect of Schooling on Parental Integration

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Abstract. *Exploiting the age-at-enrollment policies in 16 German states as exogenous source of variation, I examine whether the schooling of the oldest child in a migrant household affects parents' integration. My analysis links administrative records on primary school enrollment cutoff dates with micro data from the German Socioeconomic Panel (SOEP). Using a regression discontinuity design around the school enrollment cutoff and an instrumental variable approach I show that children's schooling improves the integration of parents along several dimensions, such as labor market outcomes, financial worries, and German language skills. Labor market outcomes are most positively affected for mothers. Additional analysis of underlying mechanisms suggests that results are driven by gains in disposable time and exposure to the German language and culture.*

Keywords: *international migration, assimilation, integration, education, schooling, family, regression discontinuity, instrumental variables*

JEL Classification: *F22, I24, I26, J16*

1 Introduction

Immigration into developed countries has become an increasingly important topic in recent decades and is not going to subside anytime soon. By the end of 2020, Germany had a migrant population of over 10 million people[2], representing 13.7 % of the nation's total population (Destatis 2020). One million alone are Syrian refugees who entered Germany in the mid 2010s (BAMF 2016). Such inflow poses major challenges for public policy (Angelini et al. 2015), first and foremost the question of successful integration into the host country. The literature on labor market outcomes, cultural and social assimilation and well-being of migrants is vast and shows over and over again how migrants lack behind their native counterparts. They obtain lower wages and show higher unemployment rates (Algan et al. 2010; Borjas 2015) and suffer from cultural or political marginalization (Algan et al. 2012). Not only the migrants themselves suffer from their

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[2]Individuals who were not born in Germany but regularly reside in Germany.

disintegration (Angelini et al. 2015) but so do the host countries, as it potentially leads to ethnic enclaves and social unrest (Gathmann and Keller 2018). On the other hand, the inflow of migrants constitutes a major chance for countries like Germany to address their demographic change and the subsequent shortage of skilled workers. Consequently, the most pressing question for host countries is how to facilitate successful integration of immigrants into the country.

Integration is a complex, multidimensional process, spanning economic outcomes as well as social and cultural assimilation (Constant and Zimmermann 2008; Facchini et al. 2015). Lots of works focus on labor market outcomes (Aldashev et al. 2009; Bleakley and Chin 2004; Chiswick and Miller 1995; Dustmann 1994; Dustmann and Van Soest 2001), and the positive effects of native language skills on labor market outcomes specifically (Aldashev et al. 2009; Bleakley and Chin 2004; Chiswick and Miller 1995; Dustmann 1994; Dustmann and Fabbri 2003; Dustmann and Van Soest 2001). Some works also observe societal integration (Danzer and Yaman 2013; Gambaro et al. 2021), and well-being (Battisti et al. 2022). One potentially key factor driving such integration outcomes is primary schooling. While the effect of school attendance and attendance of early childhood education and care facilities (ECEC, e.g. kindergarten) on migrant children themselves has been extensively studied (Bleakley and Chin 2008; Cornelissen et al. 2018; Felfe and Lalive 2018), relatively few studies have investigated how attendance of such facilities might impact their parents. Drange and Telle (2015) used data from Norway and found that ECEC attendance among migrant children did not have a significant impact on their parents' employment or educational outcomes. Gambaro et al. (2021), on the other hand, exploit regional differences in the availability of ECEC facilities in German states as exogenous sources of variation to estimate the effect of attendance of such facilities by refugee children on their parents' integration. They create an integration index from several measures of integration in the German Socioeconomic Panel (SOEP) and find a significant positive effect on overall integration, with particularly strong effects on labor market outcomes and language proficiency.

In this study I provide the first evidence for a positive effect of primary schooling on parents' integration. I link micro data from the German Socioeconomic Panel with administrative records on primary school enrollment cutoff dates, exploiting the German age-at-enrollment policy as exogenous source of variation in school entry timing. Using a regression discontinuity design around the school enrollment cutoff and an instrumental variable approach, my results show that both early school enrollment as well as each additional month of schooling of the oldest child in the household positively affect parental integration. Schooling increases labor market participation – i.e. parents' probability to be in regular employment and their weekly working hours – as well as monthly income and hourly wages. These effects are especially strong among the formerly unemployed and those who carry the main burden of childcare in the household, i.e. mothers. Apart from labor market outcomes, I find positive effects on parents' financial worries, health status, staying intentions and self-assessed German language skills.

I assess two potential channels driving effects: time and exposure. The first is based on the assumption that upon enrollment of their oldest child, parents have more disposable time on hand which they can then use to actively work on their integration (e.g. by attending language courses) or to participate in the labor market. The second relates to the idea that children's school attendance entails exposure to the German language and culture. Though they cannot clearly be disentangled, I find evidence that both channels play a role in shaping integration outcomes.

The rest of the paper is organized as follows. Section 2 provides institutional background on the German primary schooling system and describes the research design and data. Section 3 presents main results, Section 3.1 examines potential mechanisms and Section 3.2 draws a comparison to outcomes among a sample of parents born in Germany. Section 3.3 discusses limitation of the analysis and provides robustness checks. Section 4 concludes.

2 Institutional Background, Estimation Strategy and Data

Estimating the effect schooling on the integration of the schooled child's parents can be challenging given that migrant parents differ in their ability and willingness to integrate. To overcome this, I exploit age-at-enrollment policies in the 16 German states as exogenous source of variation for school enrollment timing.

2.1 Institutional Background

In Germany schooling is free and compulsory. From the age of six up to the age of 18 (age of legal majority) children are officially obliged to attend school. This includes primary and secondary school and, after finishing secondary education, vocational school. Parents have to ensure that their child fulfills their obligation to attend school or otherwise face legal consequences – penalty fees and in some states even prison sentences up to 6 months. The exact length of compulsory schooling as well as its start is subject to states (*Bundesländer*) legislation.

In each of Germany's 16 states, the start of compulsory schooling is defined relative to a cutoff date. While these cutoff dates differ between states, they all follow the same general rule: children who turn six before or on the cutoff date of the state they regularly reside in are admitted to primary school in the respective school year. Children who turn six after the cutoff date are admitted to primary school one year later. The start of the school year itself differs between states, too, but is usually between the end of July and the middle of September.

In addition, there is some basic maturity test administered to all children who are about to enter school. Based on this test, school enrollment can be postponed by one year even if children are born before the cutoff date. Postponement can also happen upon the parents' specific wish that their child be enrolled a year later. According to the parents' wish, children can also be admitted prematurely if they are born after the cutoff date.

Parents might bring forward enrollment if their child shows signs of learning potential that exceeds their age cohort average (Angrist and Krueger 1992). On the other hand, they might postpone enrollment because they feel their child lacks the necessary maturity for enrollment (*absolute age effect*, see e.g. DiPasquale et al. 1980; Fredriksson and Öckert 2014) or to give their child a comparative advantage over their classmates (*relative age effect*, see e.g. Deming and Dynarski 2008).[3] This might lead to distortion in the identification strategy and will be addressed in Section 2.3.[4]

2.2 Data

For this analysis I match administrative records on primary school enrollment cutoff dates with micro data from the German Socioeconomic Panel (SOEP). The enrollment cutoff dates are obtained from the Journals of Laws and Ordinances (*Gesetz- und Verordnungsblätter*) of the respective states and are available for all 16 states from the year 1992 on. The cutoff dates vary, depending on state and year, between the last day of June and the last day of December. The German Socioeconomic Panel is a longitudinal household survey across all 16 German states (Goebel et al. 2019).[5] It provides yearly information on households and all individual household members since 1984. In addition to information on migration background and state of residence as well as birth dates and enrollment years of children, it offers a wide variety of questions on sociodemographic status and integration outcomes. For my sample I utilize the SOEP waves of 1992 to 2020 (the first year for which I can provide complete records on school enrollment cutoffs for all 16 states up to the last currently available year). I identify all adult migrants (i.e. individuals who are not born in Germany and have migrated to Germany at age 16 or above) for whom data on their oldest child's birth date and actual school enrollment is available.

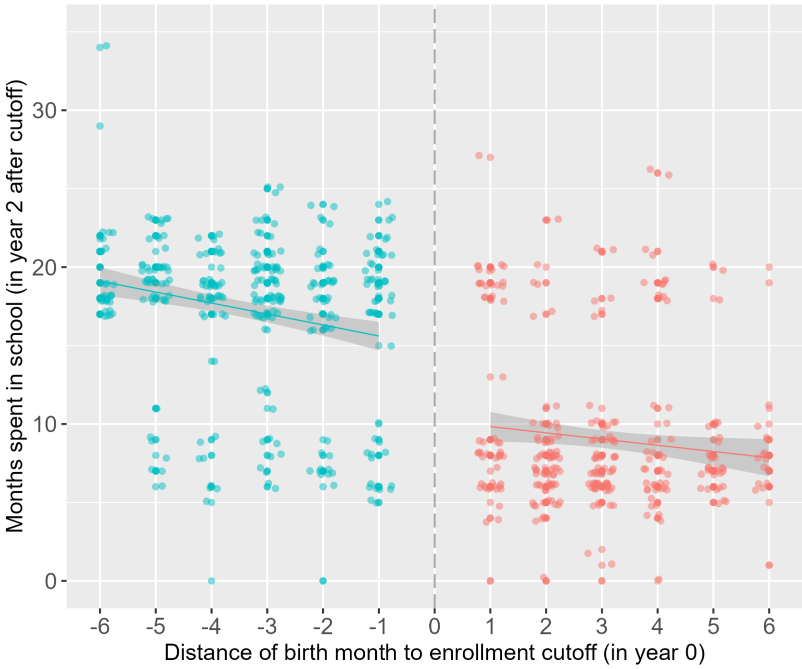
I focus on the oldest child in the household since they are naturally the first ones to enter school. The existence of children who had entered school earlier in the household

[3] Research on age-at-enrollment has overtly shown that later school entry can raise academic achievement (Black et al. 2011; Cascio and Schanzenbach 2016; McEwan and Shapiro 2008; Puhani and Weber 2008), and even positively affect long-term life outcomes (Bauer and Riphahn 2009; Bedard and Dhuey 2006, 2012; McAdams 2016). Other works, however, show how early school entrance can be beneficial due to longer total schooling (in states where compulsory schooling legally ends after a certain age is reached) or due to peer effects (Currie 2001). Both is of special benefit for disadvantaged pupils who would otherwise have not spend so much time in a positive learning environment, like migrants (Schneeweis 2006).

[4] For a detailed description of the German school system see Lohmar and Eckhardt (2014).

[5] The SOEP is an extensive representative survey of the population in Germany which has been conducted yearly since 1984 and covers a wide range of information on each individual living in the observed households, including underage children. Many studies use the SOEP as data base for their analyses. E.g. Kaas et al. (2021) in exploring low homeownership rates in Germany or Odermatt and Stutzer (2019) in studying the accuracy with which people predict their future well-being after facing major life events like unemployment, widowhood, or disability. With regards to early childhood care, e.g. Bick (2016) examines the role of available early childcare on womens' labor market participation and fertility using SOEP data. For a detailed description of the SOEP see Wagner et al. (2007).

Figure 1: *Discontinuity in Months Spent in School (in Year 2 After Cutoff)*



Note: Number of months spent in school in year 2 after initial cutoff by eligibility for enrollment in year 0 (no = red, yes = blue). Vertical and horizontal noise added to avoid overplotting.

would distort the identification of the effect of school enrollment on parental integration.[6][7] To assess the child's enrollment eligibility I compare their birth month and year with the enrollment cutoff date at the state their family resides in the year they turn six.[8] If they were born before or on the respective cutoff date, they are assumed eligible for enrollment in this year, and if they were born after the cutoff they are assumed eligible for enrollment in the following year. Lastly, I eliminate all individuals with missings in relevant variables. This yields 678 individuals in 473 households.[9]

Figure 1 plots the average months spent in school by the oldest child two years after they turned six (*initial cutoff*) against their birth month distance to the enrollment cutoff. Negative distance means they are born before the cutoff and hence were eligible for enrollment the year they turned six (year 0), positive distance means they were born after the cutoff and hence were not eligible for enrollment the year they turned six (year 0), but only one year later. E.g. if an oldest child in a given household turned six on June 15th and the cutoff date in their state of residence was June 30th, they would be eligible

[6]There are no households in the sample for which a younger child is enrolled earlier than the oldest child.

[7]The existence of younger children in the household, who potentially visit early childhood education and care (ECEC) facilities, could similarly distort the identification, and will be dealt with accordingly (for more detail see Section 2.3).

[8]Since birth month and year of children is most commonly provided while exact birth day is not, I utilize monthly cutoffs. This does not reduce precision since all enrollment cutoff dates in all states and years observed relate to the first or last day of a respective month.

[9]For a step-by-step explanation of sample construction and shrinkage see Table A in the Appendix.

for enrollment in the same year (year 0), and had a distance to the cutoff of -1 in the graph. Had they been born on July 1st of the respective year in the same state, they would not have been eligible for enrollment in the same year but only one year later and had a distance to the cutoff of +1 in the graph.[10]

The focus on outcomes in the second year after the oldest child turns six (year 2 after cutoff) is explained by one major limitation in the SOEP data. The yearly surveys of the SOEP are done during all 12 months of each year, but actual school enrollment, depending on state, only happens between July and September. Focusing on outcomes two years after the earliest enrollment ensures that the households which children were eligible for enrollment in the same year they turned six had at least one full year of schooling before their outcome is measured.[11]

As can be seen in Figure 1, there is a considerable discontinuity in months spent in school by the oldest child in year 0 between those eligible for enrollment in year 0 (i.e. born before the cutoff, pictured in blue), and those not eligible (i.e. born after the cutoff, pictured in red). On average, those eligible in year 0 have spent 8.7 more months in school in year 2 than those not eligible (17.7 months compared to 9.0 months).

To obtain reliable estimates of effects this difference in oldest child's school enrollment timing and months spent in school has on parental integration outcomes, several assumptions regarding the data need to hold. First, birth dates of the oldest children and consequently their enrollment eligibility should be exogenously given. While there has been some discussion on potential correlation between children's birth month and parental characteristics, the assumption that children's birth dates are exogenously given is widely used in economic literature, especially regarding the effects of age at school entry (Angrist and Krueger 1992) and policy changes that affect only children born after a certain cutoff and their parents (Cygan-Rehm et al. 2018; Danzer and Lavy 2018; Dustmann and Schönberg 2012).

Second, assuming that the children's birth dates are exogenously given, parents should not differ in their characteristics except for their oldest child's enrollment eligibility. Hence, I compare the averages in parental sociodemographic characteristics and observed integration outcomes between both groups prior to any school enrollment. Due to the data structure of the SOEP, households are potentially surveyed after the enrollment of their oldest child in year 0, which could distort the results. Therefore I use one year before the initial enrollment cutoff as control year. This ensures that all observed households are surveyed strictly before the enrollment of their oldest child. As Table 1 shows, their differences are mostly negligible, except for a slightly higher average years of education among the group whose children are eligible in year 0. This is to be expected given that children who are eligible for enrollment at age six tend to be slightly older than those who

[10]Due to the aforementioned limitations in the data regarding precise birth days the cutoff distance can only relate to full months. I.e. I cannot differentiate whether the child is born on June 1st or June 30th, their cutoff distance will in both cases be -1 if the cutoff date is June 30th. Hence the cutoff distance can take any integer value between -6 and 6, except for 0.

[11]The potential threat to identification the differences in survey months poses and how I deal with it is discussed in Section 2.3.

Table 1: *Differences in Parental Characteristics and Integration Outcomes between Parents whose Oldest Child was Eligible and Not Eligible for Enrollment the Year They Turned Six*

	Not eligible	Eligible	Difference
Parental characteristics			
Age	32.5	32.8	0.3
Female (%)	52.5	55.6	3.1
Years of education	10.6	10.9	0.3^{*}
Currently in parental leave (%)	9.3	10.7	1.4
Owner of housing (%)	24.8	23.6	-1.2
Refugee (%)	5.0	6.5	1.5
Years since migration	15.3	15.1	-0.2
Number of children	1.7	1.7	0.0
Younger children in ECEC (%)	12.4	16.6	4.2
Oldest in ECEC before enrollment (%)	84.8	91.0	5.2^{**}
Parental integration outcomes			
Monthly parental income (Euro)	$1,128.8$	$1,079.3$	-49.5
Currently employed (%)	62.1	56.5	-5.6
Working hours per week	23.9	22.3	-1.6
Hourly net wage (Euro)	7.0	6.7	-0.3
Childcare hours per day	4.7	5.0	0.3
Worried about own finances (scale 1-3)	2.1	2.2	0.1
Staying intention (%)	69.3	75.1	5.8
Number of observations	322	356	678

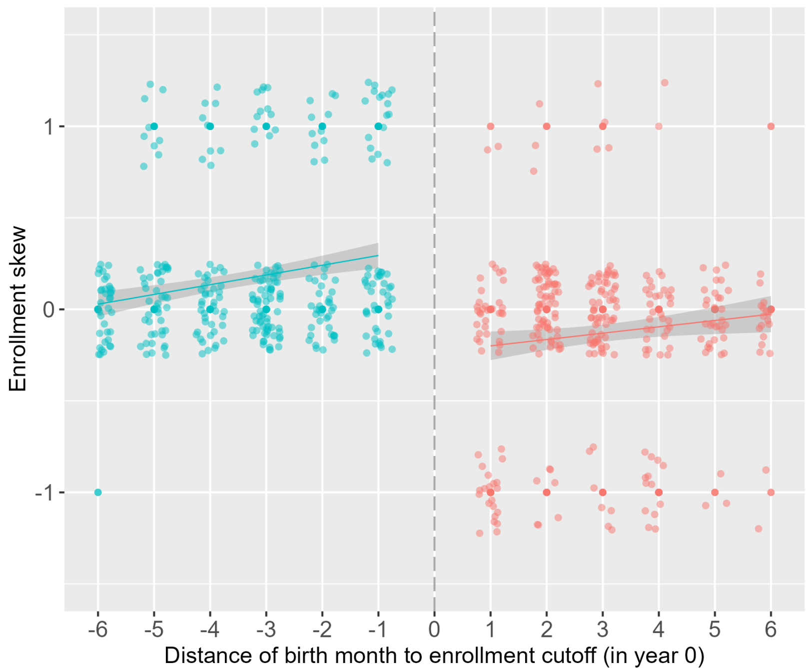
Note: Means 1 year before initial enrollment cutoff. Unpaired two-sample Wilcoxon tests with potential unequal variance in both samples for differences in variables between groups. For detailed variable description see Table B in the Appendix. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

are not. Also, the percentage of children being in ECEC facilities, such as kindergartens, before enrollment is slightly higher among the eligible. The differences in age of children and their earlier ECEC attendance could pose a threat to identification, which will be addressed in Section 2.3.

Third, the *common trend assumption* should hold, i.e. in the absence of treatment (here: eligibility for enrollment in year 0), the difference between the treated and not treated should be constant over time. Though I cannot statistically test this assumption, I can examine time points before the initial enrollment cutoff and extrapolate that the outcomes would have followed the same trajectory if it weren't for the treatment. Hence, prior to any effects of school enrollment, the observed parental integration outcomes should be comparable between groups (eligible and not eligible). Figure A in the Appendix plots all observed parental integration outcomes over several time points and shows that there are indeed no statistically significant differences between both groups.

Fourth, as established in Section 2.1, parents have the choice to bring forward or postpone the enrollment of their child, contrary to their state-mandated enrollment eligibility.

Figure 2: *Discontinuity in Actual Enrollment in Year 0 (Treatment Assignment)*



Note: Skew of actual enrollment by eligibility for enrollment in year 0 (no = red, yes = blue). Enrollment skew of -1 means enrollment has been brought forward by 1 year, enrollment skew of +1 means enrollment has been postponed by 1 year. Vertical and horizontal noise added to avoid overplotting.

This introduces non-compliance with the treatment assignment (eligibility for enrollment in year 0), which in turn poses a threat to identification and hence will be addresses accordingly in Section 2.3. But first, I check whether the oldest child in the household has actually been enrolled according to their eligibility or whether their enrollment has been brought forward or postponed – i.e. whether each specific household complied with the treatment assignment. Figure B in the Appendix shows that there is non-compliance with the treatment assignment in the data. 8.3 % of parents bring forward the enrollment of their oldest child by one year, and 11.0 % postpone it. Altogether, 19.3 % of parents do not comply with the treatment assignment. There is a discontinuity in the actual enrollment around the enrollment cutoff. As Figure 2 shows, bringing forward the oldest child's enrollment (enrollment skew of -1) is more likely for children closer to the cutoff. This is not surprising, as children closer to the cutoff are older than those farther away from the cutoff. Hence their parents might feel that they are ready to enter school even though they were not born before the state-mandated birth date cutoff. Vice versa, children born before the state-mandated cutoff but quite close to it are more likely to be enrolled one year later. Their parents might feel that their children are not yet ready for school despite being eligible for enrollment and postpone their enrollment by one year.

Since parents have this choice regarding enrollment, students with brought forward or delayed school entry are not randomly selected. Naturally, this raises the question whether parents who decide to deviate from the state-mandated enrollment eligibility of their child differ from parents who enroll their child in accordance with state-mandated eligibility. I compare parents who have brought forward or postponed their oldest' enrollment with those who have not made use of this option, using unpaired two-sample Wilcoxon tests. Table C in the Appendix shows that there is not much difference between the groups. Migrant parents with more education and those who have come to Germany more recently seem to make use of the option to bring forward or postpone the enrollment of their child more often. Additionally, those who did not enroll their oldest child according to eligibility have less monthly net income.

2.3 Estimation Strategy

First, I estimate the effect of one additional month of schooling. The number of months the oldest child has spent in school is not exogenously given but driven by the enrollment timing. This, in turn, is determined by the exogenous variation in birth month distance to enrollment cutoff which predicts eligibility for enrollment, and unobservable factors driving parental discretion to enroll their children in accordance with eligibility or not. To exploit the exogenous variation in birth month distance to enrollment cutoff, I utilize a two stage least squares (2SLS) method. In the first stage months of schooling M_{ht} is instrumented by enrollment eligibility E_h

$$\begin{aligned}\widehat{M_{ht}} &= \alpha_{21} + \zeta_{21} E_h + \gamma_{21} C_{ht} + \tau_{21} T_t + \phi_{21} P_i \\ &\quad + \omega_{21}(S_{ht} \times W_{iht}) + \epsilon_{21,iht}\end{aligned} \tag{1}$$

where $\widehat{M_{ht}}$ is the (estimated) number of months spent in school by the oldest child of household h in year t. E_h is a dummy variable which takes a value of 1 if the oldest child in household h was eligible for school enrollment the year they turned six, i.e. if they were assigned the treatment; and a value of 0 if the oldest child was not eligible for school enrollment the year they turned six, i.e. they were not assigned the treatment. Then the fitted values of $\widehat{M_{ht}}$ are plugged into the second stage of the 2SLS equation

$$\begin{aligned}\text{Integration Outcome}_{iht} &= \alpha_{22} + \beta_{22} \widehat{M_{ht}} + \gamma_{22} C_{ht} + \tau_{22} T_t + \phi_{22} P_i \\ &\quad + \omega_{22}(S_{ht} \times W_{iht}) + \epsilon_{22,iht}\end{aligned} \tag{2}$$

where $\text{Integration Outcome}_{iht}$ denotes the integration outcome of parent i in household h in year t. Parent i is either of the parents of the oldest child in the household h. Integration is displayed in different aspects of individuals' lives, hence observed outcomes are parental monthly income, parental employment, working hours per week, hourly income, hours spent with childcare per day, worries about personal finances, health status, staying intentions and German language skills.[12] C_{ht} are time-variant controls at

[12]Contrary to the other outcomes are parental employment and staying intentions not linear but binary outcomes, yet are estimated via linear regression.

household h level at time t; those include the number of children in the household h, a dummy indicating whether the oldest child was in ECEC before school enrollment and a dummy indicating whether any existing younger children are in ECEC.[13]

T_t are time fixed effects to control for heterogeneity in observational years and P_i are time-invariant individual fixed effects. The latter also cover country of origin fixed effects to ensure that results are not driven by factors related to origin countries. $S_{ht} \times W_{iht}$ is an interaction between the state in which the household h resides in year t and the month in which parent i in household h was surveyed in year t.[14] β_1 then denotes the estimated effect of an additional month of schooling of the oldest child in the household on parental integration outcomes. For a detailed description of all variables see Table B in the Appendix.

Second, I estimate the effect of early school enrollment of the oldest child in the household on parental integration.[15] As mentioned earlier, in order to estimate an average treatment effect, the non-compliance with the treatment assignment in the data must be accounted for. As in the 2SLS approach, this is done via instrumental variable regression. Except now treatment status D_{ih} (being enrolled in year 0) is instrumented by treatment assignment E_h (being eligible for enrollment in year 0) in a fuzzy regression discontinuity design (RDD). RDD approaches generally exploit changes in treatment status at a certain observable cutoff point. Different from the sharp RDD approach, in which the treatment status is perfectly determined by a certain cutoff point (i.e. perfect compliance with treatment assignment), in the fuzzy RDD case treatment status D_{ih} is not deterministically related to the threshold-crossing of a certain cutoff. In the data, there is a jump in the probability of treatment D_{ih} at the birth month cutoff $x_h = 0$. Before this cutoff $(x_h < x_h = 0)$ the oldest child in the household is eligible for enrollment in the given year, and after this cutoff $(x_h > x_h = 0)$ they are eligible for enrollment only

[13]The existence of younger children in the household, who potentially visit early childhood education and care (ECEC) facilities, could distort the identification. Similarly could the attendance of ECEC facilities of the oldest child before school enrollment. Hence both are controlled for in the estimations. Additionally, I run all main estimations on a subset of households with only one child, and find that results are not substantially different to those with (multiple) younger children. I also run all main estimations on a subset of households whose oldest child has attended ECEC facilities before school enrollment and find no substantial differences to the baseline estimations.

[14]The state fixed effect accounts for heterogeneity in institutional factors between German states. The interaction term is added to account for the heterogeneity in months spent in school between oldest children of different households and states. As mentioned earlier, in the SOEP data survey months differ both between households h and between years t within households. As such, even if two households reside in the same state and their oldest child was enrolled in the same year, depending on survey month their oldest children might have spent different numbers of months in schooling at time t.

[15]*Early* school enrollment here refers to children who are eligible for school enrollment the year they turn six; not enrollment which has been brought forward despite the child only being eligible for enrollment the following year. I.e. it refers to children who were enrolled one year earlier than their peers of comparable age who were not eligible for enrollment the year they turned six; not children who were enrolled early in their lifetime.

in the next year, such that

$$P(D_{ih} = 1|x_h) = \begin{cases} g_1(x_h) & \text{if } x_h < \ x_h = 0 \\ g_0(x_h) & \text{if } x_h > \ x_h = 0 \end{cases} \tag{3}$$

where $g_1(x_h = 0) \neq \ g_0(x_h = 0)$. Functions $g_0(x_h)$ and $g_1(x_h)$ differ. $g_1(x_h = 0) > g_0(x_h = 0)$ is assumed, such that $x_h < \ x_0$ makes treatment more likely. The dummy variable $E_h = 1$ for $x_h < \ x_h = 0$. It indicates the point where treatment D_{ih} dependent on x_h is discontinuous. Using only E_h as an instrument for treatment status D_{ih} leads to the first stage

$$\widehat{D_{ih}} = \alpha_{41} + \pi E_h + \epsilon_{41,ih} \tag{4}$$

where π is the first stage effect of E_h. The fitted values for D_{ih} from the first stage are then plugged into the second stage estimation

$$\text{Integration Outcome}_{iht} = \alpha_{42} + \eta x_h + \rho\widehat{D_{ih}} + \epsilon_{42,ih} \tag{5}$$

where ρ is the estimated local average treatment effect (LATE) for compliers in the observed bandwidth around the cutoff. This means it captures the causal effect of the treatment for those who comply with the treatment assignment mechanisms, i.e. those who enroll their child according to state-mandated eligibility, within the observed bandwidth (compare to Angrist and Pischke 2009; Imbens and Angrist 1994). Focusing on a narrow bandwidth around the treatment assigning cutoff has a certain advantage. In Section 2.2 I showed that the parents assigned treatment and those not assigned treatment did not differ much. Any last concerns regarding differences between both groups, i.e. differences in years of education and whether the oldest child was in ECEC before enrollment, can be ruled out once the bandwidth around the cutoff is at ± 4 (4 months left and right to the cutoff included), as Table D in the Appendix shows. Concerns that the enrolled children differ in their age (given that children who are eligible for enrollment at age six are on average a bit older than those who are not) can also be ruled out by decreasing the observed bandwidth. Hence I will adjust the bandwidth around the enrollment cutoff accordingly in the following estimations in Section 3 to reduce the risk of potential confounders.

2.4 Descriptive Analysis

In a first descriptive analysis on some selected integration outcomes I see that treated parents (i.e. parents whose oldest child was eligible for enrollment and enrolled in the year they turned six) have on average better outcomes than parents whose oldest child was not eligible for enrollment and not enrolled in the year they turned six (control). E.g., the top panel of Figure 3 plots a discontinuity at the birth date cutoff in the parental monthly income. Despite overlapping confidence intervals, the monthly income in year

Figure 3: *Discontinuity in Parental Monthly Income and Parental Staying Intentions (in Year 2 After Cutoff)*

Note: Parental monthly income (top panel) and parental staying intentions (bottom panel) in year 2 after initial cutoff by treatment status (not treated = red line, treated = blue line). Dots indicate whether the oldest child in the household was enrolled in year 0 (not enrolled in year 0 = red dots, enrolled in year 0 = blue dots). Vertical and horizontal noise added to avoid overplotting.

2 is statistically different between the treated and control group.[16] In year 2 the treated group has an average monthly net income of 1304 Euro while the control group only has 1150 Euro, and the difference is significant (p-value of 0.066).[17] Also, as shown in the bottom panel of Figure 3, the probability to intend to stay in Germany indefinitely is larger within the treatment group. 78 % of the treated group intend to stay in Germany in year 2, compared to 72 % of the control group, and the difference is significant (p-value of 0.064).

Since there is non-compliance with the treatment assignment in the data, those purely

[16]The 95 % confidence intervals for two means can overlap despite the two means being statistically significantly different from one another (Austin and Hux 2002).

[17]If not stated otherwise, p-values of tests of differences in means refer to p-values obtained from unpaired two-sample Wilcoxon tests with two-sided alternative and potentially unequal variance in both samples.

descriptive results could be partially driven by self-selection into treatment (e.g. parents who integrate more easily to begin with enroll their children according to their eligibility). Those concerns will be addresses by applying the estimation strategies introduced earlier in Section 2.3.

3 Results

First, estimation results regarding parents' labor market outcomes will be presented in the following. In Section 3.1 follows an analysis of potential channels through which schooling of the oldest child in the household affects parents' integration outcomes. In addition, a comparison to outcomes among a sample of parents born in Germany is drawn in Section 3.2. Lastly, Section 3.3 discusses limitations of the data and analysis and the external validity of the results.

Table 2: *Estimates of Months of Schooling and Enrollment Timing on Parental Monthly Income*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.81^{***}			
	(0.46)			
Months in school		15.99^{***}		
		(2.80)		
Treated (compliers)			303.26^{**}	472.81^{***}
			(148.91)	(125.97)
R^2	0.65	0.90	–	–
Adj. R^2	0.53	0.87	–	–
Num. obs.	2712	2712	577	491
Num. individuals	678	678	577	491
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental monthly income in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental monthly income in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

The estimated effects on parental monthly net income are shown in Table 2. First, I estimate the effect of one additional month of schooling of the oldest child in a 2SLS. In the first stage the months of schooling are instrumented by the enrollment eligibility in year 0 (see Equation 1 in Section 2.3). Column (1) shows that in the first stage enrollment eligibility in year 0 strongly predicts the months of schooling in year 2. A Wald test comparing the model including and excluding the instrument proves instrument relevance (F-statistic of the first stage is highly significant with p-value $< 2.2e^{-16}$). In the second stage the fitted values of stage 1 are plugged into a regression which estimates the causal effect of one additional month of schooling of the oldest child on parental monthly income (see Equation 2 in Section 2.3). As Column (2) shows, one additional month of schooling of the oldest child increases the parental monthly net income by around 16 Euro and the effect is significant on the 1 % level. Second, I estimate the effect of school enrollment of the oldest child in the household on parental monthly income via Fuzzy RDD, as described in Equation 4 and 5 in Section 2.3. The LATE for compliers within a bandwidth of ± 5 months around the enrollment cutoff is roughly an additional 303 Euro monthly parental income. Within a smaller bandwidth of ± 4 months the LATE is even larger with around 473 Euro additional parental monthly income and significant on the 1 % level.[18]

One additional month of schooling for the oldest child increases the parents' probability to be regularly employed, as Table 3 shows. Each additional month brings 0.01 higher probability to be in regular employment. Parents whose oldest child was enrolled one year earlier have a 0.21 to 0.31 higher probability to be employed in year 2 (within a bandwidth of ± 5 and ± 4 around the enrollment cutoff, respectively). All coefficients are significant at the 1 % level.

Both parental employment and monthly income are positively affected by children's schooling. With the improved employment and income situation also comes a better outlook on personal finances. As the fuzzy RDD results of Table 4 show, parents whose oldest child was enrolled one year earlier are 0.21 to 0.29 points less worried about their personal financial situation on a 3-point scale (within a bandwidth of ± 5 and ± 4 around the enrollment cutoff, respectively).

3.1 Channel Analysis

Given the observed positive effects on the labor market outcomes, the question arises which mechanisms underlie them. Specifically, I want to shed light on two potential channels which might drive my results – *disposable time* and *exposure* to the German language and culture. For the first the assumption is that upon enrollment the hours the oldest child spends at school every weekday becomes disposable to the parent(s), increasing their daily number of disposable hours. They can use this gained time to actively work on their integration (e.g. through language courses) or participate in the labor market (compare with Müller and Wrohlich (2020) who makes the same argument regarding early childhood education and care (ECEC)). The latter relates to the idea that migrant parents are likely to profit from the exposure to the German language and culture

[18] All following result tables will be build and interpreted like Table 2, showing the estimation results for 2SLS and Fuzzy RDD.

Table 3: *Estimates of Months of Schooling and Enrollment Timing on Parental Employment*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	± 5 months	± 4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.81^{***}			
	(0.46)			
Months in school		0.01^{***}		
		(0.00)		
Treated (compliers)			0.21^{***}	0.31^{***}
			(0.08)	(0.09)
R^2	0.65	0.77	–	–
Adj. R^2	0.53	0.69	–	–
Num. obs.	2712	2712	577	491
Num. individuals	678	678	577	491
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State $\times$ interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental employment in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental employment in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

the school attendance of their child entails. Yet labor market participation also brings more exposure to German language and culture through direct contacts to coworkers and supervisors. And contacts to natives have been shown to foster assimilation (Facchini et al. 2015; Martinovic et al. 2009, 2015). Hence, the two channels are not easily disentangled.

School enrollment considerably increases the daily disposable time of parents. Indeed, the hours per weekday spent with childcare decrease from an average of 4.86 to 4.42 after school enrollment of the oldest child, which is a significant difference (p-value of 0.057).[19]

This decrease in childcare hours differs considerably between genders. For the mothers in the sample the average number of childcare hours per weekday decreases from 6.97 to 6.33 and the difference is statistically significant (p-value of 0.027). On the contrary, the difference for fathers with only 0.22 (decrease from 2.38 to 2.16 hours on

[19]Since all oldest children, independent of their enrollment eligibility in year 0, will not be enrolled in year -1 and be enrolled in school in year 2, I will here and in the following analysis compare year -1 and year 2 averages.

Table 4: *Estimates of Months of Schooling and Enrollment Timing on Parental Worries about Personal Finances*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.76^{***}			
	(0.45)			
Months in school		0.00		
		(0.00)		
Treated			-0.21^{**}	-0.29^{***}
			(0.09)	(0.09)
R^2	0.65	0.61	–	–
Adj. R^2	0.57	0.47	–	–
Num. obs.	2596	2596	549	469
Num. individuals	649	649	549	469
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental worries about personal finances in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental worries about personal finances in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

average) is much smaller and statistically not significant (p-value of 0.183).[20]

Clearly, the increase in disposable time for both parents is not nearly proportional to the child's time spent at school – assuming a 5 to 6 hours school day during the first and second grades. Parents have to bring and pick up their children from school, prepare lunches, help with homework, and keep contact to teachers and administrators (e.g. via parents' evenings). Also, parents often have more than one child – in the migrant sample 80 % of households have 2 children or more. Since I analyze the school enrollment of the oldest child, potential younger children in the household are not in

[20]In general mothers carry the main burden of childcare. Over all observed years they spend an average of 6.79 hours per weekday with childcare (compared to only 2.27 hours the fathers spend). Since the regularly employed individual in the migrant sample spends an average of 7.95 hours per weekday at work, this is almost equivalent to full-time employment. Do their childcare responsibilities mean that mothers spend more time with children and less time at work overall? In the sample roughly 39 % of mothers are regularly employed and spend an average of 12.52 hours per week at work (or 2.50 hours per day if I assume a 5 day work week). Fathers, on the other hand, have a regular employment share of 86 % and spend an average of 36.20 hours per week at work (7.24 hours per day).

Table 5: *Estimates of Months of Schooling and Enrollment Timing on Parental Childcare Hours per Day*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.81^{***}			
	(0.46)			
Months in school		-0.05^{***}		
		(0.01)		
Treated (compliers)			-3.05^{***}	-3.43^{***}
			(0.47)	(0.60)
R^2	0.65	0.74	–	–
Adj. R^2	0.53	0.65	–	–
Num. obs.	2712	2712	577	491
Num. individuals	678	678	577	491
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental childcare hours spent per day in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental childcare hours spent per day in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

school, yet. Even though the oldest child spends a lot of time at school daily, younger siblings still need childcare. Apart from this, parents whose children have attended early childhood education and care facilities before primary school enrollment gain less or no additional time at all upon school enrollment.[21] All of these factors lead to the rather limited decrease in childcare hours upon enrollment. Nonetheless, the hours spent with childcare daily decrease, and they decrease more strongly for the treated parents. As the 2SLS estimate in Table 5 shows, one additional month of schooling reduces the daily time spent with childcare by 0.05 hours on average. With regard to the effect of early school enrollment, parents whose oldest child was enrolled one year earlier spent 3.05 to 3.43 hours less with childcare per day (within a bandwidth of ± 5 and ± 4 around the enrollment cutoff, respectively).

Parents can use this additional disposable time during the weekday to enter the labor market by taking up regular employment or increase their working hours if they were already employed. If the time gained by reduced childcare hours would perfectly translate

[21] This is controlled for in all regressions.

Table 6: *Estimates of Months of Schooling and Enrollment Timing on Parental Working Hours*

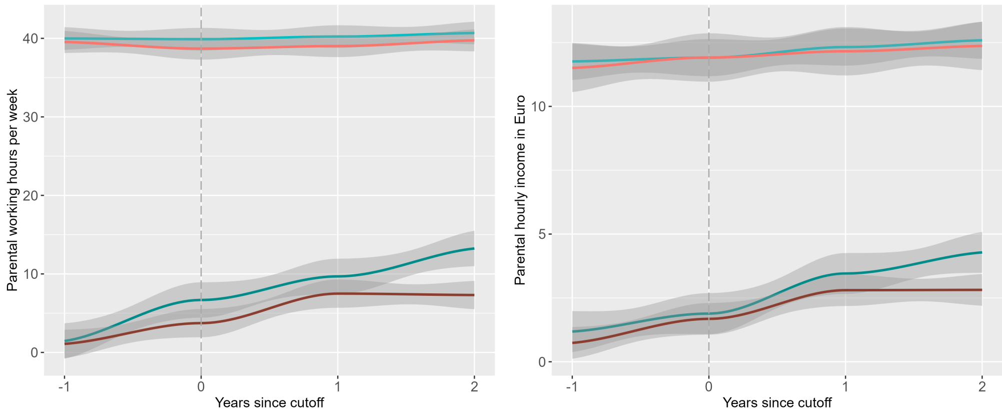
	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.81^{***}			
	(0.46)			
Months in school		0.26^{***}		
		(0.06)		
Treated (compliers)			5.12^{*}	7.75^{**}
			(2.86)	(3.44)
R^2	0.65	0.82	–	–
Adj. R^2	0.53	0.76	–	–
Num. obs.	2712	2712	577	491
Num. individuals	678	678	577	491
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental working hours in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental working hours in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

into increased working hours, the effect of schooling of the oldest child on labor market outcomes would be driven fully by a time effect. Yet, among the treated parents the increase in working hours exceeds the hours gained through less childcare, as Table 6 shows. One additional month of schooling increases the parental working hours per week by 0.26 hours, far exceeding the reduce in childcare hours of 0.05 hours shown in Table 5. Parents whose oldest child was enrolled one year earlier had on average 5.12 to 7.75 more weekly working hours (within a bandwidth of ± 5 and ± 4 around the enrollment cutoff, respectively). This also exceeds the disposable time gained.

As seen in Table 3, there is a significant surge in employment probability among the treated parents. Does this mean that the increase in working hours is mainly driven by job uptake? While working hours among the formerly employed stay largely constant, for the formerly unemployed they increase, which is stronger among the treated parents (see Figure 4). This could be because parents who were regularly employed before school enrollment of their oldest child have already hit full-time employment (on average 39.8 hours per week). As a result, there is not much possibility to increase labor market participation for them. As such, the increase in working hours is clearly due to job uptakes after school enrollment, and there is vast difference between treated and control. Prior to treatment, shares of individuals who are not in regular employment are comparable between treated and control (41 % in each). Post-treatment, 2.1 % of the complete control group and 6.8 % of the treated have taken up regular employment. This is also underlined by the positive effect of the oldest child's schooling on parental employment probability among the formerly unemployed shown in Table E in the Appendix.

Figure 4: *Means in Parental Working Hours per Week and Parental Hourly Income Over Years*



Note: Parental parental working hours per week (left panel) and parental hourly income (right panel) over the years by treatment status and former employment (not treated = red lines, treated = blue lines, formerly employed = lighter lines, formerly unemployed = darker lines). 95 % confidence intervals.

But can those increased working hours fully explain the increase in individual income that was shown in Table 2? Among the treated parents the monthly net income increases from around 1126 to 1304 Euro and the difference is highly significant ($p < 0.01$). This depicts an increase of roundly 178 Euro per month, while the average working hours per month (assuming a month with 4.35 working weeks) have increased by 12.18 hours among the treated parents. Given the pre-treatment average hourly income among the treated of 7.00 Euro, the increase in monthly working hours should have been 25.36 to explain the increase in income; a value which is more than double the actual increase. The increase in individual income cannot be explained by increase in working hours alone. Hence, in the next step I analyze the change in hourly income.

The estimation in Table 7 shows that one additional month of schooling of the oldest in the household brings a net income increase of 0.12 Euro per parental working hour. Parents whose oldest child was enrolled one year earlier have on average 3.62 to 4.35 Euro more hourly income (within a bandwidth of ± 5 and ± 4 around the enrollment cutoff, respectively). Subsampling reveals that for those who were in regular employment prior to enrollment of their oldest child, hourly income stays mostly constant and there is no significant difference between treated and not treated (see lighter lines in right panel of Figure 4). For the formerly unemployed, however, hourly income increases and this is stronger among the treated parents (see darker lines in right panel of Figure 4). Hence, like the increase in working hours, the increase in hourly income seems mainly driven by job uptake of the formerly unemployed.

Table 7: *Estimates of Months of Schooling and Enrollment Timing on Parental Hourly Income*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.81^{***}			
	(0.46)			
Months in school		0.12^{***}		
		(0.02)		
Treated (compliers)			3.62^{***}	4.35^{***}
			(1.37)	(1.22)
R^2	0.65	0.78	–	–
Adj. R^2	0.53	0.71	–	–
Num. obs.	2712	2712	577	491
Num. individuals	678	678	577	491
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental hourly income in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental hourly income in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

While the gained disposable time upon school enrollment of the oldest child in the household is limited, the allocation of disposable time throughout the day could be of importance. School attendance frees up time between morning and midday, a time slot that offers good working opportunities as many jobs require attendance during school hours, especially traditional part-time positions. This gives the part of the household which carries the majority of the childcare burden the opportunity to enter the labor market.[22] Not coincidentally, these are mostly the women in the sample.

[22]Parents have to allocate their time between childcare at home and work. Before primary school enrollment childcare options outside the home are either quite limited in their availability and in the time they free up (e.g. communal kindergartens) or come with additional costs (e.g. privately paid kindergartens, day care centers, nannies). So for parents who carry the main childcare burden in the household (i.e. who are not the breadwinners), their income generated due to the time freed up by outside childcare options has to exceed the amount they spend on these outside options to make employment a financially feasible option. This changes upon school entry, which basically constitutes an outside childcare option that is free of charge since there are no school fees at German public schools. Hence, it can then be a financially feasible option to work during school times even for parents with low hourly incomes. Also, more flexibility regarding time and place of work offers opportunities for better positions and higher pay.

Women make up the largest share of those not in regular employment (85.0 %) and have only an average of 11.3 weekly working hours prior to the enrollment of their oldest child (compared to 37.0 hours among men). Taking a closer look at gender subsamples, I find that positive effects of the oldest child's schooling on parental outcomes seem to be largely driven by the women in the sample. As Table G in the Appendix shows, one additional month of schooling of the oldest child brings on average around 17 Euro more monthly income for the mothers. Mothers whose oldest child was enrolled one year earlier had on average around 484 to 581 Euro more monthly income (within a bandwidth of ± 5 and ± 4 around the enrollment cutoff, respectively).[23] The employment probability of mothers increases significantly with their oldest child's schooling (see Table H in the Appendix). As Table I in the Appendix shows, womens' working hours per week increase strongly among the treated. One additional month of schooling of their oldest brings 0.28 more working hours per week. Mothers whose oldest child was enrolled one year earlier had on average 8.64 to 11.72 more weekly working hours (within a bandwidth of ± 5 and ± 4 around the enrollment cutoff, respectively). These increases in working hours are considerably larger in size compared to the whole sample, which is not surprising given the difference of daily childcare hours for women upon enrollment of their oldest child. Table K shows that mothers whose oldest child was enrolled one year earlier spent on average 4.49 to 5.05 less hours per day with childcare (within a bandwidth of ± 5 and ± 4 around the enrollment cutoff, respectively). Each additional month of schooling of their oldest child brings a decrease of 0.08 hours per day spent with childcare for mothers.

Those results are in favor of the argument that for the formerly unemployed time has freed up in the mornings till midday, allowing them to take up jobs (see the substantial effect of early enrollment on treated parents among the formerly unemployed in Table F in the Appendix) – and this applies majorly to the mothers in the sample who are the main carriers of the childcare burden in the household. Consequently, there is strong evidence for a *time effect* of school enrollment. However, since the increase in labor market participation exceeds the gained disposable time, my findings cannot be driven solely by such time effect. In addition, if the outcomes were fully explainable by gained disposable time, parents in households with only one child should have considerably stronger decreases in childcare hours and increases in labor market participation compared to those with several children. Yet, looking at a subsample of parents with only one child in the household, the effects among parents are largely comparable with the whole sample (see Table L to Table P in the Appendix for regression results for parents with only one child). Even with regard to the change in childcare hours, there is not much difference. Though they were less to begin with compared to parents with several children, the daily hours spent with childcare on average decrease by 0.44 hours a day (from 3.68 to 3.28), which is exactly the same amount as the decrease for parents with several children.

Also, I can exploit the heterogeneity in effects between migrants who live in households with German individuals and migrants who live in migrant-only households. All

[23] Tables G to K in the Appendix show the estimation results for labor market outcomes (monthly income, employment, working hours per week, hourly income) as well as childcare hours per day among women.

previous observations include all migrants, independent of whether they live in a household with other migrants or Germans. Now I run the main regressions on a subsample of migrants living in migrant-only households. Results are shown in Table V to Table Z in the Appendix. The decrease in daily time spent with childcare is slightly smaller in the migrant-only sample compared to the whole sample. Despite this, the increase in employment probability and hourly income among the treated is comparably larger in the migrant-only sample: Parents whose oldest child was enrolled one year earlier had on average 0.27 to 0.39 higher employment probability (within a bandwidth of ± 5 and ± 4 around the enrollment cutoff, respectively). Further, parents whose oldest child was enrolled one year earlier had on average 6.10 to 7.51 more hourly income (within a bandwidth of ± 5 and ± 4 around the enrollment cutoff, respectively) – almost double the effects size compared to the mixed households. This is further evidence that the positive effects found are not solely driven by a time channel.

Besides gains in disposable time – that can only to some extent explain the outcomes – which mechanisms lead to improved integration outcomes for parents? When it comes to drivers of language acquisition, some authors, like Chiswick and Miller (2005) and Isphording and Otten (2014), differentiate between three ones: economic incentives, exposure, and individual ability. Those can also be applied more broadly to other assimilation measures. I will not focus on the first or last, since I do not expect differences in economic incentives between parents of children depending on different birth dates (treatment assignment). Similarly, differences in individual ability should be controlled for by the identification strategy and individual fixed effects. This leaves me with exposure as a potential channel through which integration happens.

Upon the enrollment of their oldest child into primary school, parents enter a higher level of exposure to the German language and culture. Direct personal contact to teachers, administrators as well as other children and parents offers the opportunity to build social networks and practice the local language. Further, indirect contact to the culture and language via community meetings (e.g. parents' evenings and school trips) and their children's language acquisition can promote cultural assimilation and language skills (Avitabile et al. 2013; Dustmann 1996). Regular contacts to natives overall have been shown to support assimilation (Facchini et al. 2015; Martinovic et al. 2009, 2015). As such, migrant parents are likely to profit from their children's school enrollment in their whole integration process – and not only regarding labor market outcomes. Integration is a complex, multidimensional process, spanning economic outcomes (Aldashev et al. 2009; Bleakley and Chin 2004; Chiswick and Miller 1995; Dustmann 1994; Dustmann and Van Soest 2001) as well as social and cultural assimilation (Constant and Zimmermann 2008; Danzer and Yaman 2013; Facchini et al. 2015; Gambaro et al. 2021) and well-being (Battisti et al. 2022). Hence, in the following some outcomes not directly related to the labor market are analyzed.

While health might not be understood as a traditional integration outcome in itself, it very well can be a proxy for overall well-being. Table 8 shows that on a self-assessed health scale which runs from 1 (very bad) to 5 (very good), parents whose oldest child was enrolled one year earlier report a 0.70 to 0.86 points higher health status (within a bandwidth of ± 5 and ± 4, respectively).

Table 8: *Estimates of Months of Schooling and Enrollment Timing on Parental Self-Assessed Health*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	± 5 months	± 4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	7.16^{***}			
	(0.48)			
Months in school		0.00		
		(0.00)		
Treated (compliers)			0.70^{***}	0.86^{***}
			(0.21)	(0.17)
R^2	0.66	0.59	–	–
Adj. R^2	0.54	0.45	–	–
Num. obs.	2304	2304	488	419
Num. individuals	576	576	488	419
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State $\times$ interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental self-assessed health in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental self-assessed health in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

When it comes to the intention to stay in Germany indefinitely, parents whose oldest child was enrolled one year earlier have on average a 0.15 to 0.23 higher probability to have permanent staying intentions (within a bandwidth of ± 5 and ± 4, respectively). However, the results are only significant within a bandwidth of ± 4 around the enrollment cutoff (see Table 9).[24]

In addition to the direct contact to teachers, administrators and other parents, schooling also brings indirect contact to the German language. In their everyday lives migrant parents might not need their German language skills too often. But once their oldest child enters school, their child not only learns to write and read the German language, but also learns all other subjects in German, and might need help with homework assignments. Indeed, Table 10 shows that treated parents report better German speaking, reading and writing skills on a scale from 0 (no knowledge) to 4 (very good knowledge). Parents whose

[24] I find no significant effect of the enrollment of the oldest child in the household on the parents' probability of living in government subsidized housing and their overall life satisfaction (not reported here).

Table 9: *Estimates of Months of Schooling and Enrollment Timing on Parental Staying Intentions*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	± 5 months	± 4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.69^{***}			
	(0.46)			
Months in school		0.00		
		(0.00)		
Treated (compliers)			0.15	0.23^{**}
			(0.09)	(0.11)
R^2	0.65	0.73	–	–
Adj. R^2	0.57	0.64	–	–
Num. obs.	2552	2552	544	460
Num. individuals	638	638	544	460
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State $\times$ interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental staying intentions in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental staying intentions in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

oldest child was enrolled one year earlier report significantly higher knowledge: 0.53 to 0.66 points higher speaking skills, 0.78 to 0.85 points higher reading skills, and 0.77 to 0.89 points higher writing skills (within a bandwidth of ± 5 and ± 4, respectively).[25]

Overall, there is some evidence on the role of exposure in everyday life on parents' integration outcomes. Exposure to Germans through personal contact to teachers, administrators, other children and parents offers the opportunity to practice the German language and build social networks. Those are particularly important for job search and promotion opportunities (Aldashev et al. 2009; Bleakley and Chin 2004; Chiswick and Miller 1995; Dustmann 1994; Dustmann and Fabbri 2003; Dustmann and Van Soest 2001). Increased labor market participation in turn creates more exposure to German language and culture through direct contacts to coworkers, supervisors and customers.

[25] It has to be noted that the number of observations is limited due to the periodicity of the language skills questions, which were only included in alternate survey years until 2007 and were not asked in 2012. Thus, the analysis is based on a sample of 437 individuals from 310 households who provided information on their German language skills. This number further decreases by narrowing the bandwidth to ± 5 and ± 4 months around the enrollment cutoff.

Table 10: *Estimates of Enrollment Timing on Parental Self-Assessed German Language Abilities*

	Fuzzy RDD					
	German speaking		German reading		German writing	
	±5 months	±4 months	±5 months	±4 months	±5 months	±4 months
	(1)	(2)	(3)	(4)	(5)	(6)
Treated (compliers)	0.53^{***}	0.66^{***}	0.78^{***}	0.85^{***}	0.77^{***}	0.89^{***}
	(0.19)	(0.21)	(0.18)	(0.19)	(0.24)	(0.25)
Num. obs.	383	320	383	320	383	320
Num. individuals	383	320	383	320	383	320

Note: Fuzzy RDD LATE estimates of enrollment in year 0 on parental self-assessed German language knowledge in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on level of cutoff distance for discontinuity samples. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Thus, the child's school entrance can foster a circle of exposure for the parents, which in turn fosters assimilation (Facchini et al. 2015; Martinovic et al. 2009, 2015). This is especially important for mothers, who have much lower labor market participation rates before school enrollment of their oldest child. Since they weren't in regular employment prior to school enrollment, they have not been subject to the German language and culture – at least not through their job. If they do not maintain regular contact to natives outside of work, they establish regular contact to Germans only upon school enrollment. This can potentially explain the strong labor market effects of the child's school enrollment among formerly unemployed parents and mothers.

3.2 Comparison to German Parents

Another interesting aspect to shed light on is whether schooling of the oldest child also has effects on German parents. For this, I identify German individuals[26] residing in households with an oldest child for whom I have information on their enrollment eligibility and actual enrollment year. As in the migrant sample, I eliminate all individuals with missings in relevant variables. This yields 3137 individuals in 1996 households. Tables Q to U in the Appendix show the same estimations on labor market outcomes and childcare hours as estimated for the migrant sample.

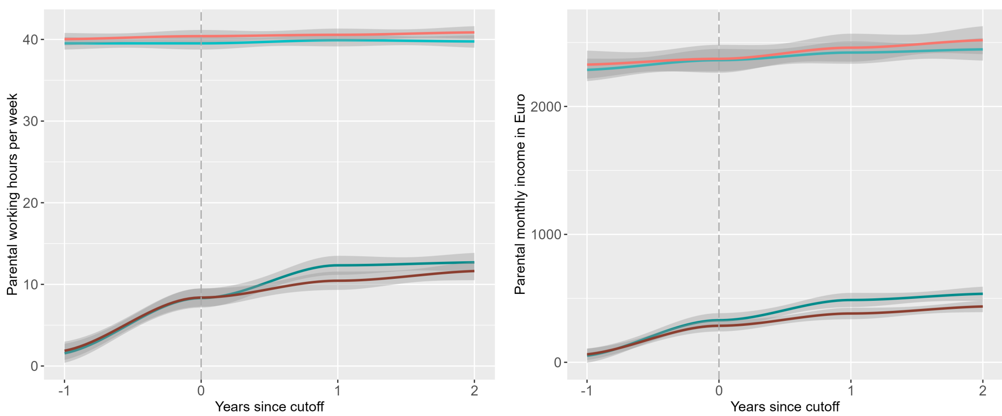
The first stage of the 2SLS regression shows that enrollment eligibility in year 0 strongly predicts the months of schooling in year 2 (see Column (2) of Table Q in the Appendix). One additional month of schooling of the oldest child increases parental monthly income by around 12 Euro. Additionally, it increases parents' working hours by 0.15 hours per week and the hourly income by 0.08 Euro (see Table S and Table T in the Appendix, respectively). Therefore, with regards to the effect of months of schooling the results among Germans are largely comparable to the migrant sample, though smaller in magnitude. A possible explanation for that is that native Germans, contrary to migrants, have easier access to ECEC. Indeed, the share of parents whose oldest child was in

[26]Individuals who were born in Germany.

ECEC before school enrollment is higher among Germans (94.2 %) than among migrants (87.7 %). As such, school entrance of the oldest child in the household is a large positive shock in disposable time for fewer Germans than migrant parents.

Still, like among migrants, the increased labor market participation and returns are largely driven by those who were not in regular employment prior to the school enrollment of their oldest child. And just like among migrants, the vast majority of parents who were not in regular employment before enrollment of their oldest child are women (91.8 % of the formerly unemployed). Among the formerly employed German parents there is not much possibility to increase labor market participation, as they worked on average 39.8 hours per week before school enrollment of their oldest child – which is equivalent to full-time employment. Therefore, the increase in working hours upon school enrollment is due to job uptakes among the formerly unemployed, i.e. mothers. As the lighter lines in the left panel of Figure 5 show, working hours among those who were employed before stay constant. The darker lines show that working hours have increased for those who were not in regular employment in year -1. The right panel of Figure 5 shows that the same pattern applies to monthly income.

Figure 5: *Means in Parental Working Hours per Week and Parental Monthly Net Income Over Years (Germans)*



Note: German sample. Parental working hours per week (left panel) and parental monthly net income (right panel) over the years by treatment status and former employment (not treated = red lines, treated = blue lines, formerly employed = lighter lines, formerly unemployed = darker lines). 95 % confidence intervals.

The gender split of childcare is not substantially different in the German sample compared to the migrant sample. Carrying the main burden of childcare in the household, German mothers spend an average of 7.48 hours per weekday with childcare (compared to the 2.13 hours the fathers spend) – almost equivalent to the 8.01 hours the regularly employed individual in the German sample spends at work per weekday. They reduce their childcare time upon enrollment of their oldest child by 1.04 hours per day, a highly significant difference (p-value $< 2.2e^{-16}$). For German fathers, the reduction in childcare hours is almost negligible (0.03 hours on average) and not significant.

Interestingly, Figure 5 also shows that the effects on income and working hours among the treated parents, though small, already realize in the first year after the initial enrollment. Parents whose oldest child was enrolled one year earlier see no significant effects regarding their labor market outcomes in the second year (see LATE from RDD regressions in Tables Q to T in the Appendix, which are based on year 2 outcomes and are mostly insignificant). It seems that labor market returns upon school enrollment of the oldest child in the household realize faster among German parents compared to migrant parents. Formerly unemployed German parents seem to get into employment rather quickly after the surge in disposable time, while for migrant parents job search seems to take longer. The latter experience labor market returns of increased disposable time only in the second year after the initial enrollment. This is not surprising, as migrant parents can use their gained disposable time to improve their own value on the German labor market and hence employment opportunities, e.g. by attending German language courses and building networks. Then they can enter the labor market with increased employment chances and higher potential wages. In conclusion, since integration is a gradual process, it is expected to take some time to fully manifest in the outcome variables. In addition, in the German sample – though it is much larger and thus even smaller effect sizes should be identified – I cannot identify effects of schooling on parental financial worries or health status (not reported here). All of this evidence suggests that schooling of the oldest child in the household drives integration among migrant parents, which relies on mechanisms beyond the effect of disposable time.

3.3 Threats to Identification and Sensitivity Analysis

In the following some major threats to identification and how they are dealt with, as well as additional robustness checks, will be discussed.

Identification of causal effects in fuzzy RDD approaches relies on several assumptions. First, there should be no manipulation, i.e. individuals should not display sorting on the enrollment cutoff distance. McCrary density test is used to check whether there is bunching of units at the cutoff (McCrary 2008). Under the null hypothesis, the density should be continuous at the cutoff point and under the alternative hypothesis, the density should increase at the cutoff point. The null is rejected (at a p-value of 0.026), so there is some evidence for manipulation. Yet, since children's birth dates are randomly distributed, there is no reason to assume manipulation around the cutoff. A more likely explanation for the observation of bunching at the cutoff is that there are too little observations in the sample to distinguish a discontinuity in the density from noise.

Second, individuals and households should have parallel trends in outcomes under the absence of treatment. In Section 2.2, I demonstrate that the pre-treatment means of both controls and outcomes are similar between the treatment and control groups, indicating that the pre-treatment trends are parallel.

Third, pre-treatment characteristics that are in expectation not qualitatively affected by the treatment should be invariant to change in treatment assignment. A covariat balance test reveals that there is no observable discontinuous change around the cutoff in the average values of covariates that should not be affected by the treatment assignment, i.e. parents' gender, years since migration, whether they own the house they reside in, whether they are a refugee, and whether they have a permanent residence permit or German citizenship.[27]

Lastly, just as there should not be any effects on those covariates, there should also not be effects on the outcomes of interest at arbitrarily chosen cutoffs. Following Imbens and Lemieux (2008), I look at one side of the discontinuity and take the median value of the running variable (distance to the enrollment cutoff) in this selection. Looking at the right side of the discontinuity and using the median of 3 as an arbitrarily chosen cutoff, I find no sign of discontinuity at this point in any of the outcomes of interest (parental monthly income, employment probability, working hours per week, hourly income as well as childcare hours per day, worries about personal finances, health status, staying intention or German language skills). The same holds when I look at the left side of the discontinuity and use the median of -3 as an arbitrarily chosen cutoff.[28]

In addition, I run an additional placebo test in which I assign the treatment randomly, given the same probability to be assigned the treatment as under the birthdate cutoff rule. To check whether this random treatment assignment can predict the months spent in school by the oldest child, I regress the random treatment assignment on the months spent in school by the oldest child as an outcome. This is the same set-up as the first stage of the 2SLS instrumental variable approach (Equation 1), except now the treatment is not assigned based on the birthday of the oldest child with respect to the enrollment cutoff, but randomly. Figure 6 plots the distribution of the random treatment assignment coefficient for $N = 10000$ repetitions. The coefficient is normally distributed around 0, which means in most cases of random treatment assignment the null hypothesis that the randomly assigned treatment has no effect on the months the oldest child spent in school cannot be rejected. This underlines the validity of the actual treatment assignment as an instrument for months of schooling of the oldest child.

The use of self-reported measures (e.g. worries about personal finances and self-assessed language skills) might introduce unobserved heterogeneity between individuals. However, the panel data structure and the introduction of individual fixed effects in the estimations should account for varying reporting styles and personality traits across respondents (Angelini et al. 2015).[29]

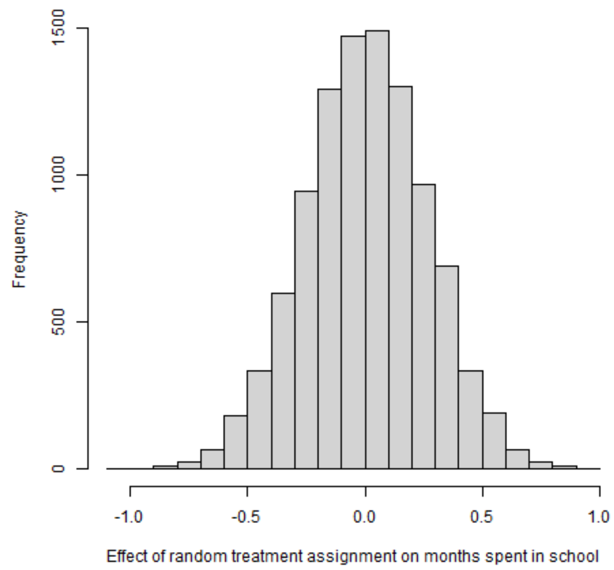
Another potential threat to identification is the timing of the survey interviews. As mentioned in Section 2.3, the yearly SOEP interviews are conducted during all 12 months of the year. The actual school enrollment, depending on the federal state, only happens

[27] In the long run, residence permits and naturalization can be an integration output which is potentially affected by children's schooling. However, in the short run I expect no effects on these outcomes since changes in residence status and acquiring citizenship take a lot of time.

[28] The only exception is hourly income for which I find marginally significant results on the left side of the cutoff, but not the right.

[29] Also, most studies on the effect of school enrollment timing on children's outcomes, like test results and lifetime earnings, are potentially biased by age-at-test effects since children who were enrolled earlier are of younger age when they are tested for their academic achievements, and vice versa. This, however, is not a concern in this study as it focuses on the outcomes of parents in the household rather than those of the children.

Figure 6: *Distribution of Random Treatment Assignment Coefficient on Months Spent in School (N=10000)*



Note: Distribution of linear regression coefficient of random treatment assignment on oldest child's months spent in school. $N = 10000$ repetitions of random treatment assignment, given same assignment probability as under birthdate cutoff rule.

between July and September, though. Hence, I have to ensure that when I measure results post-treatment, the treated have been subject to at least one year of schooling of their oldest child. In addition, it is possible that households are surveyed in the cutoff year 0 after the actual school entry of their child (for example if the oldest child was eligible for enrollment in year 0 and resided in a state where school started in August but the household was only interviewed in November). To address this, I define the year before the enrollment cutoff (year -1) as the pre-treatment period, and use the second year after enrollment cutoff (year 2) as the post-treatment period. In addition, I introduce an interaction term between the state of residence and interview month (see Section 2.3).

One additional concern is that there is not too much difference in disposable time and exposure for parents upon enrollment of the oldest child when they have visited ECEC facilities before enrollment. Besides controlling for ECEC of the oldest in the baseline regressions, I run the baseline regression for a subsample of only those households whose oldest child were in ECEC before entering school. Since results are vastly similiar in direction and magnitude there is no evidence that ECEC plays a large role in diminishing the effect of schooling.[30]

Another concern is that migrant parents might have a stronger incentive to stay in Germany and integrate themselves once their oldest child has entered school, driving in part the positive effects. Migrant parents who did not integrate well to begin with, on the other hand, could potentially postpone the school enrollment of their oldest child and

[30] Results are available from the author upon request.

emigrate from Germany before their child enters school. Though this poses a serious threat to identification of effects, in the data there is no evidence for this. Intentions to stay in Germany are not significantly different between parents who comply with the treatment assignment (i.e. parents who enroll their child according to eligibility) and parents who do not comply (i.e. parents who bring forward of postpone the enrollment of their child), as Table C in the Appendix shows.

Lastly, the external validity of the results is limited. Though the SOEP is a German-wide representative survey, the data only observes migrants in Germany, and the sample shrinkage leaves only a rather limited number of observations, especially in subsample analyses. Also, only two years after school enrollment are observed. Hence, estimated effects within those years cannot easily be generalized to a larger time frame. I.e. the estimated causal effect of one additional month of schooling of the oldest child in the household on parental integration cannot be extrapolated to an arbitrary number of years after initial enrollment exceeding the observed 2 years. With regards to the timing of school enrollment, the LATE estimated via RDD approach can only be applicable to parents whose children are born close to the enrollment cutoff. This limits the extent in how far the presented results can be generalized to other migrants, more years of observation and other countries. Despite those limitations, the analysis produces interesting first insights on the effect of schooling and school enrollment timing on migrant parents' integration outcomes.

4 Conclusion

In this paper, I exploit age-at-enrollment policies in 16 German states as exogenous source of variation to examine the effect of schooling of the oldest child in the household on parental integration. For this, I link administrative records on primary school enrollment cutoff dates with micro data from the German Socioeconomic Panel. Via a fuzzy regression discontinuity design around the school enrollment cutoff, I estimate the effect the early school entry of the oldest child in the household has on parental integration outcomes. Via an instrumental variable approach, I estimate the effect one additional month of schooling of the oldest child has on the parents' integration outcomes.

I find that the schooling of the oldest child in the household positively affects parental labor market outcomes. It increases labor market participation, parental monthly income and hourly wages. These effects are especially strong among the formerly unemployed and those who carry the main burden of childcare in the household, i.e. the mothers. Apart from labor market outcomes, I find positive effects of the oldest child's schooling on parental health status, staying intentions and self-assessed German language skills in speaking, reading and writing. My results are robust to various robustness checks, and not driven by self-selection into school entry due to parental choice to deviate from the state-mandated enrollment eligibility.

An analysis of potential channels reveals that both gained disposable time and exposure to the German language and culture play a role in shaping integration outcomes.

Schooling not only opens up time for migrant parents to spend at work, but also boosts their overall labor market outcomes, and language skills. Those results contribute to our understanding in how far direct and indirect exposure to the German language and culture via compulsory schooling hold the potential to enhance the integration of migrant parents.

Appendix

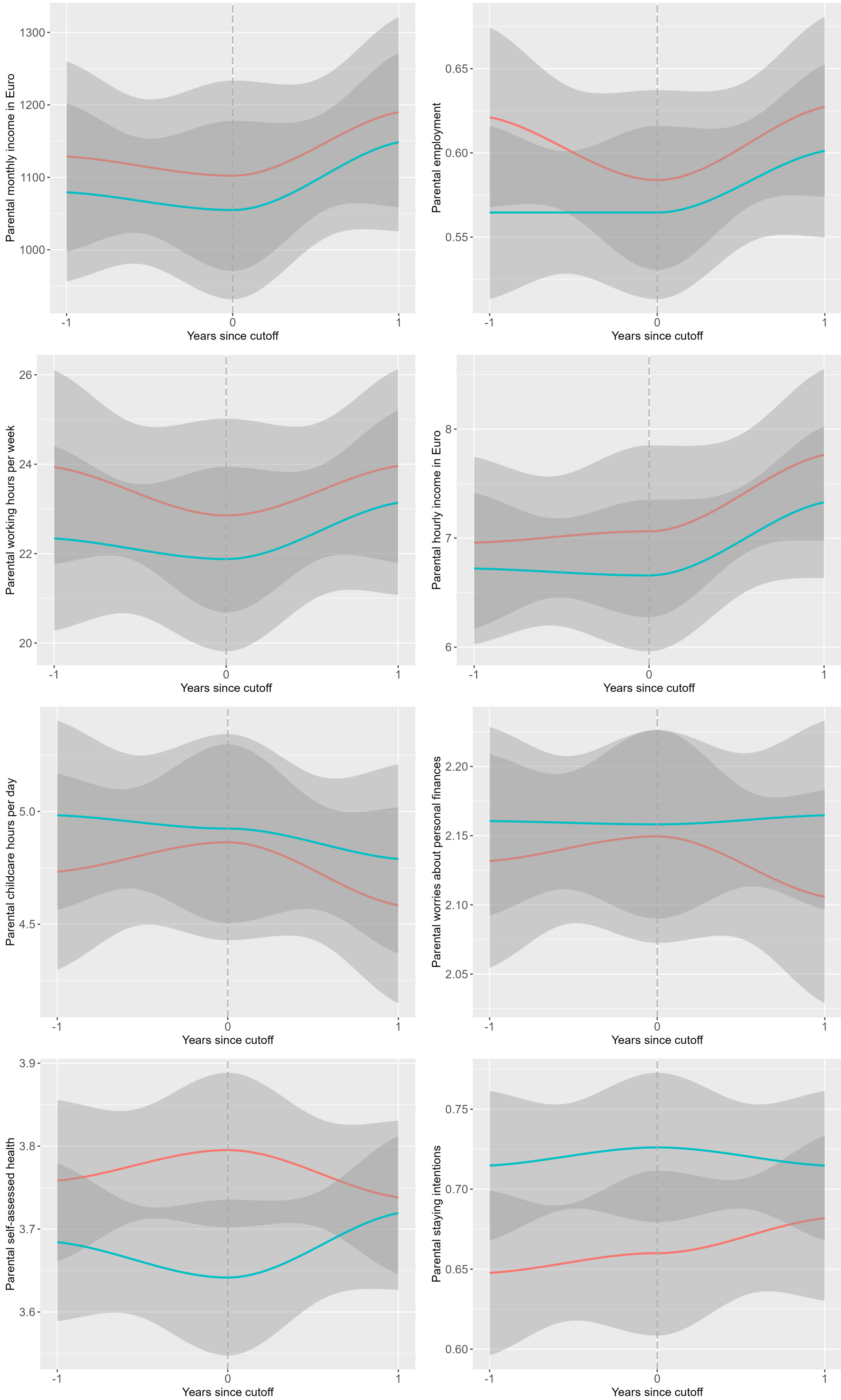
Table A: *Sample Shrinkage due to Missings*

Step	Action	Observations	Individuals	Households
1	Identify adult migrants with children of enrollment age	21573	2758	1643
2	Identify observations with complete records on enrollment	15313	1897	1137
3	Remove observations with < 4 years of interviews around cutoff	4244	1062	682
4	Remove missings: Interview month	3556	889	592
5	Remove missings: Parental characteristics	3192	798	543
5	Remove missings: Parental employment	3192	798	543
5	Remove missings: Parental monthly income	2972	743	503
6	Remove missings: Parental working hours per week	2852	713	494
7	Remove missings: Parental childcare hours per week	2712	678	473

Table B: *Variables Description*

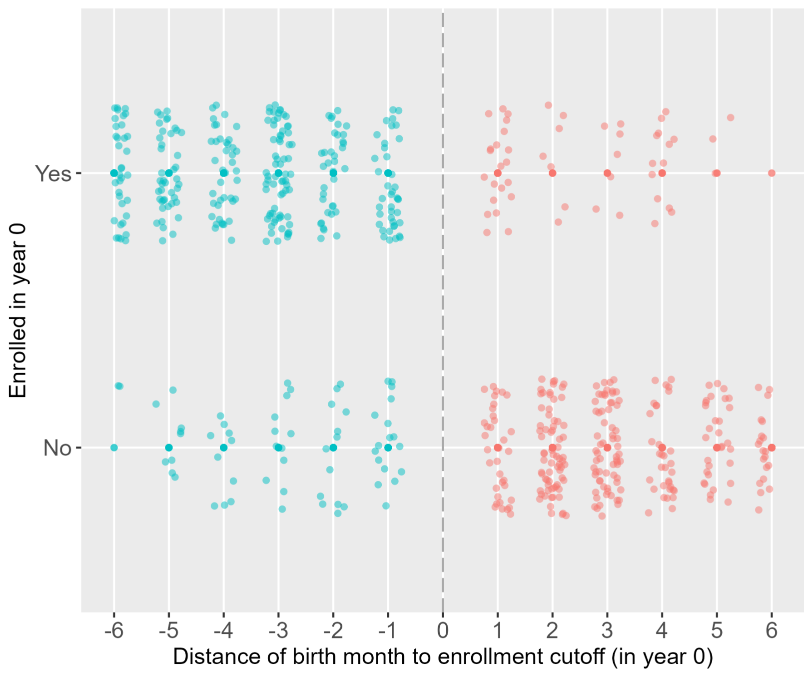
Variable	Type	Description
Eligible for enrollment	Binary	Oldest child was eligible for school enrollment in the year they turned six years old (*treatment group*) (*Ref* = *not eligible (control group)*).
Months in school	Numerical	Number of months spent in school by oldest child since enrollment.
Age	Numerical	Age in years.
Female	Binary	Female gender (*Ref* = *male*).
Years of education	Numerical	Number of years spent in formal education.
Currently in parental leave	Binary	Currently in maternity or paternity leave (*Ref* = *not in parental leave*).
Owner of housing	Binary	Owner of current dwelling (*Ref* = *not owner*).
Refugee	Binary	Status as a refugee (*Ref* = *no refugee*).
Years since migration	Numerical	Years spent in Germany since initial migration.
Number of children	Numerical	Number of children (under 18 years old) in household.
Younger children in ECEC	Binary	Younger children have spent time in any kind of early childhood education and care facilities (e.g. kindergarten) (*Ref* = *younger children not in ECEC*).
Oldest in ECEC before enrollment	Binary	Oldest child has spent time in any kind of early childhood education and care facilities (e.g. kindergarten) before school enrollment (*Ref* = *oldest child not in ECEC before enrollment*).
State	Categorical	One of 16 current states of living in Germany.
Interview month	Categorical	Month in which interview was conducted in given year.
German language skills: speaking / writing / reading	Numerical	Self-assessed ability in speaking / writing / reading German on a scale from 0 (no knowledge) to 4 (very good).
Parental monthly income	Numerical	Monthly income in Euro earned by individual after taxes and social security contributions, adjusted for inflation.
Parental employment	Binary	Current regular employment in paid occupation or in education (*Ref* = *not regularly employed*).
Parental working hours per week	Numerical	Average actual working hours in paid employment per week.
Parental hourly income	Numerical	Monthly income in Euro earned by individual after taxes and social security contributions, adjusted for inflation, divided by the actual average working hours per month (assuming a month with 4.35 working weeks).
Parental worries about personal finances	Numerical	Worries about personal finances on a scale from 1 (not worried) to 3 (strongly worried).
Parental staying intentions	Binary	Intention to stay in Germany indefinitely (*Ref* = *intention to stay only for several years*).
Parental health	Numerical	Parental self-assessed current health status on a scale from 1 (bad) to 5 (very good).

Figure A: *Means in Parental Integration Outcomes Over Years*



Note: Plot of outcome means in years -1, 0 and 1 from cutoff by eligibility for enrollment in year 0 (no = red, yes = blue). 95% confidence intervals.

Figure B: *Actual Enrollment in Year 0 (Treatment Compliance)*



Note: Actual enrollment in year 0 by eligibility for enrollment in year 0 (no = red, yes = blue). Vertical and horizontal noise added to avoid overplotting.

Table C: *Pre-Treatment Differences in Parental Characteristics and Integration Outcomes between Parents who Enrolled their Children According to Eligibility and Parents who Brought Forward or Postponed Enrollment*

	Enrolled according to eligibility		Difference
	Yes	*No*	
Parental characteristics			
Age	32.8	31.8	−1.0
Female (%)	53.8	55.5	1.7
Years of education	10.7	11.0	0.3*
Currently in parental leave (%)	10.4	8.6	−1.8
Owner of housing (%)	25.1	20.3	−4.8
Refugee (%)	5.5	7.0	1.5
Years since migration	15.6	13.5	−2.1**
Number of children	1.7	1.7	0.0
Younger children in ECEC (%)	14.7	14.1	−0.6
Oldest in ECEC before enrollment (%)	88.5	85.9	2.6
Parental integration outcomes			
Monthly parental income (Euro)	1,140.5	940.8	−200.5**
Currently employed (%)	50.8	61.1	10.3
Working hours per week	23.6	20.8	−2.8
Hourly net wage (Euro)	7.0	6.0	−1.0
Childcare hours per day	4.8	5.3	0.5
Worried about own finances (scale 1-3)	2.2	2.1	−0.1
Staying intention (%)	73.8	72.1	−1.7
Number of observations	550	128	678

Note: Means 1 year before initial enrollment cutoff. Unpaired two-sample Wilcoxon tests for differences in variables between groups. For detailed variable description see Table B. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table D: *Differences in Parental Characteristics and Integration Outcomes between Parents whose Oldest Child was Eligible and Not Eligible for Enrollment the Year They Turned Six (Bandwidth* ± 4*)*

	Not eligible *Control*	Eligible *Treatment*	Difference
Parental characteristics			
Age	32.6	32.2	-0.4
Female (%)	52.4	52.3	-0.1
Years of education	10.7	11.0	0.3
Currently in parental leave (%)	9.1	10.1	1.0
Owner of housing (%)	24.8	20.3	-4.5
Refugee (%)	5.1	7.6	2.5
Years since migration	15.4	15.1	-0.3
Number of children	1.6	1.7	0.1
Younger children in ECEC (%)	13.0	14.8	1.8
Oldest in ECEC before enrollment (%)	89.0	91.1	2.1
Parental integration outcomes			
Monthly parental income (Euro)	$1,172.8$	$1,094.2$	-78.6
Currently employed (%)	63.8	57.0	-6.8
Working hours per week	24.7	22.9	-2.8
Hourly net wage (Euro)	7.3	6.7	-0.6
Childcare hours per day	4.7	4.9	0.2
Worried about own finances (scale 1-3)	2.2	2.1	-0.1
Staying intention (%)	76.4	71.1	-5.3
Number of observations	254	237	491

Note: Means 1 year before initial enrollment cutoff. Unpaired two-sample Wilcoxon tests with potential unequal variance in both samples for differences in variables between groups. For detailed variable description see Table B. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table E: *Estimates of Months of Schooling and Enrollment Timing on Parental Employment for Formerly Unemployed*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.46^{***}			
	(0.66)			
Months in school		0.02^{***}		
		(0.00)		
Treated (compliers)			0.15^{*}	0.24^{**}
			(0.09)	(0.10)
R^2	0.65	0.54	–	–
Adj. R^2	0.52	0.37	–	–
Num. obs.	1108	1108	232	194
Num. individuals	277	277	232	194
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental employment in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental employment in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table F: *Estimates of Months of Schooling and Enrollment Timing on Parental Childcare Hours per Day for Formerly Unemployed*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	$\pm$5 months	$\pm$4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.46^{***}			
	(0.66)			
Months in school		-0.08^{***}		
		(0.02)		
Treated (compliers)			-3.51^{***}	-4.21^{***}
			(0.81)	(0.87)
R^2	0.65	0.67	–	–
Adj. R^2	0.52	0.54	–	–
Num. obs.	1108	1108	232	194
Num. individuals	277	277	232	194
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental childcare hours spent per day in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental childcare hours spent per day in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table G: *Estimates of Months of Schooling and Enrollment Timing on Parental Monthly Income for Women*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.90^{***}			
	(0.49)			
Months in school		16.64^{***}		
		(2.99)		
Treated (compliers)			484.40^{***}	580.88^{***}
			(153.03)	(147.04)
R^2	0.66	0.84	–	–
Adj. R^2	0.53	0.79	–	–
Num. obs.	1468	1468	309	257
Num. individuals	367	367	309	257
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental monthly income in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental monthly income in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table H: *Estimates of Months of Schooling and Enrollment Timing on Parental Employment for Women*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.90^{***}			
	(0.49)			
Months in school		0.01^{***}		
		(0.00)		
Treated (compliers)			0.35^{***}	0.48^{***}
			(0.13)	(0.11)
R^2	0.66	0.72	–	–
Adj. R^2	0.53	0.62	–	–
Num. obs.	1468	1468	309	257
Num. individuals	367	367	309	257
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental employment in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental employment in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table I: *Estimates of Months of Schooling and Enrollment Timing on Parental Working Hours for Women*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	$\pm$5 months	$\pm$4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.90^{***}			
	(0.49)			
Months in school		0.29^{***}		
		(0.08)		
Treated (compliers)			8.64^{***}	11.72^{***}
			(3.31)	(3.65)
R^2	0.66	0.77	–	–
Adj. R^2	0.53	0.69	–	–
Num. obs.	1468	1468	309	257
Num. individuals	367	367	309	257
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State $\times$ interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental working hours in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental working hours in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table J: *Estimates of Months of Schooling and Enrollment Timing on Parental Hourly Income for Women*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.90^{***}			
	(0.49)			
Months in school		0.14^{***}		
		(0.03)		
Treated (compliers)			2.94^{***}	2.69^{***}
			(1.03)	(0.97)
R^2	0.66	0.71	–	–
Adj. R^2	0.53	0.61	–	–
Num. obs.	1468	1468	309	257
Num. individuals	367	367	309	257
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental hourly income in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental hourly income in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table K: *Estimates of Months of Schooling and Enrollment Timing on Parental Childcare Hours per Day for Women*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.90^{***}			
	(0.49)			
Months in school		-0.08^{***}		
		(0.02)		
Treated (compliers)			-4.49^{***}	-5.05^{***}
			(0.41)	(0.43)
R^2	0.66	0.68	–	–
Adj. R^2	0.53	0.56	–	–
Num. obs.	1468	1468	309	257
Num. individuals	367	367	309	257
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental childcare hours spent per day in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental childcare hours spent per day in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table L: *Estimates of Months of Schooling and Enrollment Timing on Parental Monthly Income for Parents with only One Child*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.67^{***}			
	(1.01)			
Months in school		8.33^{*}		
		(4.35)		
Treated (compliers)			708.51^{**}	585.93^{**}
			(310.82)	(240.41)
R^2	0.65	0.91	–	–
Adj. R^2	0.52	0.88	–	–
Num. obs.	532	532	114	95
Num. individuals	133	133	114	95
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental monthly income in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental monthly income in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table M: *Estimates of Months of Schooling and Enrollment Timing on Parental Employment for Parents with only One Child*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.67^{***}			
	(1.01)			
Months in school		0.00		
		(0.00)		
Treated (compliers)			0.24^{**}	0.28^{***}
			(0.10)	(0.11)
R^2	0.65	0.76	–	–
Adj. R^2	0.52	0.67	–	–
Num. obs.	532	532	114	95
Num. individuals	133	133	114	95
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental employment in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental employment in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table N: *Estimates of Months of Schooling and Enrollment Timing on Parental Working Hours for Parents with only One Child*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.67^{***}			
	(1.01)			
Months in school		0.07		
		(0.11)		
Treated (compliers)			4.46	3.21
			(7.29)	(7.10)
R^2	0.65	0.85	–	–
Adj. R^2	0.52	0.79	–	–
Num. obs.	532	532	114	95
Num. individuals	133	133	114	95
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental working hours in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental working hours in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table O: *Estimates of Months of Schooling and Enrollment Timing on Parental Hourly Income for Parents with only One Child*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.67^{***}			
	(1.01)			
Months in school		0.08^{*}		
		(0.05)		
Treated (compliers)			11.13^{***}	10.79^{***}
			(3.31)	(2.16)
R^2	0.65	0.73	–	–
Adj. R^2	0.52	0.63	–	–
Num. obs.	532	532	114	95
Num. individuals	133	133	114	95
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental hourly income in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental hourly income in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table P: *Estimates of Months of Schooling and Enrollment Timing on Parental Childcare Hours per Day for Parents with only One Child*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.67^{***}			
	(1.01)			
Months in school		-0.05^{**}		
		(0.02)		
Treated (compliers)			-1.45^{**}	-1.33^{*}
			(0.76)	(0.76)
R^2	0.65	0.76	–	–
Adj. R^2	0.52	0.66	–	–
Num. obs.	532	532	114	95
Num. individuals	133	133	114	95
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental childcare hours spent per day in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental childcare hours spent per day in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table Q: *Estimates of Months of Schooling and Enrollment Timing on Parental Monthly Income for Germans*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	7.27^{***}			
	(0.21)			
Months in school		12.10^{***}		
		(1.22)		
Treated (compliers)			178.73^{*}	173.79
			(103.52)	(122.20)
R^2	0.66	0.92	–	–
Adj. R^2	0.55	0.90	–	–
Num. obs.	12548	12548	2665	2197
Num. individuals	3137	3137	2665	2197
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental monthly income in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental monthly income in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table R: *Estimates of Months of Schooling and Enrollment Timing on Parental Employment for Germans*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	7.27^{***}			
	(0.21)			
Months in school		0.00^{***}		
		(0.00)		
Treated (compliers)			0.01	0.01
			(0.03)	(0.03)
R^2	0.66	0.76	–	–
Adj. R^2	0.55	0.68	–	–
Num. obs.	12548	12548	2665	2197
Num. individuals	3137	3137	2665	2197
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental employment in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental employment in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table S: *Estimates of Months of Schooling and Enrollment Timing on Parental Working Hours for Germans*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	7.27^{***}			
	(0.21)			
Months in school		0.15^{***}		
		(0.02)		
Treated (compliers)			0.01	0.93
			(0.74)	(0.61)
R^2	0.66	0.85	–	–
Adj. R^2	0.55	0.80	–	–
Num. obs.	12548	12548	2665	2197
Num. individuals	3137	3137	2665	2197
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental working hours in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental working hours in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table T: *Estimates of Months of Schooling and Enrollment Timing on Parental Hourly Income for Germans*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	7.27^{***}			
	(0.21)			
Months in school		0.08^{***}		
		(0.01)		
Treated (compliers)			0.38	0.47
			(0.87)	(0.89)
R^2	0.66	0.78	–	–
Adj. R^2	0.55	0.70	–	–
Num. obs.	12548	12548	2665	2197
Num. individuals	3137	3137	2665	2197
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental hourly income in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental hourly income in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table U: *Estimates of Months of Schooling and Enrollment Timing on Parental Childcare Hours per Day for Germans*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	7.27^{***}			
	(0.21)			
Months in school		-0.05^{***}		
		(0.01)		
Treated (compliers)			-0.11	0.02
			(0.28)	(0.12)
R^2	0.66	0.80	–	–
Adj. R^2	0.55	0.73	–	–
Num. obs.	12548	12548	2665	2197
Num. individuals	3137	3137	2665	2197
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental childcare hours spent per day in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental childcare hours spent per day in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table V: *Estimates of Months of Schooling and Enrollment Timing on Parental Monthly Income for Households with only Migrants*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.88^{***}			
	(0.72)			
Months in school		18.58^{***}		
		(4.55)		
Treated (compliers)			496.30^{**}	486.33^{**}
			(206.26)	(239.80)
R^2	0.66	0.90	–	–
Adj. R^2	0.54	0.87	–	–
Num. obs.	1344	1344	292	252
Num. individuals	336	336	292	252
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental monthly income in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental monthly income in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table W: *Estimates of Months of Schooling and Enrollment Timing on Parental Employment for Households with only Migrants*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.88^{***}			
	(0.72)			
Months in school		0.01^{***}		
		(0.00)		
Treated (compliers)			0.40^{***}	0.53^{***}
			(0.09)	(0.10)
R^2	0.66	0.77	–	–
Adj. R^2	0.54	0.69	–	–
Num. obs.	1344	1344	292	252
Num. individuals	336	336	292	252
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental employment in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental employment in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table X: *Estimates of Months of Schooling and Enrollment Timing on Parental Working Hours for Households with only Migrants*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.88^{***}			
	(0.72)			
Months in school		0.37^{***}		
		(0.09)		
Treated (compliers)			6.90^{***}	8.21^{**}
			(2.53)	(3.57)
R^2	0.66	0.82	–	–
Adj. R^2	0.54	0.76	–	–
Num. obs.	1344	1344	292	252
Num. individuals	336	336	292	252
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental working hours in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental working hours in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table Y: *Estimates of Months of Schooling and Enrollment Timing on Parental Hourly Income for Households with only Migrants*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.88^{***}			
	(0.72)			
Months in school		0.12^{***}		
		(0.03)		
Treated (compliers)			7.70^{***}	8.61^{***}
			(0.89)	(0.82)
R^2	0.66	0.80	–	–
Adj. R^2	0.54	0.73	–	–
Num. obs.	1344	1344	292	252
Num. individuals	336	336	292	252
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental hourly income in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental hourly income in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table Z: *Estimates of Months of Schooling and Enrollment Timing on Parental Childcare Hours per Day for Households with only Migrants*

	2SLS		Fuzzy RDD	
	1. stage	2. stage	±5 months	±4 months
	(1)	(2)	(3)	(4)
Eligible for enrollment	6.88^{***}			
	(0.72)			
Months in school		-0.06^{***}		
		(0.02)		
Treated (compliers)			-4.79^{***}	-5.39^{***}
			(0.93)	(0.83)
R^2	0.66	0.74	–	–
Adj. R^2	0.54	0.65	–	–
Num. obs.	1344	1344	292	252
Num. individuals	336	336	292	252
Controls	✓	✓	–	–
Time FE	✓	✓	–	–
Individual FE	✓	✓	–	–
State × interview month	✓	✓	–	–

Note: 2SLS (Column 1-2) estimate coefficients of months of schooling on parental childcare hours spent per day in all years. First stage of 2SLS instruments months of schooling via enrollment eligibility, and Wald test comparing the model including instrument and excluding instrument proves instrument relevance. Fuzzy RDD LATE (Column 3-4) estimates of enrollment in year 0 on parental childcare hours spent per day in year 2. Fuzzy RDD estimates use triangular kernel weighting and all first stage F-statistics are highly significant ($p < 2.2e^{-16}$). Standard errors (in parentheses) are clustered on household level for 2SLS, and clustered on level of cutoff distance for discontinuity samples. Controls include number of children, a dummy for younger child(ren) in household in ECEC, and a dummy for oldest child of household in ECEC before enrollment. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

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