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DISCUSSION PAPER SERIES

IZA DP No. 10661

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ABSTRACT

Does Early Child Care Attendance Influence Children's Cognitive and Non-Cognitive Skill Development?

While recent studies mostly find that attending child care earlier improves the skills of children from low socio-economic and non-native backgrounds in the short-run, it remains unclear whether such positive effects persist. We identify the short- and medium-run effects of early child care attendance in Germany using a fuzzy discontinuity in child care starting age between December and January. This discontinuity arises as children typically start formal child care in the summer of the calendar year in which they turn three. Combining rich survey and administrative data, we follow one cohort from age five to 15 and examine standardised cognitive test scores, non-cognitive skill measures, and school track choice. We find no evidence that starting child care earlier affects children's outcomes in the short- or medium-run. Our precise estimates rule out large effects for children whose parents have a strong preference for sending them to early child care.

JEL Classification: J13, I21, I38

Keywords: child care, child development, skill formation, cognitive skills, non-cognitive skills, fuzzy regression discontinuity

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1. Introduction and motivation

The socio-economic gradient in skills is already well established before children enter school in many OECD countries (Bradbury et al., 2015). These early childhood skill gaps are important from a policy perspective because they are likely to lead to inequalities later in life, including health (Case et al., 2005), education (Almond and Currie, 2011), and employment (Black et al., 2007). Child care constitutes a major aspect of early childhood given an average participation rate in formal child care of 83.8% among three to five years olds across OECD countries in 2014 (OECD, 2016). These child care programs intend to increase maternal labour force participation (e.g., Havnes and Mogstad, 2011b; Bauernschuster and Schlotter, 2015) and aim to improve children's skills (Blau and Currie, 2006). As such, child care constitutes an investment in children's skill formation.

The economics literature largely agrees that investments in children's skill formation should be targeted at earlier stages, rather than later in children's lives (Heckman and Mosso, 2014). The case for early investments, including universal child care, as an effective way of influencing the development of children rests on three major arguments. First, the time period to reap the benefits of learning is longer for early investments (Becker, 1964). Second, consistent with studies from neuroscience documenting early childhood as a critical period for brain development (Phillips et al., 2000), early investments typically carry higher returns compared to later investments (Heckman and Kautz, 2014). Third, the skill accumulation process may exhibit dynamic complementarities (Cunha et al., 2010). These dynamic complementarities imply that the cross-partial derivative for investments in different time periods is positive, i.e., later investments may have higher returns if they are preceded by early investments.

Although access to universal child care programs has greatly expanded over time across countries (OECD, 2011), the causal evidence on the effects of these universal programs produces mixed findings. For Germany, for example, Cornelissen et al. (2015) and Felfe and

Lalive (2014) estimate marginal treatment effects and exploit quasi-experimental variation in the expansion of child care centres within municipalities over time. Both studies examine children's short-run outcomes, including overall school readiness and language skills, measured at school entrance examinations. Cornelissen et al. (2015) show that returns to early child care attendance are positive only for children whose parents are least likely to enrol their children. The point estimates even suggest negative effects for children whose parents are most likely to enrol their children early. In contrast, Felfe and Lalive (2014) document that returns are positive only for children who are most likely to attend early child care. Aside of these differences, it remains unclear from these studies whether any positive short-run effects of early child care translate into better schooling outcomes and generate long-run effects.

Against this backdrop, we make two contributions to the literature. First, we analyse the effects of early child care attendance on children's skills at the ages of five and 15. To overcome data limitations that prevent most previous studies from examining both short- and medium-run outcomes, we construct a unique data basis that combines administrative and survey data allowing us to follow one birth cohort of German children over time. Second, we provide comprehensive evidence along different skill dimensions that strongly affect later educational attainment, labour market outcomes, and health (Almlund et al., 2011), including cognitive test scores, non-cognitive skill measures, school entrance examinations, and school track choice. This multi-dimensionality is especially important as Havnes and Mogstad (2015) find no effect of universal child care on test scores, but on educational attainment and earnings. Their results suggest that improvements in non-cognitive skills, rather than cognitive skills, drive the child care effect.

To estimate the causal effect of early child care, we exploit exogenous variation in child care starting age within a fuzzy regression discontinuity design. While children in our sample are legally entitled to a slot in public child care only from their third birthday onwards, many children start formal child care earlier. Specifically, many children start child care in the summer of the calendar year in which they turn three. Therefore, children born in the last quarter of a year start child care on average at the age of three years and two months, whereas children born in the first quarter of the subsequent calendar year start on average at the age of three years and seven months. This enrolment pattern creates a December/January discontinuity in child care starting age of five months. As this discontinuity does not affect children's age at school entrance, starting child care earlier increases the duration of child care attendance. Hence, we estimate the effect of starting child care earlier and attending for longer on children's skills.

Our results are summarised as follows: we first show that children who start child care earlier do not perform differently in terms of standardised cognitive test scores, measures of non-cognitive skills, or school track choice, measured in grade 9, i.e. at the age of 15. Given the strong first stage and large sample sizes, the zero effects are estimated precisely enough to rule out that starting child care earlier substantially improves skills on average. We then examine the effects for subgroups by parental education, migration background, and gender, and find no robust evidence for treatment effects for any subgroup. Next, we investigate whether the treatment never generated positive effects, or whether any initial gains faded out over time. To this end, we analyse the short-run effect on children's skills measured during school entrance examinations at the ages of five to six. Examining children's overall school readiness, language competencies, motor skill difficulties, and behavioural problems, we do not find a robust effect overall, nor for any of the subgroups.

Our instrumental variables approach identifies the effect from children who enter child care before becoming legally entitled. We show that for these children, whose parents have a low resistance to early child care, starting child care earlier is unlikely to improve or deteriorate their skills. Hence simply easing access to early child care, for instance by providing more slots, does not improve these children's skill development. While our approach cannot uncover the effect for children of parents with a high resistance, we extend the literature by documenting the absence of short- and medium-run effects on cognitive and non-cognitive skills for our compliers who have a high preference for early child care.

Our paper is organised as follows: we first provide a brief review of the relevant literature in Section 2. Section 3 describes the different data sources and the different measures for skill development. Section 4 outlines the institutional features of the German child care system, giving special emphasis to the enrolment patterns. These patterns determine our identification strategy, also described in Section 4. Section 5 presents both graphical and regression-based evidence for the effect of early child care attendance on various measures of children's skill development. We discuss these results and conclude the paper in Section 6.

2. Related literature

Previous studies that analyse the effect of universal child care programmes on children's outcomes differ with respect to i) the age at which children begin child care; ii) the age at which the outcomes are observed; iii) the outcomes themselves (different measures of cognitive and non-cognitive skills); iv) the countries and data used; v) identification strategies (difference-in-difference, instrumental variables, regression discontinuity, control function); vi) the intensity *and* the quality of treatment; vii) the counter-factual care mode (maternal/formal/informal care). Due to these differences, it is not surprising that the studies yield ambiguous evidence ranging from negative effects (Baker et al., 2008; Gupta and Simonsen, 2010) to long-lasting positive effects (Havnes and Mogstad, 2011a, 2015). See Table A.1 for a concise review of that literature.[1]

As Felfe and Lalive (2014) and Cornelissen et al. (2015) also use German data, from school

[1]Bernal and Keane (2011); Bradley and Vandell (2007); Burger (2010); Elango et al. (2016); Pianta et al. (2009); Ruhm and Waldfogel (2012) also provide more detailed literature reviews.

entrance examinations, these papers are of particular interest for our study. These large datasets include information on completed years of child care attendance, though not on exact child care duration. Both studies exploit the differential rates of expansion of child care centres across municipalities over time as an instrumental variable within a marginal treatment effects (MTE) approach to estimate the effect of attending child care earlier on children's skills.

Felfe and Lalive (2014) use the school entrance examinations data for one German federal state, *Schleswig-Holstein*, for children born between 2002 and 2004. The study exploits the expansion of child care centres across school districts between 2005 and 2008 and focuses on attending child care before the age of three. The study reports positive linear IV estimates for overall school readiness (+6.6pp) and socio-emotional development (+9.6pp). They find evidence for heterogeneous effects: whereas the skills of children of highly educated mothers are not affected, children of low/medium educated mothers, and of migration background, benefit substantially from early child care attendance. For instance, their policy simulations for an expansion of the supply of early child care from 8.4% to 20% of children show that the probability of having no language problems increases by 31.6pp for foreign children; similarly, the motor-skills and behavioural outcomes of children of less educated mothers also benefit from the same policy intervention. Their MTE curves only show positive skill effects for children of parents with a high preference for early child care.

Cornelissen et al. (2015) use the same methodology, but a different data source, namely administrative school entrance examinations data from the *Weser-Ems* region. The study examines the outcomes of children born between 1987 and 1995 and exploits the staggered expansion of child care centres in the mid-1990s, which mainly increased access to child care for children aged three to four. On average, the study finds no effects on school readiness, motor skills, or the Body-Mass-Index of children. However, using the MTE approach, the study shows that the effects of attending child care for at least three years on school readiness are highest for children of parents with a low preference for early child care. While the average treatment effect for the treated is insignificant, Cornelissen et al. (2015) document that the average treatment effect for the untreated amounts to 17.3pp for school readiness. However, their point estimates along the MTE curves also suggest that attending child care earlier may harm children whose parents have a strong preference for early child care.

3. Institutions and data sources

3.1. Institutional details

In this section we describe the institutional features of the West German child care system, when our sample members entered child care. The formal child care system for pre-school aged children mainly caters for children of two different age groups: infants aged zero to three (*Krippe*) and children aged three to six (*Kindergarten*). Child care is typically organised and funded at the local level where municipalities (*Gemeinde*) bear the primary responsibility (Evers and Sachße, 2002). The child care centres are mainly run either by municipalities or by large non-governmental organisations that cooperate closely with the municipal decision makers in the planning process (Heinze et al., 1997).[2] The child care year runs in parallel with the academic school year, which starts either in August or September depending on the federal state. Due to the decentralised planning process, large regional differences in child care coverage exist (Mamier et al., 2002), although child care quality is highly regulated and thus fairly homogeneous.

Compared to child care provision in, e.g., the US or the UK, a distinguishing feature of the German system is that no noteworthy private child care market has emerged (Evers et al., 2005), mainly due to strict regulations, high market entry barriers, and dominance by

[2] For more information on the administration and funding of the German child care system, see the excellent descriptions by Kreyenfeld et al. (2001) and Evers and Sachße (2002).

publicly funded providers (Kreyenfeld and Hank, 2000). In the late 1990s, children spent usually half a day in formal child care, typically 4 hours in the morning, and spent the rest of their day with their parents or "other" informal care (Hank and Kreyenfeld, 2003).[3] The provision was highly subsidised so that parents typically did not pay more than 3-4% of annual household income on child-care services (Evers et al., 2005); for families on social assistance, child care provision was free.[4] Since 1 August 1996, children from age three until school entry were legally entitled to attend highly subsidised half-day public child care.[5]

Due to political decentralisation, state governments decide about educational policies in Germany. Hence, each federal state has enacted a separate law regulating the provision of child care.[6] Although the wording differs between the states, each federal state sets clear developmental goals regarding the language, motor skill, and behavioural development of children for the care centres. However, the pedagogical approach contrasts to other countries, such as France, where early child care follows strict guidelines that emphasise fairly formalised forms of early learning (Chartier and Geneix, 2007). In contrast, the educational content in Germany follows the social pedagogy tradition where children develop social, language, and physical skills mainly through play and informal learning (Scheiwe and Willekens, 2009).

In Germany, children start school by age 6 where the school entrance cut-off during the

[3]For instance, less than 2% (5%) of children between the ages of three to six (under the age of three) where looked after by a child-minder in the late 1990s (Evers et al., 2005).

[4]The average expenditure per attending child amounted to 4,937$ in 1999 which was higher than the OECD average of 3,847$. For comparison, the expenditures totalled 3,901$ in France, 6,233$ in the UK, and 6,692$ in the US (see OECD, 2002).

[5]Given the low initial level of child care provision, creating slots for all children was considered quite demanding. To ease that burden, municipalities were allowed to implement cut-off rules until the end of 1998 and thereby hold back children for up to one year. In Section 4 we show that our identifying variation is driven by children who start child care before their third birthday, so that they started child care before they became legally entitled. Hence these children were not affected by such rules. Only in August 2013 did the government extend the legal entitlement for children from their first birthday onwards.

[6]These are all available at http://bage.de/menue/links/links-zu-den-kita-gesetzen-der-einzelnen-bundeslaender. Last accessed 30/11/2016.

period of analysis was at the end of June (see Faust, 2006).[7] The secondary school system consists of a basic vocational track, *Hauptschule*, an intermediate vocational track, *Realschule*, and an academic track, *Gymnasium*. Students are tracked into these types of secondary schools after completing primary school typically at age 10. The tracking decision is based on teachers' assessments of children's academic potential, while–depending on the state–parents have some discretion to enrol their children in a higher track than recommended (for details, see Dustmann et al., 2016). Hence, another way of examining the effect of starting child care earlier on children's skills is by looking at the effect on children's school track choice (Schneeweis and Zweimüller, 2014).

3.2. National Education Panel Study (NEPS)

For our main analysis we use data from the German National Education Panel Study (NEPS). The NEPS was initially developed in 2009 to provide information on the determinants of education, the consequences of education, and to describe educational trajectories over the life course (Blossfeld et al., 2011).[8] We focus on *Starting Cohort 4*, which was first surveyed in grade 9 in 2010. We use data from the first two waves that were both conducted in grade 9 during the academic year 2010/11, when schooling was still compulsory for this cohort.

The NEPS interviews both the children and parents separately. The interviews contain rich information on parental education and occupations, migration background, family income, and the household size and composition, which we use to assess the importance of potential confounders. Furthermore, the parents state the year and month when a child first entered formal child care.[9] To better understand the heterogeneity in the effect of early child care

[7]At that time, less than 3% of children started school earlier and the share of children starting earlier is even lower for children in December and January, see Figure A.1.

[8]The NEPS study uses a stratified two-stage sampling procedures. The NEPS draws a random and representative sample of schools in Germany and then samples classes. For more details on the study design and sampling process, see Skopek et al. (2013).

[9]About 60% of parents complete the parental interview. Hence we refer to the 'full' and 'parent'

attendance, we classify parents as possessing low/medium education if parents' highest level of education is less than upper secondary (*Fachabitur/Abitur*). Moreover, we classify parents as 'non-natives' if the mother's native tongue is not German.[10]

The NEPS provides standardised test scores to assess children's competencies in different dimensions. We use these standardised skill measures for German language, STEM (Science, Technology, Engineering, Mathematics) subjects, and general cognition. Aside of cognitive skill measures, the NEPS also collects information on two well-established measures of non-cognitive skills, the Strength and Difficulties Questionnaire (SDQ) and the Big-Five personality traits. We again standardise each score to have mean 0 and standard deviation 1 (see Appendix A for details on the computation of all skill measures).

Our analysis focuses on children who were born between July 1994 and June 1996. Since we observe these children in grade 9, children born between July 1994 and June 1995 either repeated a grade or delayed their school entry; children born between July 1995 and June 1996 represent the regular school cohort. Furthermore, we exclude children currently living in East Germany due to large differences in early child care attendance between East and West Germany (Kreyenfeld et al., 2001).

3.3. Administrative data

In addition to the NEPS data, we use two sources of administrative data which cover the same birth cohorts: school census data from Bavaria and administrative school entrance examinations from *Schleswig-Holstein*.[11] Both data sets are suitable for our analysis within

sample in Section 5, where we also provide evidence that our estimated treatment effects are not biased by non-random response.

[10] Since the alternative form of care at that time largely was maternal care (Cornelissen et al., 2015; Evers et al., 2005), non-native children would mainly be exposed to a foreign language in the counter-factual care situation. We are precisely interested in the treatment effect for this group, particular in terms of language skills, and thus we prefer this 'linguistic' definition of non-native.

[11] The choice of these two particular states is entirely driven by the restricted availability of administrative education data in Germany.

a two-sample instrumental variables approach (see Section 4) since they cover the same birth cohorts as the NEPS and, due to the large sample sizes, yield more precise reduced form estimates. To alleviate any concerns about the comparability between these two states and the rest of West-Germany we additionally use data from the German Microcensus, which annually provides a one percent sample of the population currently living in Germany. Table A.2 compares some basic demographic characteristics, which also affect children's skills, and shows that Bavaria and *Schleswig-Holstein* do not differ substantially from the rest of West-Germany along these characteristics, apart from urbanity.

To examine track choice at age 15, i.e., in the last year of compulsory secondary schooling, we use data from the Bavarian school census for the academic school year 2010/11. The school census covers the full population of students in Bavaria and includes information on month and year of birth, the attended track, but not on test scores.[12] Furthermore, the census contains information on children's migration background. A child is classified as 'non-native' if either of the parents was born abroad, does not hold German citizenship, or if the main language spoken at home is not German. However, no other socio-demographic characteristics are available.

Additionally, we use data from school entrance examinations in *Schleswig-Holstein*, where these examinations are compulsory for children shortly before entering primary school. The purpose of the exams is to assess children's health, their socio-emotional development, their language and motor skills, and to ultimately provide a recommendation whether children are ready to enter school the following academic year. The standardised tests and assessments are all conducted by public health paediatricians. These data provide a comprehensive picture of child development and are more objective than parent-reported information. Paediatricians assess children's language based on children's articulation and

[12]In its regular version, the scientific use files only contain quarter of birth due to data confidentiality. Given the importance of month of birth for our identification strategy, we were able to negotiate access to month of birth, whilst having to compromise on parental migration characteristics.

hearing problems. Motor skills are assessed through different exercises, including jumping across a line, standing on one leg, and jumping on one leg. To assess behavioural problems, paediatricians make a clinical assessment based on the child's behaviour and parental information during the medical screening. In some districts, parents additionally fill out the SDQ.

Paediatricians provide a school readiness recommendation. This recommendation is not a mechanical function of the medical diagnoses as the paediatricians are allowed to weight the information differently depending on the children's development and socio-economic background. For instance, paediatricians may weight language skills differently by migration background. Ultimately, the recommendation is not binding.

For the subgroup analysis, we consider the following groups: (1) parental educational achievement, where we classify parents as possessing low/medium education if parents' highest level of education is less than upper secondary[13]; (2) the child's gender; (3) migration background, where we classify mothers as 'non-natives' if they were born abroad in absence of information on the mothers' native tongue or the primary language spoken at home.

4. Empirical strategy

4.1. Enrolment in child care

Parents can theoretically enrol their children in at least three ways that we illustrate in Panel A of Figure 1. We focus on August as the starting point of the child care year for ease of illustration.

First, parents may enrol their children once they reach their legal entitlement age, i.e., the

[13]Since the information is provided voluntarily by the parents, parental education is missing for about 40% of children.

month they turn three. If all parents enrolled their children exactly on the day their child turned three, the average age at child care entry would be flat at age three across all birth months. This practice would correspond to continuously enrolling children during the child care year.

Second, parents may enrol their children at the start of the child care year after attaining legal entitlement. This form of enrolment is fairly practical: as the oldest children in child care leave and start school in August, the vacant places then become available for the next cohort of children to start child care in August. The downward sloping lines in Panel A show how this pattern would lead to a downward-sloping linear relationship between birth month and child care starting age. The shifts between the downward sloping lines are explained by the decision to enrol children the year they turn two, three, or four.

Third, children born between August and December attain their legal entitlement shortly after the beginning of the child care year. Given the short gap between the start of the child care year and becoming legally entitled, parents may try to enrol their children at the beginning of the child care year to allow their children to start child care jointly with the rest of the group. While children in this case would start child care without legal entitlement, child care centres were legally allowed to accept younger children if slots were available. Thus, children born between August and December may actually start child care before they turn three.

Overall, it remains an empirical question which of these enrolment pattern dominates. We therefore examine the empirical evidence on enrolment using the NEPS survey. Overlaying the empirical evidence over the theoretical regimes, Panel B presents evidence supporting all three enrolment regimes.

Figure 2 presents the three major patterns in more detail for all children. First, the majority of children start child care at the start of the school year in August/September (Panel A). Second, despite the legal entitlement to subsidised child care from the third birthday onwards, only few children start child care the month they turn three (see Panel B). The only systematic exception are children born in August/September whose birth month happens to overlap with the start of the child care year. And third, Panel C shows that children born between August/September and December are substantially more likely to start child care before their third birthday.

Taken together, children typically start child care at the beginning of the academic year of the calendar year in which the child turns three. This pattern generates a discontinuity in the average child care starting age between December and January. Averaging over birth months across cohorts, Panel A in Figure 3 shows that children born in the last quarter of a calendar year start child care substantially earlier than children born in the first quarter of the subsequent year. Specifically, we observe that the average child care starting age jumps discontinuously between December and January by just over 0.4 years, i.e., about 5 months. The discontinuous jump is of similar size for both December–January windows in our sample, see Panel B in Figure 3. We next describe how we use this relationship in our empirical methodology.[14]

4.2. Fuzzy Regression Discontinuity Design

We model the effect of child care starting age ($kgage$) on children's skill measures as follows:

$$y_i^k = \alpha_c^k + \beta^k kgage_i + f_c^k(M_i) + \phi^k X_i + \epsilon_i^k \tag{1}$$

where the dependent variable y_i^k denotes the skill measure k for individual i. Since we observe two school entrance cohorts of children in grade 9, we include a cohort fixed

[14]To alleviate any concerns about recall bias, we also examine child care starting age in the NEPS Starting Cohort 2, which covers children born between 2005 and 2006. These children were first surveyed in 2009. Figure A.2 shows that we still observe a similar discontinuity in child care starting age between December/January. Again, this discontinuity is driven by the majority of parents enrolling their children at the start of the child care year in August/September.

effect (α_c^k). To maximise precision while avoiding the June/July school entrance cut-off, we restrict our sample to children born between August and May.[15] Given the potential importance of absolute and relative age effects, which may link children's birth months with their skills, we control for the assignment variable (month of birth) using a linear control function $f_c^k(M_i)$ that also interacts month of birth with the December/January threshold. We also test the sensitivity of our results with respect to changes in the specification of $f_c^k(M_i)$. For simplicity, we recode month of birth so that the December/January threshold lies in the centre of the interval $M_i = (-4.5, ..., 4.5)$. X_i represents a vector of child and parent characteristics, ϵ_i^k is an error term.

A simple OLS regression of equation 1 will yield biased coefficients, since parents are likely to selectively send their children to child care earlier, or later, depending on (unobserved) parental preferences and children's skills. To solve the endogeneity of $kgage$, we use birth month to construct our instrument and define the indicator variable $before$ as

$$before_i = I(M_i < 0) \tag{2}$$

Hence, $before$ equals 1 for individuals born between August and December, and 0 otherwise. Our first stage equation is

$$kgage_i = \alpha_{1c} + \delta_1 before_i + f_{1c}(M_i) + \phi_1 X_i + \epsilon_{1i} \tag{3}$$

The parameter δ_1 identifies the discontinuous jump in average child care starting age at the December/January threshold conditional on birth month effects. Since the probability of starting child care earlier does not switch from 0 to 1 between December and January (Panel B, Figure 3), our approach resembles a fuzzy regression discontinuity (RD) design.

[15] In Appendix B we report all main results for a smaller three months window ranging from October to March. The results remain the same, although they are estimated less precisely.

To compute the Wald estimator, we estimate the reduced form as in equation (4):

$$y_i^k = \alpha_{2c}^k + \delta_2^k before_i + f_{2c}^k(M_i) + \phi_2^k X_i + \epsilon_{2i}^k \tag{4}$$

where δ_2^k denotes the average difference in skill k between children born before and after the December/January threshold. Using one data set, the Wald estimator corresponds to the two-stage least squares estimator. Given monotonicity and exogeneity, the Wald estimator for the effect of our continuous treatment variable identifies an "average causal response" which computes a weighted average of several local average treatment effects (LATE, Angrist and Pischke, 2009).

Since we only observe child care starting age in the NEPS, we cannot compute the two-stage least squares estimator using the administrative data sets. However, since we observe month of birth in all data sets, we can compute the two-sample instrumental variables estimator (Angrist and Pischke, 2009) dividing the reduced form coefficient of any sample by the first stage coefficient from the NEPS sample. The approach relies on the assumption that the first stage applies equally to the different samples. To support this assumption, we first compare federal states on socio-demographic and socio-economic characteristics relevant for child development. Table A.2 shows that Bavaria and *Schleswig-Holstein* are similar to the rest of West-Germany regarding socio-economic characteristics, apart from urbanity. Second, we show that the first stage relationship holds similarly when pooling data from Bavaria and *Schleswig-Holstein* only. If anything, the first stage relationship is slightly stronger in these two states than for West Germany as a whole, leading to an overestimate of the treatment effect using the overall first stage coefficient. Taken together, these pieces of evidence support our approach to combine the first stage estimates from the NEPS with the reduced form estimates from these two states.[16]

[16]Ideally, we would estimate the first stage separately for Bavaria and *Schleswig-Holstein*. Unfortunately, state-specific analyses are prohibited with the NEPS data. However, we are allowed to combine at least

5. Results

5.1. Instrument validity and first stage estimates

The validity of our analysis requires the instrument to be as good as randomly assigned. We perform two tests to examine this assumption. First, we examine whether births are smoothly distributed around the December/January threshold. Figure 4 shows that births are evenly distributed around the turn of the year supporting the assumption that the running variable, month of birth, is not systematically manipulated.

Second, we perform balancing tests and examine whether pre-determined characteristics that affect children's cognitive and non-cognitive skills differ discontinuously between children born before and after the December/January threshold. In our context, children's age at testing is of particular interest given the importance of relative age effects in such tests (Black et al., 2011). Therefore, Figure 5 presents the average age at testing by year and month of birth. In line with our identifying assumptions, age at testing is distributed smoothly around the discontinuity.

Table 1 reports the means of further characteristics for children born within a five-months window before and after the December/January threshold along with the group differences, corresponding t-statistics, p-values, and normalised differences.[17] We use the rich information on socio-economic characteristics from both the parents' and the children's interviews. The results show no substantial differences between the two groups of children. Panel A presents the results using information from the child surveys, i.e., the full sample that we can use in the reduced form analysis; Panel B relies on information from the smaller

two states for the analysis. Figure A.3 presents the first stage using only observations from Bavaria and *Schleswig-Holstein*. Table A.3 provides the corresponding regression coefficients.

[17]The normalised difference of variable x is defined as $\Delta_x = \frac{\bar{x}_1 - \bar{x}_0}{\sqrt{(s_1^2 + s_0^2)/2}}$, where s_z^2 represents the sample variance of x for children with instrument equal to 1 ($z = 1$) and with instrument equal to 0 ($z = 0$). The normalised difference should not be larger than 0.2, see Rubin (2001), and in our case they are all below 0.1.

parent sample; Panel C finally reports regional statistics on the availability and quality of child care at the district level. The characteristics, again, are very similarly distributed between the two groups. Overall, the similarity in terms of observable characteristics lends credibility to the assumption that our instrument is as good as randomly assigned between the two groups.

We now turn to the size and robustness of the first stage relationship. Table 2 shows the estimated first stage coefficients $\hat{\gamma}$ from equation 3. To assess the robustness of the first stage relationship, we present the results for different specifications and sample definitions. Panel A starts with a 5 months-window around December/January. Column (1) shows the results of our baseline specification that controls only for a linear time trend in birth month interacted with the December/January threshold. In this specification, being born between August and December reduces the child care starting age by 0.40 years, or 4.8 months. The coefficient is highly significant with an F-statistic of 98. In column (2) we include pre-determined control variables from the children's survey including the child's gender and age (at the time of the interview), parental education, parents' place of birth, family status, mother's age, and household size. However, including these characteristics neither affects the estimated coefficient $\hat{\gamma}$ nor its statistical significance. The same holds when we interact the control function with the cohort, see column 3, or combine both, see column 4. To control for regional differences in child care coverage rates and quality, we also include 234 district fixed effects in columns (5) and (6). The point estimates increase to about 0.42 with and without control variables. Finally, column (7) additionally includes cohort-specific quadratic time trends interacted with the discontinuity. Allowing for this more flexible specification slightly increases the point estimate, and the coefficient is estimated less precisely. As neither the inclusion of a rich set of control variables, district fixed effects, nor a very flexible specification of the running variable substantially affect the point estimates compared to the basic specification in column (1), the results further strengthen the assumption that month of birth is not systematically related to unobserved characteristics of the children or the families.

We next test whether narrowing the window around the discontinuity affects the first stage estimates. Panel B in Table 2 presents the results including only children born between October and March. Across specifications (1) to (7), the point estimates are almost identical to those obtained with the wider window, although, expectedly, the estimates become less precise. Given these consistent results, we proceed with our preferred specification (including district-fixed effects and pre-determined characteristics, as reported in column 6) and the larger 5-months window in the remaining analysis.[18]

Next, we examine heterogeneities in the first stage relationship. Such heterogeneities are of particular importance in our setting for at least two reasons. First, our estimation strategy yields the average causal response. Hence, we can only interpret the results once we know which groups respond to the instrument. Second, we need to ensure that the first stage relationship holds for the subgroups that we analyse separately.

The first column of Table 3 reports the results using our preferred specification for different subgroups–by parental education, by migration background, and by the child's gender. In Panel A, we first use the demographic characteristics from the children's survey to classify the subgroups and to generate the control variables. We run a fully interacted model and report the estimated first stage relationship (see equation 3) for the reference group and for the interaction term with our instrument. Table 3 shows that the instrument affects all groups very similarly. The differences in the first stage relationship are rather small and statistically insignificant. This implies that our compliers are not a specific subgroup from the population of interest in terms of observable characteristics. Turning to the analysis for

[18] As a further robustness check, we dropped observations who gave inconsistent information about children's school career, i.e., they report the child attending a grade that they should not. Table A.4 shows that dropping these observations does not substantially affect our first stages estimates.

specific subgroups, we conclude that the first stage relationship is strong and practically identical for all of them.

To check the data quality and the robustness of our first stage relationship, Panel B reports the results when using information from the parent survey. Overall, the estimated first stage relationships do not change substantially, and again they do not differ significantly across subgroups. The results show that information from the children and from the parent survey lead to the same conclusions; we therefore use the children's information for our further analyses to maximise the sample size.

5.2. The effect of child care starting age on skills at age 15

We begin our analysis of the effect of child care starting age on later skills with a series of graphs reporting the reduced form results. Figure 6 presents the overall picture by relating mean skill outcomes to birth months. The outcomes comprise cognitive measures (language, STEM, and general cognition) as as well as non-cognitive skill measures, i.e., SDQ scores for peer problems and the Big-Five. We observe rather smooth trends in average skill measures across birth months and, importantly, no discontinuity in outcomes around the December/January threshold.

Table 4 reports results from three different specifications. Controlling only for the running variable, column (1) shows that the reduced form estimates of the effect are fairly small and statistically insignificant throughout. The estimates range from a 0.06 standard deviations decrease in neuroticism to an increase in language skills of 0.04 standard deviations.[19]

In columns (2) and (3), we include additional controls for the children's socio-demographic

[19] Appendix Figures A.5 and A.6 and Table A.5 show that we find the same patterns when including only children from the parent sample. Due to the smaller sample size, the graphical patterns become more noisy and the estimates less precise. However, the consistent results between both samples mitigate concerns that combining the first stage results from the smaller parent sample with the reduced form results for the full sample might invalidate our analysis.

characteristics and district fixed effects, respectively. Including these control variables improves the explanatory power substantially as reflected by the increasing R^2. Reassuringly, the reduced form estimates are highly robust to controlling for these characteristics. The estimated effects are close to 0 and fairly precisely estimated. Thus, we find no evidence for an effect on any of these outcomes. Given our fairly precise estimates with standard errors close to 0.04 standard deviations, our finding of no substantial effect cannot be attributed to a lack of statistical power.[20]

In column (4), we report the Wald estimates which divide the reduced form estimates from column (3) by the corresponding first stage estimate. The estimates now refer to the effect of starting child care *one year later*. This interpretation relies on the assumption that the reduced form effect, which stems from an increase in average child care starting age of about 5 months, can be linearly extrapolated. The Wald estimates in column (4) show that the estimated effects remain small and statistically insignificant for all outcomes. Since the linearity assumption may not hold when the "dose" of the treatment is doubled, we place some caution on the interpretation of the Wald estimates. Therefore, we do not scale up the following results, but report the reduced form effects that stem from a five months decrease in child care starting age.

While we do not find an effect of starting child care earlier on average, the aggregate effect might mask a substantial degree of heterogeneity across particular groups (Cornelissen et al., 2015). For instance, we may expect that children whose mothers are not native German speakers would benefit from attending child care earlier due to the higher exposure to the German language. We therefore next examine the effect of child care starting age for specific subgroups. Since the analysis of the first stage (see Table 3) did not reveal any

[20]Figure A.7 illustrates the statistical power of our analysis. For instance, given our sample size, we can detect a reduced form treatment effect of 0.05 with a probability of 80% even without conditioning on any covariates. In our richest specification with a residual variance of 0.7, we can detect a reduced form effect of 0.05 with 96% probability.

significant differences across demographic groups, any potential effects for subgroups must result from differences in the reduced form estimates.

Figures 7 and 8 present the reduced form results by maternal education, maternal native language, and child sex. Overall, we do not find that children born around the December/January threshold differ substantially with respect to the examined cognitive and non-cognitive skill measures. Table 5 confirms the graphical evidence revealing few statistically significant differences. We find significant positive effects on language skills (+0.20SD) and cognition (+0.21SD) for children of non-native mothers. However, a closer inspection of the corresponding graph shows that January-born children, who score particularly badly, drive this result. Applying a doughnut-hole specification, i.e., excluding December and January from the estimation, we observe no statistically significant difference in average language scores between non-native children born before and after the December/January threshold.[21] We therefore conclude that the subgroup analysis does not reveal any meaningful differences for any of the considered outcomes.

If attending child care earlier improves children's skills, this improvement might be reflected at different track choice margins depending on the children's abilities. For children with medium to high ability levels, skill improvements should increase the probability to attend the academic track *Gymnasium*. For children with low to medium ability levels, skill improvements might not suffice to attend the academic track, but the improvements could lift such children from the basic track, *Hauptschule*, to the intermediate vocational track, *Realschule*. We will therefore examine both margins in our analysis using both the NEPS data and the Bavarian administrative data.

We start with the probability of attending *Gymnasium*. First, using both the NEPS and

[21] Further analyses reveal that the January dip is mostly driven by children of Turkish mothers. Once we remove Turkish children (N=399) from the sample, the treatment effects become small and statistically insignificant.

Bavarian administrative data, Figure 9 relates the proportion of students attending *Gymnasium* to year and month of birth. We do not observe a discontinuity in the probability of attending *Gymnasium* at either of the December/January thresholds.[22] Reassuring for our two-sample IV approach, the figure shows that the share of students attending the academic track by year and month of birth is similar in both data sets. Hence, we proceed with the larger Bavarian sample for the subgroup analysis.

Figure 10 shows the reduced form relationship for the probability of attending Gymnasium by subgroups using the Bavarian data. We find no evidence for a treatment effect for any of the subgroups. Table 6 provides the corresponding reduced form coefficients for the effect on track choice. In Panel A, we look at the probability to attend *Gymnasium*. We find no effect. Looking at non-natives, the probability to attend *Gymnasium* is merely 0.6 percentage points larger for children born before the December/January threshold, and the difference falls short of statistical significance. Using this large dataset, the estimate is again precise enough to rule out any substantial effect.[23]

Turning to the effect on attending the basic track, *Hauptschule*, Panel B reports the respective reduced form estimates. As before, we find no effect on average for all students. For non-native students, the probability to attend the basic track is 0.9 percentage points larger for those born before the December/January threshold. This difference is not only statistically insignificant, but also runs counter to the expected improvements in cognitive skills.[24]

[22]Since we only observe children who attend grade 9, children born between July 1994 and June 1995 have either started school late, or repeated a grade. Hence they tend to have lower academic skills and a lower probability of attending *Gymnasium* compared to children in the same grade but born between July 1995 and June 1996.

[23]Since the data do not include additional information, we cannot control for additional child and parent characteristics. However, as Table 4 shows, our results from the NEPS do not depend on including additional control variables.

[24]Using the same data source for the same cohort, we also examine the effect on track choice in grade 5, i.e., just after switching from primary to secondary school at age 10. The results in Appendix Table A.6 confirm our finding that there is no effect on track choice.

5.3. Does the effect fade out over time?

So far we have provided new, comprehensive evidence that attending child care earlier does not affect children's cognitive or non-cognitive skills in grade 9 at the age of 15. We now examine whether early child care ever had an effect by using data from school entrance examinations. While we would expect positive initial effects to lead to further gains during the process of human capital formation if the skill accumulation process exhibits dynamic complementarities (Cunha and Heckman, 2007), initial gains might also fade out over time. Examples of fading-out include the Perry Pre-School trial where the initial IQ gains vanished by age 10.[25] In our setting, initial effects could for instance vanish if educators re-allocate resources away from highly-skilled children to less-skilled children. In technical terms, educators might violate the stable-unit-treatment value assumption (SUTVA). In this case, children in the control group (with a relative skill deficit) would receive additional resources and thus benefit indirectly from the treatment of other children. To assess the plausibility that initial effects fade out by age 15, we next examine the effect earlier in life, namely just before entering school.

For our reduced form analysis of the effect of child care starting age on early skill measures, we use administrative data from school entrance examinations in Schleswig-Holstein. Figure 11 visualises the reduced form analysis for four outcomes: school readiness, language development, behavioural difficulties, and motor skills. We do not find evidence for a pronounced effect of child care starting age on either of these outcomes. Figure 12 repeats the analysis for different subgroups – by mother's country of origin, mother's education, and the child's gender.[26] Overall, we observe rather smooth patterns around the December/January threshold for all of the outcomes for the subgroups. The graphical evidence,

[25]However, concerning non-cognitive skills, there was no fade out as children still did better in achievement tests later due to higher non-cognitive skills (Heckman et al., 2013).

[26]We are forced to restrict the analysis on the 1995m7-1997m6 cohorts since the basic demographic information identifying the subgroups was collected for the first time for the 1995m7-1996m6 cohort.

however, might suggest some potential effects on speech competencies for children of non-native mothers. Moreover, children of highly-educated mothers appear less likely to be ready for school if born in the last quarter, even though they are speech incompetent less often.

Panel A of Table 7 presents the corresponding regression results. On average, column (1) shows that we do not find significant effects on any of the outcomes, apart from a two percentage points decrease in girls' likelihood to be not school ready, and an even smaller reduction in average speech incompetence (-0.3 pp). However, these two differences are only marginally significant. To obtain more precise estimates, we add the subsequent school entrance cohort, doubling our sample size.[27] Panel B of Table 7 shows that the overall picture remains the same, although we now find an increase in the school readiness of girls and children of low-educated mothers of about 2 percentage points. However, when applying a doughnut-hole specification or modelling the running variable differently, the relationships disappear. Hence, we caution against overinterpreting the statistically significant estimates from the main specification.

Finally, Table 8 provides some additional heterogeneity analyses and splits the sample by gender and maternal education, and mother's country of origin. We again pool two birth cohorts (1995m7–1997m6) to obtain more precise estimates. Panel A shows that we do not find any significant reduced form effects for boys. Panel B suggests that girls benefit from starting child care earlier, where the difference in school readiness is similar across all groups. However, this effect is again not robust and should be interpreted cautiously. Moreover, any potential effect seems to either fade out or be too small to affect medium-run outcomes as reflected by track choice and skill measures in grade 9.

[27] We refrain from adding more school entrance cohorts to stay as close as possible with the birth cohorts 1994m7-1996m6 for which we can estimate the first stage using the NEPS.

5.4. Discussion

Examining several relevant cognitive and non-cognitive skill measures from different data sources and at different points in children's lives, we find no evidence for an effect of earlier child care attendance on skill development. In particular, we find little to no evidence for positive effects on children from non-native and low socio-economic backgrounds. We offer five explanations to rationalise this result.

First, is the treatment itself too short in duration to have any meaningful effect? This explanation is unlikely given that children born in December spend around 400 hours more in formal child care than children born in January.[28] The duration is therefore large in absolute and relative length. Second, is the treatment uptake too low? Again, this is unlikely given the strong and highly robust first stage results that we have documented. Third, does the treatment occur too late in life to have an effect? This explanation, again, is unlikely given that other early childhood intervention programs, such as HEAD start, occur around the same age and have an effect on children's outcomes (Garces et al., 2002). Fourth, is the form of German child care, i.e., the socio-pedagogical approach, generally unsuited to improve children's skill? This explanation is unlikely given that Cornelissen et al. (2015) and Felfe and Lalive (2014) find that starting child care earlier benefits some children–even though some findings of these two studies stand in contrast to each other.

Fifth, does our identifying variation and hence our estimated parameter explain the result? Recall that our instrumental variables approach identifies the effect for children who enter child care before becoming legally entitled. As attending child care without a legal entitlement requires some extra efforts by parents, these parents are likely to have a strong preference for, or low resistance against, early child care. Given the result of reverse se-

[28]For this calculation, we assume that children born in December attend child care five months longer than children born in January and that they attend child care for five days a week and four hours per day. The additional time spent in child care then amounts to 430 hours. Assuming children miss out one month, e.g., due to illness and/or holidays, we arrive at 344 hours.

lection on unobservable gains (Cornelissen et al., 2015), it is possible that we do find any effect because individuals shifted into treatment by our instrument are unlikely to have large gains from early child care.

To shed more light on which compliers drive our results, we follow Heckman et al. (2006) to calculate the different weights of observations in an instrumental variable estimation. As the approach requires a binary treatment, we recode our treatment variable to an indicator whether a child started child care before the third birthday. Hence, we obtain the weights that our instrument assigns to children with different propensities to start child care before their third birthday. Figure A.11 shows that our instrument puts most weight on individuals with a low resistance towards child care and hardly any weight to children with a medium to high resistance to child care. Figure A.11 therefore highlights that our identifying variation stems from children of parents with a low resistance towards starting child care early. Hence, our results are completely consistent with Cornelissen et al. (2015) who find no average effect of early child care on school readiness and that the gains are concentrated among children of parents with a high resistance to child care. As their point estimates indicate that children with a low resistance suffer from starting child care early, we extend the literature by showing that these children do not exhibit any negative effects.

6. Conclusion

Early child care programs have been increasingly introduced in many OECD countries. Whilst some studies examine whether increasing access to early child care affects children's short-run outcomes, with ambiguous findings ranging from positive to negative effects, it remains unclear how sending children to child care earlier affects children's later skill development and educational outcomes. We address this question and examine the causal effect of early child care attendance on children's short- and medium-run outcomes, including measures of cognitive and non-cognitive skills, using a fuzzy-regression discontinuity design. We combine survey data from the German National Education Panel Survey (NEPS), administrative data from school censuses and school entrance examinations for a cohort of children born between 1994 and 1996. For identification, we exploit a discontinuity in average child care starting age for children occurring between children born in the last quarter and the first quarter of the next year. As this discontinuity does not affect children's age at school entrance, starting child care earlier increases the duration of child care attendance.

We find no systematic differences in background characteristics of children born before and after the December/January threshold lending credibility to the assumption that month of birth is as good as randomly assigned in our sample. The first stage is strong and robust to the specification of the underlying control function and the inclusion of control variables. Overall, we find no effect of starting child care earlier on children's test scores in grade 9 (at the ages of 15-16), neither on measures of non-cognitive skills, nor on school track choice. In particular, our subgroup analysis reveals no evidence for positive effects on children from non-native and low socio-economic backgrounds.

Given that we can rule out large effects of early child care attendance on children's development in grade 9 for the group of compliers, we turn to the question whether early child care ever had an effect on these children's skill development. To this end, we use administrative data from compulsory school entrance examinations conducted at the ages of 5-6 by public health paediatricians. We find no evidence that starting child care earlier on average significantly affects children's school readiness, motor skills, behavioural problems, or language competencies. Again, we do not find any substantial differences by children's socio-economic status or migration background. Only for girls do we find a small improvement in school readiness; however, this effect is not robust. As we furthermore do not find any positive effects for girls at the age of 15, any potential short-run effects are too minor to influence the later acquisition of skills.

To rationalise the result, we explore several explanations and conclude that neither the length, timing, nor content of the treatment can explain the result. Instead, we argue that we do not find any effect because individuals shifted into early child care by our instrument are unlikely to have large gains from early child care, as suggested by Cornelissen et al. (2015). Indeed, we show that our IV estimates give most weight to individuals with a strong preference for, or low resistance against, early child care. Therefore, our results are consistent with Cornelissen et al. (2015) who also find no effect on average and only a positive effect for school readiness of children with a high resistance to attending child care early. While our approach is unsuited to estimate the effect for children of parents with a high resistance, we furthermore extend the literature by documenting no short- or medium-run effects on cognitive and non-cognitive skills for children with a low resistance to early child care.

As early child care intends to improve mothers' access to labour markets and children's skill development, particularly for children with low socio-economic status, our findings bear important policy insights. First, that we do not find a detrimental effect of starting child care earlier for children whose parents have a low unobserved resistance to early child care, while others, e.g. Bauernschuster and Schlotter (2015), find that child care provision improves maternal labour market participation, shows that both goals are not conflicting for this subgroup. Second and more importantly, not finding an advantageous effect for children of non-native parents and less educated parents highlights the need to further scrutinize how policy makers can achieve both goals simultaneously for children with low socio-economic status. Given these findings, simply easing earlier access to child care for these children, e.g., by providing more slots, does not influence their skill development.

These conclusions are, however, limited to starting child care around the age of three. Future research needs to examine how starting child care at even earlier ages, i.e., between zero and two years, affects children's skill development in the short- and longer-run.

Acknowledgements We are grateful for helpful comments and suggestions received by Anna Adamecz-Völgyi, Stefan Bauernschuster, Christian Dustmann, James Heckman, Mathias Huebener, Chris Karbownik, Patrick Puhani, Regina T. Riphahn, Claus Schnabel, Stefanie Schurer, Steven Stillman, Konstantinos Tatsiramos, Rudolf Winter-Ebmer, and from participants at the 2015 summer school of the DFG SPP 1764, the 2016 Ce^2 workshop in Warsaw, the 3rd network meeting of the DFG SPP 1764, and seminar participants at RWI (Essen) and the Northwestern Applied Micro Reading Group. This paper uses data from the National Educational Panel Study (NEPS): Starting Cohort 4-9th Grade, doi:10.5157/NEPS:SC4:4.0.0. From 2008 to 2013, NEPS data were collected as part of the Framework Programme for the Promotion of Empirical Educational Research funded by the German Federal Ministry of Education and Research (BMBF). As of 2014, the NEPS survey is carried out by the Leibniz Institute for Educational Trajectories (LIfBi) at the University of Bamberg in cooperation with a nationwide network. We thank the Ministry of Social Affairs, Health, Science and Equality of Schleswig Holstein, and Ute Thyen and Sabine Brehm specifically, for granting access to and providing valuable information on the school entrance examinations. Daniel Kuehnle acknowledges financial support by the DFG SPP 1764.

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Table 1: Descriptives: covariate balancing.

	before=1	before=0	diff. (Δ)	t-statistic	p-value	normalised Δ
Panel A: Based on children's information						
Child male	.51	.5	.01	-1.44	.15	-.03
Child age	15.35	14.99	.36	-39.84	0	-.81
Mother non-native speaker	.2	.19	.01	-.97	.33	-.02
Mother born abroad	.22	.2	.01	-1.37	.171	-.03
Partner non-native speaker	.15	.13	.02	-2.27	.024	-.05
Partner born abroad	.22	.2	.02	-2.32	.021	-.05
Mother's education						
Missing	.25	.26	0	.42	.673	.01
None/lower secondary	.19	.17	.02	-2.53	.011	-.05
Medium secondary	.32	.32	0	-.38	.705	-.01
Upper secondary	.13	.15	-.02	2.29	.022	.05
Any tertiary	.1	.1	0	.53	.596	.01
Partner's education						
Missing	.32	.31	0	-.45	.655	-.01
None/lower secondary	.21	.2	0	-.53	.595	-.01
Medium secondary	.23	.22	.01	-1.42	.156	-.03
Upper secondary	.12	.14	-.02	2.84	.005	.06
Any tertiary	.13	.13	0	.2	.844	0
Mother's occupational status						
Missing	.39	.38	.01	-.57	.566	-.01
1st quartile	.12	.11	.02	-2.77	.006	-.06
2nd quartile	.16	.16	0	-.02	.987	0
3rd quartile	.15	.15	-.01	1.12	.262	.02
4th quartile	.18	.2	-.02	1.97	.049	.04
Father's occupational status						
Missing	.36	.36	0	-.1	.917	0
1st quartile	.15	.14	.01	-1.32	.188	-.03
2nd quartile	.17	.16	.01	-1.51	.132	-.03
3rd quartile	.15	.17	-.02	2.14	.032	.04
4th quartile	.16	.17	-.01	.79	.428	.02
N	4766	4942	.	.	.	.

continued on the next page.

Table 1 - continued from previous page.

	before=1	before=0	diff. (Δ)	t-statistic	p-value	normalised Δ
Panel B: Based on parents' information						
Parent's age	44.8	44.74	.06	-.46	.646	-.01
Mother non-native speaker	.13	.12	.01	-1.13	.26	-.03
Mother born abroad	.14	.13	.01	-1.57	.117	-.04
Living arrangements						
Married/cohabiting	.76	.77	0	.03	.977	0
Divorced/Separated	.17	.18	-.01	.65	.517	.02
Widowed/Single	.06	.05	.01	-1.12	.261	-.03
Household size	4.02	4.05	-.03	.58	.56	.02
Mother's education						
Missing	0	0	0	-2.57	.01	-.07
None	.02	.02	0	-.47	.64	-.01
Lower secondary	.1	.09	.01	-1.86	.062	-.05
Medium secondary	.48	.47	0	-.19	.847	-.01
Upper secondary	.14	.14	0	.09	.929	0
Any tertiary	.16	.18	-.02	1.79	.073	.05
Mother's work status						
Employed - Full time	.32	.32	0	-.24	.812	-.01
Employed - Part time	.52	.53	-.01	.98	.327	.03
Not employed	.16	.15	.01	-1.04	.298	-.03
Partner non-native speaker	.14	.13	.01	-.79	.427	-.03
Partner born abroad	.15	.15	0	-.06	.95	0
Partner's age	46.39	46.42	-.03	.16	.875	0
Partner's education						
Missing	.01	.01	0	-.14	.885	0
None	.01	.01	0	-1.56	.119	-.05
Lower secondary	.1	.08	.02	-1.68	.092	-.05
Medium secondary	.45	.45	0	.01	.993	0
Upper secondary	.18	.18	0	-.35	.724	-.01
Any tertiary	.25	.28	-.02	1.78	.075	.06
Partner's work status						
Employed - Full time	.77	.78	-.01	.62	.533	.02
Employed - Part time	.12	.13	0	.46	.647	.01
Not employed	.11	.09	.01	-1.36	.173	-.04
Family income						
1st quartile	.11	.11	0	-.24	.813	0
2nd-3rd quartile	.24	.25	-.01	.58	.562	.01
4th quartile	.12	.12	0	-.25	.803	-.01
Missing	.53	.52	0	-.19	.849	0
N	2550	2701	.	.	.	

continued on the next page.

Table 1 - continued from previous page.

	before=1	before=0	diff. (Δ)	t-statistic	p-value	normalised Δ
Panel C: Official child care statistics						
Ratio child care spots per child in 1994	.86	.87	0	.84	.399	.02
1st quintile	.27	.26	.01	-.9	.369	-.02
2nd quintile	.22	.22	0	-.02	.981	0
3rd quintile	.15	.15	0	.32	.748	.01
4th quintile	.16	.16	0	.22	.826	.01
5th quintile	.2	.21	-.01	.53	.598	.01
Ratio child care spots per child in 1998	1.04	1.04	0	.44	.659	.01
1st quintile	.18	.19	-.01	.81	.416	.02
2nd quintile	.24	.23	.01	-.7	.487	-.02
3rd quintile	.23	.21	.01	-1.32	.186	-.04
4th quintile	.13	.14	-.01	.8	.424	.02
5th quintile	.22	.23	-.01	.61	.544	.02
Children per child care worker	7.7	7.64	.06	-1.69	.091	-.05
1st quintile	.13	.14	-.01	.8	.422	.02
2nd quintile	.25	.28	-.03	2.55	.011	.07
3rd quintile	.24	.21	.03	-2.93	.003	-.08
4th quintile	.25	.25	0	.39	.694	.01
5th quintile	.13	.12	.01	-1.05	.292	-.03
N	4766	4942	.	.	.	.

Notes: Descriptive statistics using a 5-months window around December/January threshold. Before=1 if a child is born between August and December, 0 otherwise.

Source: Own calculations based on NEPS Starting Cohort 4 and Official Statistics.

Table 2: First stage: effect of being born before December 31st on child care starting age.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
5-month window ($N = 5,243$)	-0.402***	-0.408***	-0.403***	-0.407***	-0.421***	-0.420***	-0.472***
	(0.041)	(0.041)	(0.041)	(0.041)	(0.040)	(0.040)	(0.064)
First-stage F	98.07	101.18	98.19	100.87	111.65	111.07	55.08
3-month window ($N = 3,190$)	-0.382***	-0.390***	-0.384***	-0.393***	-0.428***	-0.431***	-0.436***
	(0.054)	(0.054)	(0.054)	(0.054)	(0.054)	(0.054)	(0.102)
First-stage F	49.39	51.49	49.83	52.03	62.23	63.09	18.45
Month of birth controls	Linear	Linear	Linear	Linear	Linear	Linear	Quadratic
interacted with cut-off	yes	yes	yes	yes	yes	yes	yes
interacted with cohort	no	no	yes	yes	no	yes	yes
Control variables	no	yes	no	yes	yes	yes	yes
district FE	no	no	no	no	yes	yes	yes

Notes: Control variables as listed in Table 1, Panel A. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Source: Own calculations based on NEPS Starting Cohort 4.

Table 3: First stage: effect of being born in last quarter on child care starting age

	All	Mother's education		Mother's ethnicity		Child's gender	
		Low-medium	High	Native	Non-native	Boys	Girls
		(Ref.)	(Int.)	(Ref.)	(Int.)	(Ref.)	(Int.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Child's information	-0.420***	-0.409***	0.001	-0.415***	-0.097	-0.421***	0.001
	(0.040)	(0.057)	(0.095)	(0.041)	(0.155)	(0.057)	(0.081)
First-stage F	111.074	51.023	28.071	102.392	8.654	53.716	53.434
N	5243	2585	1563	4626	619	2714	2529
Panel B: Parents' information	-0.418***	-0.448***	0.082	-0.419***	-0.009	-0.425***	0.010
	(0.040)	(0.053)	(0.083)	(0.041)	(0.159)	(0.057)	(0.081)
First-stage F	111.733	72.040	32.329	104.907	6.013	54.880	51.408
N	5242	3030	2212	4646	598	2714	2528

Notes: Specifications as in Column 6 of Table 2 and fully interacted with the respective subgroup indicator variable. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Source: Own calculations based on NEPS Starting Cohort 4, full sample, 5-months window around December/January threshold.

Table 4: Reduced form effect of early child care attendance on test scores.

	Reduced form	Reduced form	Reduced form	2S-2SLS
Language overall (N=9131)	0.035	0.062*	0.028	-0.065
	(0.041)	(0.037)	(0.035)	(0.080)
R-squared	0.056	0.218	0.361	
STEM overall (N=9131)	0.006	0.022	0.006	-0.013
	(0.041)	(0.038)	(0.035)	(0.081)
R-squared	0.058	0.231	0.373	
Cognition overall (N=8526)	-0.010	0.007	-0.000	0.000
	(0.043)	(0.042)	(0.041)	(0.101)
R-squared	0.020	0.050	0.165	
Extraversion (N=8660)	0.012	0.020	0.010	-0.023
	(0.044)	(0.043)	(0.044)	(0.095)
R-squared	0.000	0.020	0.045	
Agreeableness (N=8660)	0.015	0.026	0.038	-0.085
	(0.043)	(0.043)	(0.043)	(0.094)
R-squared	0.001	0.022	0.052	
Conscientiousness (N=8660)	0.009	0.021	0.035	-0.076
	(0.044)	(0.043)	(0.043)	(0.095)
R-squared	0.001	0.047	0.081	
Neuroticism (N=8660)	-0.060	-0.044	-0.038	0.084
	(0.044)	(0.043)	(0.043)	(0.094)
R-squared	0.001	0.054	0.078	
Openness (N=8660)	0.034	0.058	0.051	-0.112
	(0.043)	(0.042)	(0.043)	(0.094)
R-squared	0.001	0.059	0.087	
SDQ - Pro-Social (N=7211)	0.003	0.024	0.029	-0.072
	(0.046)	(0.045)	(0.046)	(0.109)
R-squared	0.001	0.059	0.097	
SDQ - Peer problems (N=7211)	0.026	0.016	0.011	-0.026
	(0.048)	(0.048)	(0.049)	(0.116)
R-squared	0.009	0.023	0.063	
Control variables	no	yes	yes	yes
District FE	no	no	yes	yes

Notes: Specification as in Column 6 of Table 2. Robust standard errors in parentheses; standard errors on the 2S-2SLS estimates are calculated using the delta method. *** p<0.01, ** p<0.05, * p<0.1
Source: Own calculations based on NEPS Starting Cohort 4, full sample, 5-months window around December/January threshold.

Table 5: Reduced form effect of early child care attendance on test scores.

	All	Low Edu	High Edu	Native	Non-Native	Boys	Girls
Language overall	0.028	0.037	0.025	-0.006	0.205**	-0.013	0.054
	(0.035)	(0.047)	(0.073)	(0.037)	(0.099)	(0.049)	(0.051)
N	9131	4652	2360	7424	1707	4597	4534
R-squared	0.361	0.328	0.393	0.326	0.374	0.357	0.401
STEM overall	0.006	0.019	-0.032	-0.014	0.080	-0.028	0.048
	(0.035)	(0.047)	(0.080)	(0.039)	(0.084)	(0.052)	(0.048)
N	9131	4652	2359	7424	1707	4597	4534
Cognition overall	-0.000	-0.017	0.017	-0.045	0.214*	-0.051	0.038
	(0.041)	(0.058)	(0.078)	(0.044)	(0.113)	(0.060)	(0.057)
N	8526	4320	2218	6947	1579	4296	4230
Extraversion	0.010	-0.028	0.026	-0.001	0.042	-0.036	0.057
	(0.044)	(0.063)	(0.091)	(0.049)	(0.105)	(0.061)	(0.065)
N	8660	4448	2258	7081	1579	4316	4344
Agreeableness	0.038	0.017	0.004	0.024	0.084	-0.005	0.089
	(0.043)	(0.060)	(0.092)	(0.047)	(0.118)	(0.063)	(0.060)
N	8660	4448	2258	7081	1579	4316	4344
Conscientiousness	0.035	0.044	0.066	0.041	0.102	-0.016	0.088
	(0.043)	(0.061)	(0.089)	(0.048)	(0.115)	(0.062)	(0.062)
N	8660	4448	2258	7081	1579	4316	4344
Neuroticism	-0.038	-0.103*	-0.005	-0.042	-0.002	-0.094	-0.005
	(0.043)	(0.062)	(0.091)	(0.048)	(0.113)	(0.062)	(0.062)
N	8660	4448	2258	7081	1579	4316	4344
Openness	0.051	0.000	0.108	0.041	0.088	0.055	0.068
	(0.043)	(0.061)	(0.089)	(0.047)	(0.112)	(0.063)	(0.060)
N	8660	4448	2258	7081	1579	4316	4344
SDQ - Pro-Social	0.029	-0.005	-0.024	0.033	-0.056	-0.033	0.098
	(0.046)	(0.063)	(0.102)	(0.051)	(0.121)	(0.070)	(0.061)
N	7211	3625	1784	5830	1381	3540	3671
SDQ - Peer problems	0.011	0.101	-0.153	-0.011	0.133	0.075	-0.025
	(0.049)	(0.069)	(0.100)	(0.055)	(0.118)	(0.075)	(0.065)
N	7211	3625	1784	5830	1381	3540	3671

Notes: Specification as in Column 6 of Table 2. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Source: Own calculations based on NEPS Starting Cohort 4, full sample, 5-months window around December/January threshold.

Table 6: Reduced form effect of early child care attendance on track choice in Grade 9

	All	Native	Non-Native
Panel A: Gymnasium			
All	-0.011	-0.014	0.006
	(0.009)	(0.010)	(0.021)
N	102523	90892	11631
Boys	-0.009	-0.014	0.024
	(0.013)	(0.014)	(0.029)
N	52922	46895	6027
Girls	-0.014	-0.014	-0.015
	(0.013)	(0.014)	(0.032)
N	49601	43997	5604
Panel B: Hauptschule			
All	0.005	0.005	0.009
	(0.010)	(0.010)	(0.029)
N	102523	90892	11631
Boys	0.013	0.016	0.010
	(0.014)	(0.014)	(0.040)
N	52922	46895	6027
Girls	-0.004	-0.007	0.011
	(0.013)	(0.014)	(0.042)
N	49601	43997	5604

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source: Own calculations based on administrative data from the Bavarian school census. 5 months-window around December/January threshold.

Table 7: Reduced form effect of early child care attendance on school entrance exams.

	All	Low Edu	High Edu	Native	Non-Native	Boys	Girls
Panel A: Birth cohorts 1995m7-1996m6							
Not school ready	-0.007	-0.005	0.035	-0.007	0.034	0.006	-0.019*
(mean=0.052)	(0.008)	(0.012)	(0.030)	(0.009)	(0.032)	(0.013)	(0.010)
N	23593	10591	2525	16444	2295	12026	11567
Speech incompetent	-0.003*	-0.000	0.001	-0.001	-0.005	-0.002	-0.004
(mean=0.063)	(0.002)	(0.002)	(0.002)	(0.002)	(0.007)	(0.002)	(0.002)
N	23593	10591	2525	16444	2295	12026	11567
Behavioural difficulties	0.009	0.007	0.023	0.003	0.026	0.023	-0.005
(mean=0.068)	(0.010)	(0.015)	(0.032)	(0.013)	(0.031)	(0.016)	(0.013)
N	23593	10591	2525	16444	2295	12026	11567
Motor skill difficulties	0.007	0.005	-0.046	0.005	0.014	0.008	0.008
(mean=0.131)	(0.014)	(0.021)	(0.042)	(0.017)	(0.040)	(0.023)	(0.015)
N	23593	10591	2525	16444	2295	12026	11567
Panel B: Birth cohorts 1995m7-1997m6							
Not school ready	-0.010**	-0.017**	0.005	-0.009	-0.028	-0.000	-0.020***
(mean=0.048)	(0.005)	(0.007)	(0.019)	(0.006)	(0.022)	(0.008)	(0.007)
N	48925	23355	5569	34855	4951	25041	23884
Speech incompetent	-0.002	-0.000	-0.005	0.000	-0.006	-0.004	-0.000
(mean=0.060)	(0.002)	(0.003)	(0.006)	(0.002)	(0.007)	(0.003)	(0.003)
N	48925	23355	5569	34855	4951	25041	23884
Behavioural difficulties	0.004	0.003	0.021	0.005	-0.024	0.010	-0.002
(mean=0.071)	(0.007)	(0.010)	(0.022)	(0.009)	(0.022)	(0.012)	(0.009)
N	48925	23355	5569	34855	4951	25041	23884
Motor skill difficulties	-0.000	-0.022	0.005	-0.003	-0.025	0.002	-0.001
(mean=0.140)	(0.010)	(0.015)	(0.031)	(0.013)	(0.029)	(0.016)	(0.011)
N	48925	23355	5569	34855	4951	25041	23884

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source: Own calculations based on administrative school entrance examinations from Schleswig-Holstein, 5-months window around December/January threshold.

Table 8: Subgroups analysis: reduced form effect of early child care attendance on school entrance exams.

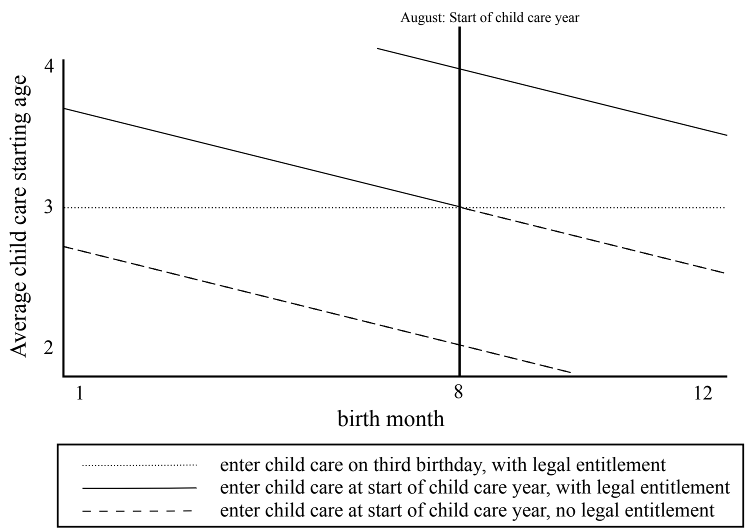
	All	Low Edu	High Edu	Native	Non-Native
Panel A: Boys (Birth cohorts 1995m7-1997m6)					
Not school ready	-0.000	-0.016	0.036	-0.003	-0.015
(mean=.062)	(0.008)	(0.011)	(0.030)	(0.009)	(0.033)
N	25041	11971	2823	18001	2504
Speech incompetent	-0.004	-0.005	-0.006	-0.002	-0.012
(mean=.057)	(0.003)	(0.004)	(0.007)	(0.003)	(0.012)
N	25041	11971	2823	18001	2504
Behavioural problems	0.010	0.000	0.037	0.011	-0.027
(mean=.093)	(0.012)	(0.017)	(0.036)	(0.014)	(0.034)
N	25041	11971	2823	18001	2504
Motor skills	0.002	-0.025	0.009	-0.001	-0.038
(mean=.203)	(0.016)	(0.024)	(0.051)	(0.020)	(0.049)
N	25041	11971	2823	18001	2504
Panel B: Girls (Birth cohorts 1995m7-1997m6)					
Not school ready	-0.020***	-0.017*	-0.027	-0.014*	-0.039
(mean=.032)	(0.007)	(0.009)	(0.022)	(0.007)	(0.028)
N	23884	11384	2746	16854	2447
Speech incompetent	-0.000	0.005	-0.004	0.002	0.000
(mean=.063)	(0.003)	(0.004)	(0.009)	(0.003)	(0.008)
N	23884	11384	2746	16854	2447
Behavioural problems	-0.002	0.005	0.004	-0.000	-0.018
(mean=.047)	(0.009)	(0.012)	(0.025)	(0.011)	(0.027)
N	23884	11384	2746	16854	2447
Motor skills	-0.001	-0.018	0.001	-0.008	0.007
(mean=.074)	(0.011)	(0.016)	(0.035)	(0.014)	(0.029)
N	23884	11384	2746	16854	2447

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

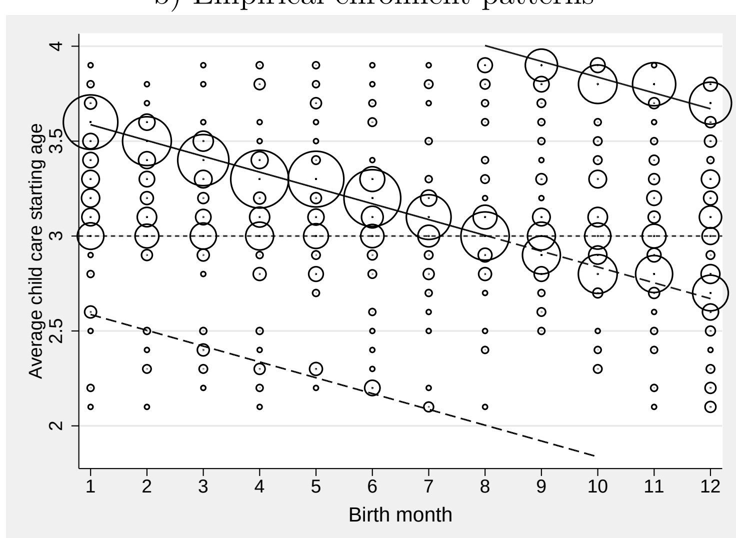
Source: Own calculations based on administrative school entrance examinations from Schleswig-Holstein, 5-months window around December/January threshold.

Figure 1: Child care enrolment patterns

a) Theoretical enrolment patterns



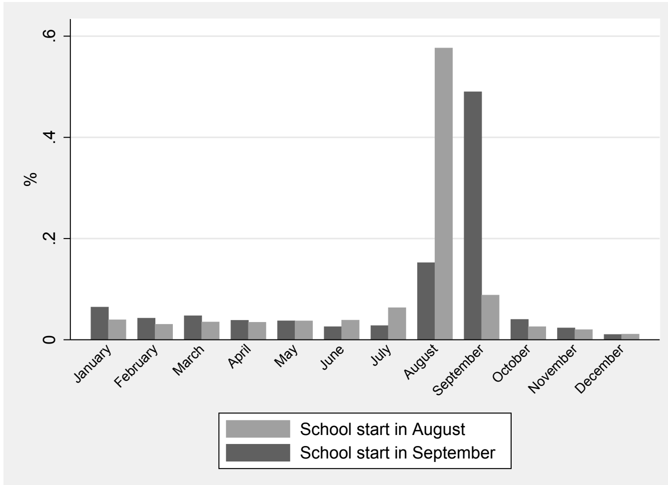
b) Empirical enrolment patterns



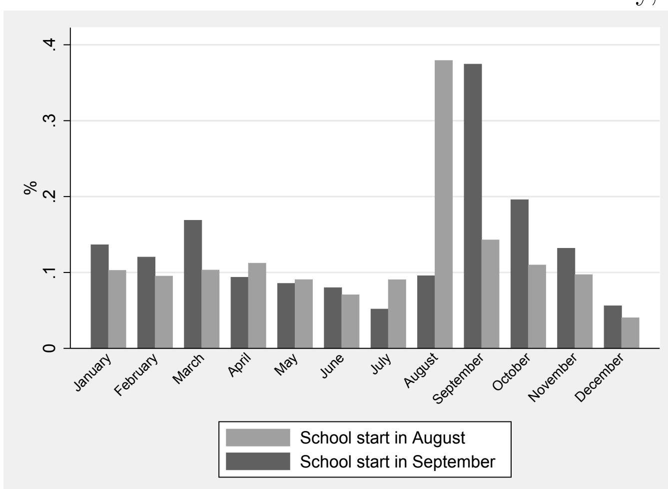
Notes: Panel B uses the NEPS Starting Cohort 4 and proportionally weights the data points according to the number of children enrolling at a given age, by birth month. Own compilations.

Figure 2: Empirical evidence on enrolment patterns.

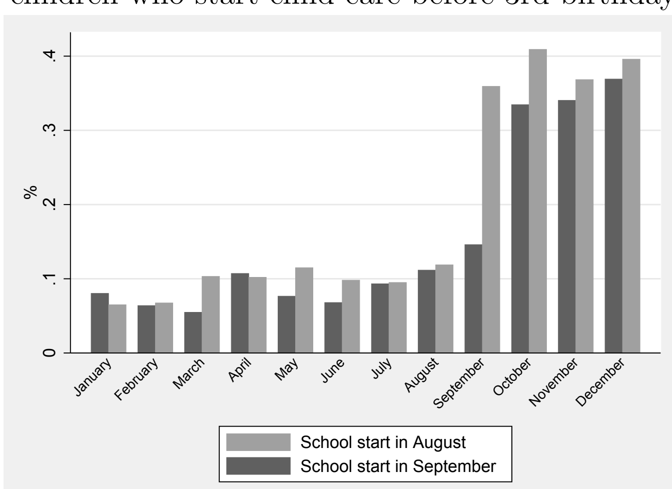
a) Calendar month of child care entrance



b) Proportion of children who start child care on 3rd birthday, by birth month



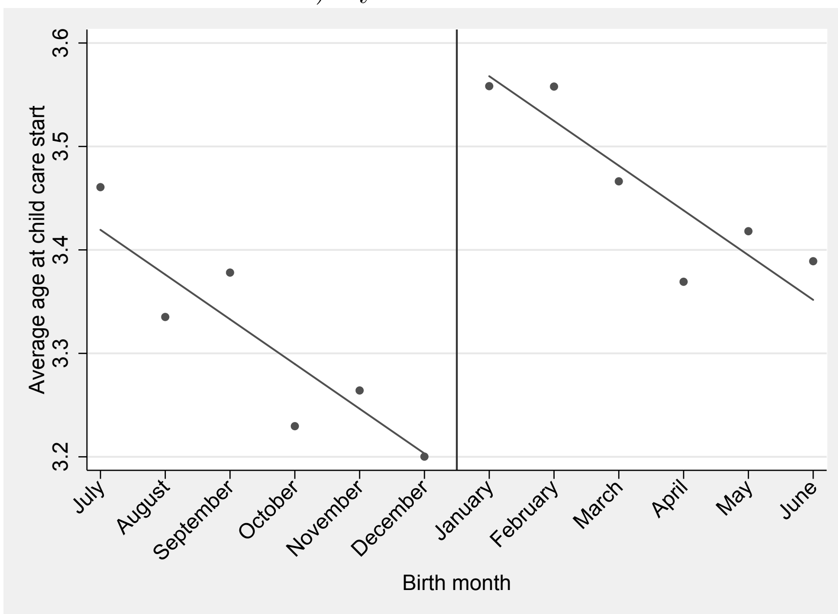
c) Proportion of children who start child care before 3rd birthday, by birth month



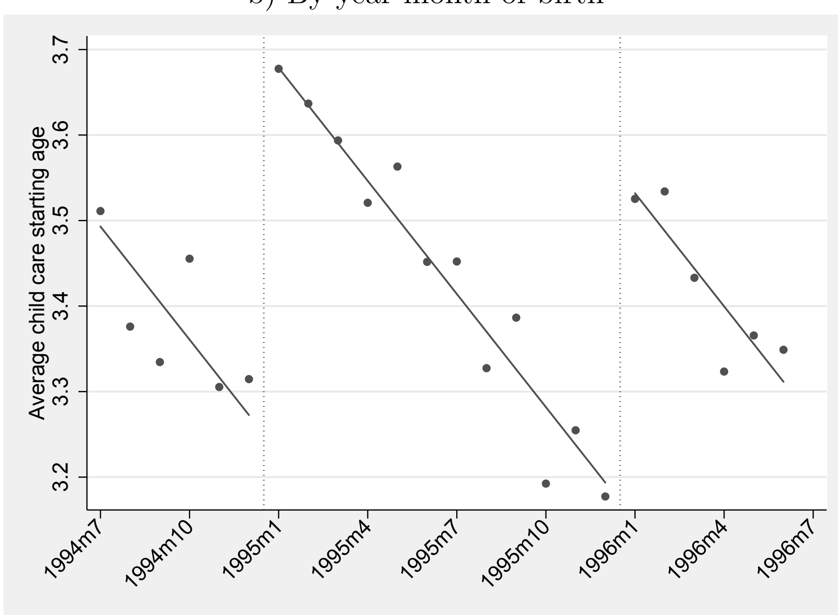
Source: NEPS Starting Cohort 4.

Figure 3: First stage: average child care starting age.

a) By birth month

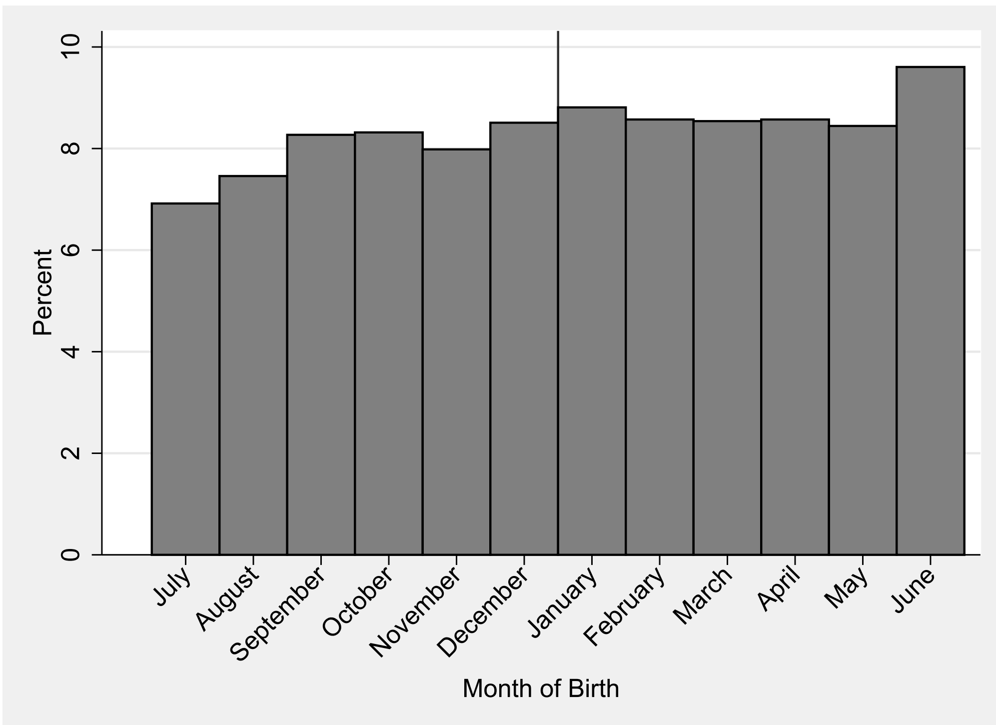


b) By year-month of birth



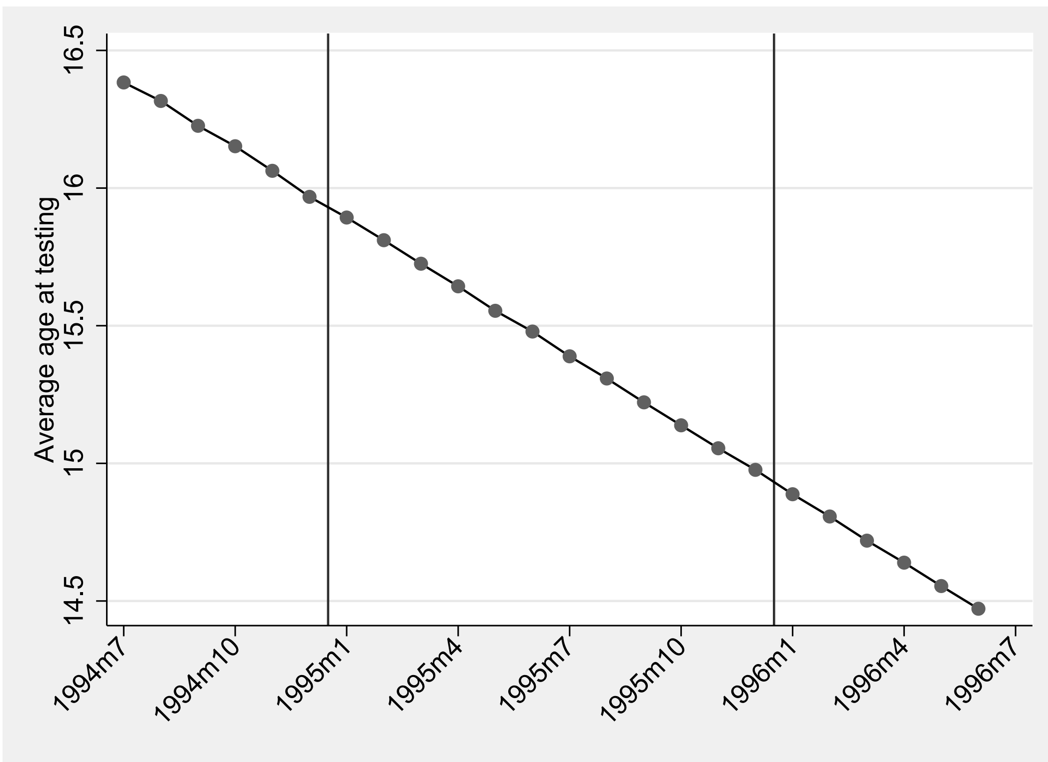
Source: NEPS Starting Cohort 4.

Figure 4: Distribution of births by month of birth.



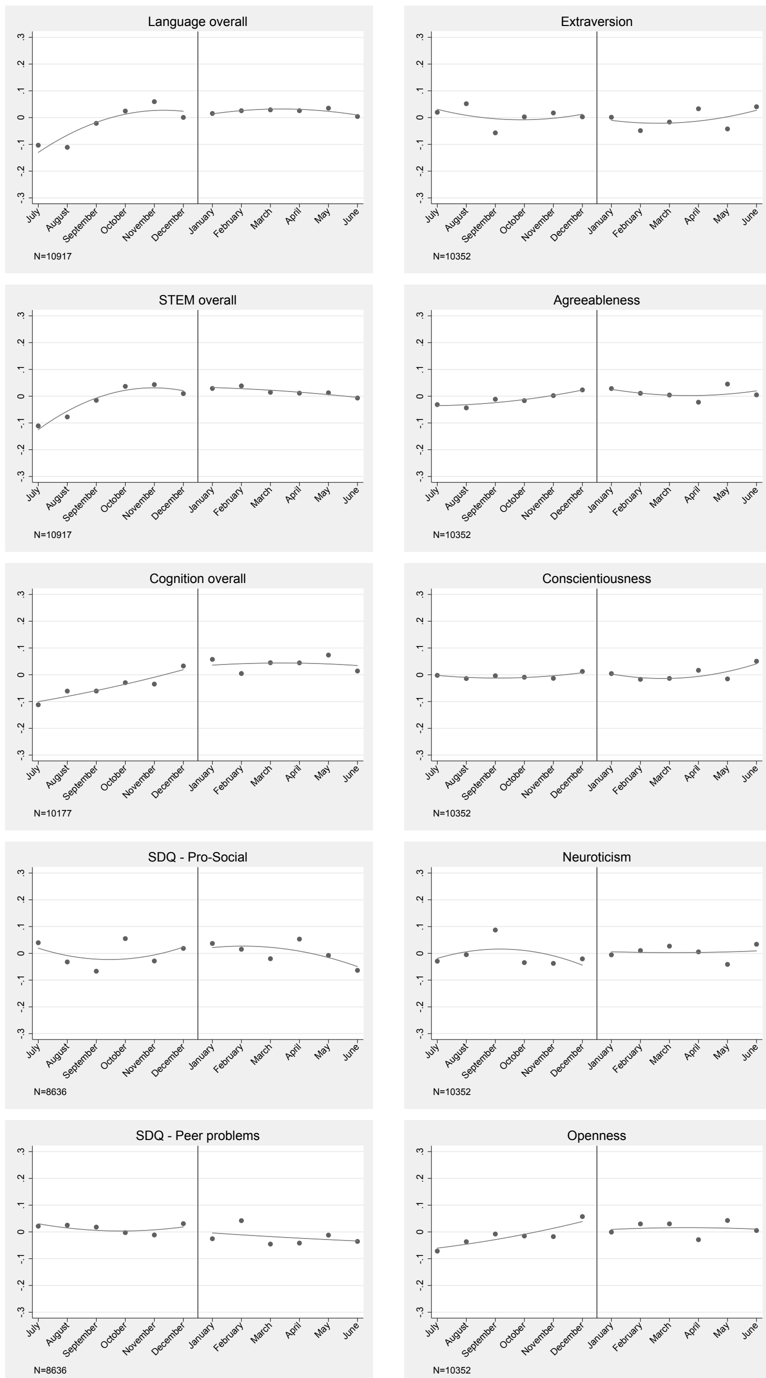
Source: NEPS Starting Cohort 4.

Figure 5: Average age at testing, by year-month of birth



Source: NEPS Starting Cohort 4.

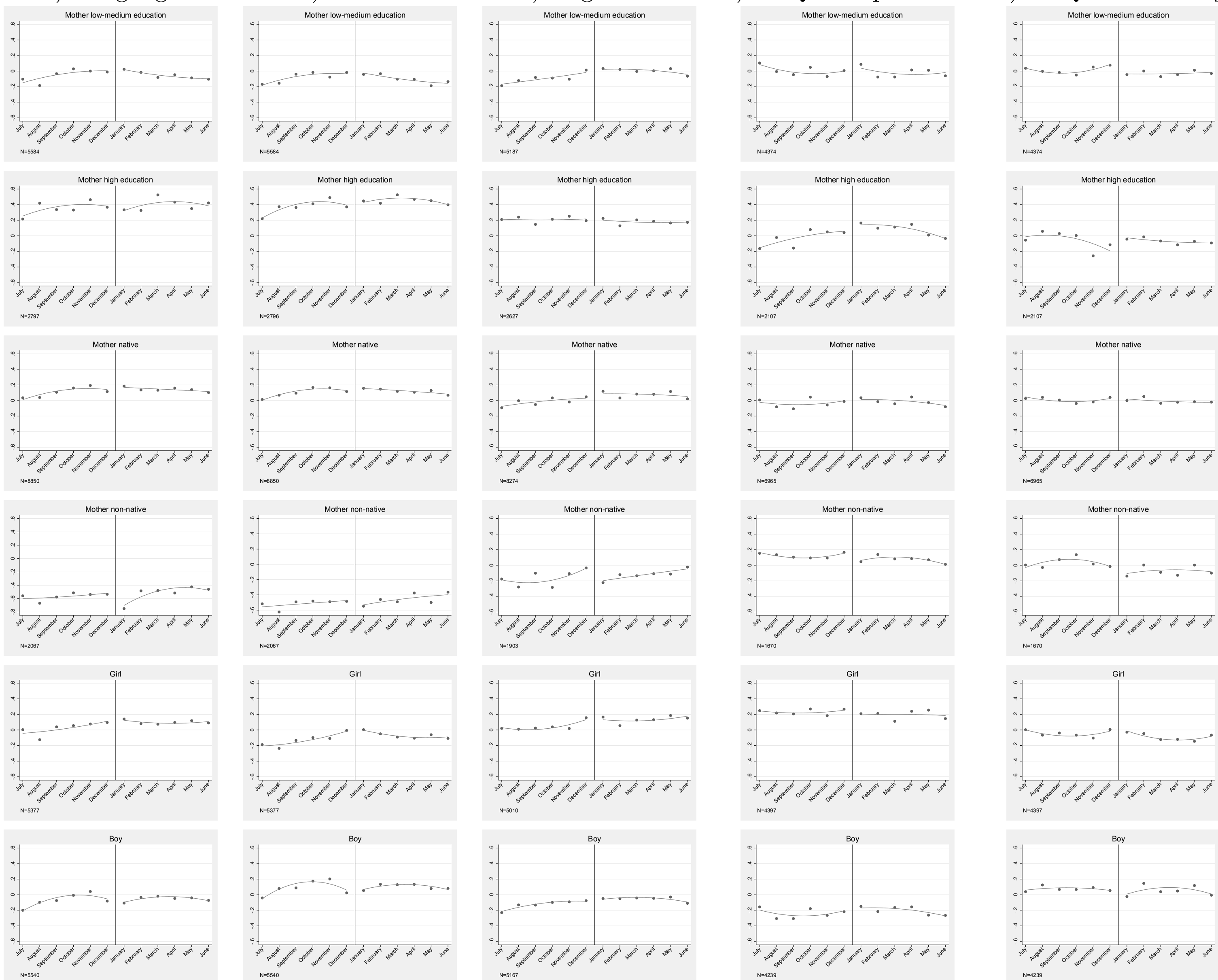
Figure 6: Reduced form effect on cognitive and non-cognitive skills.



Source: NEPS Starting Cohort 4. Based on full sample.

Figure 7: Reduced form: effect heterogeneity on cognitive skills and SDQ.

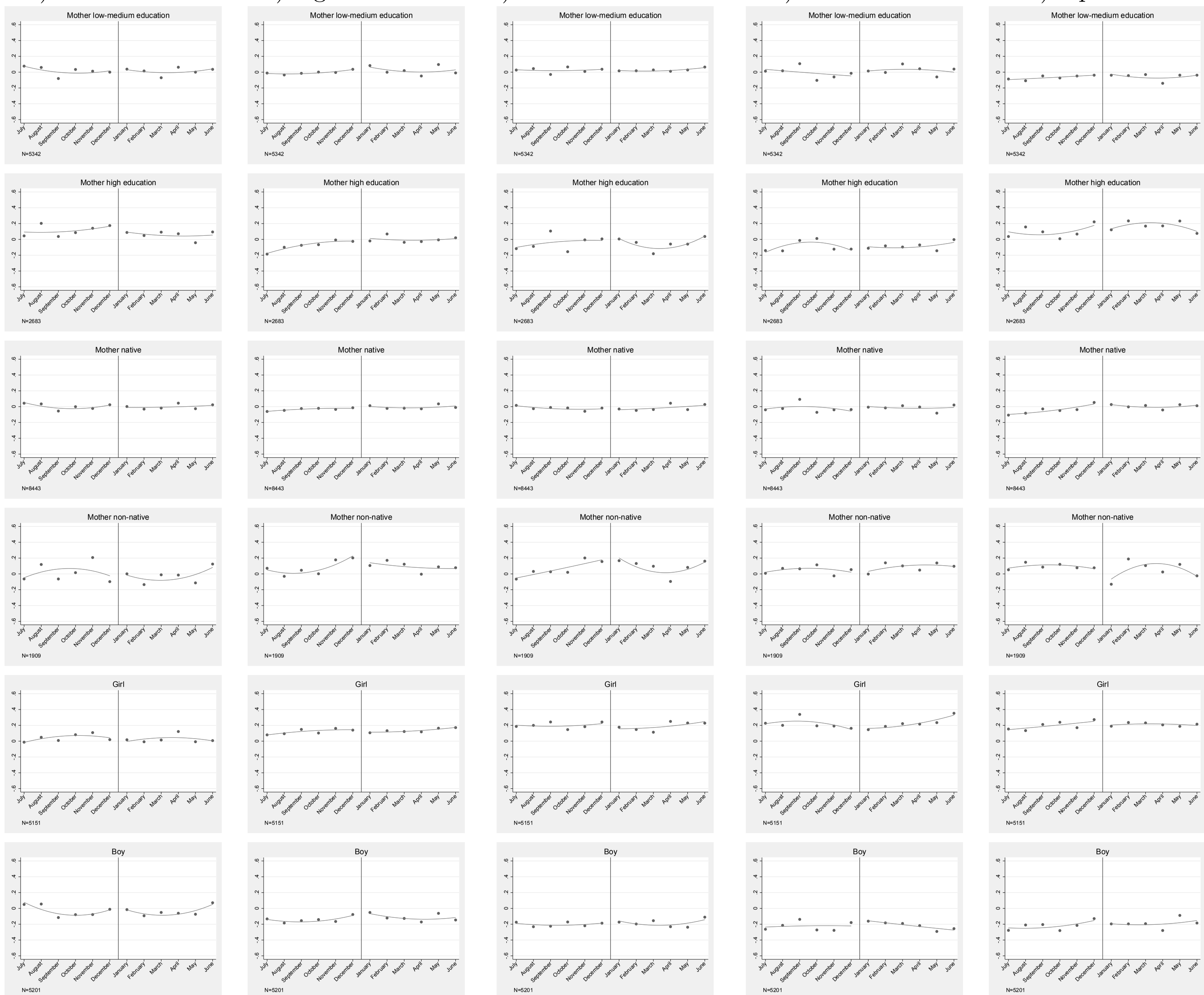
a) Language b) STEM c) Cognition d) SDQ Peer problems e) SDQ Pro-sociality



Source: NEPS Starting Cohort 4. Based on full sample.

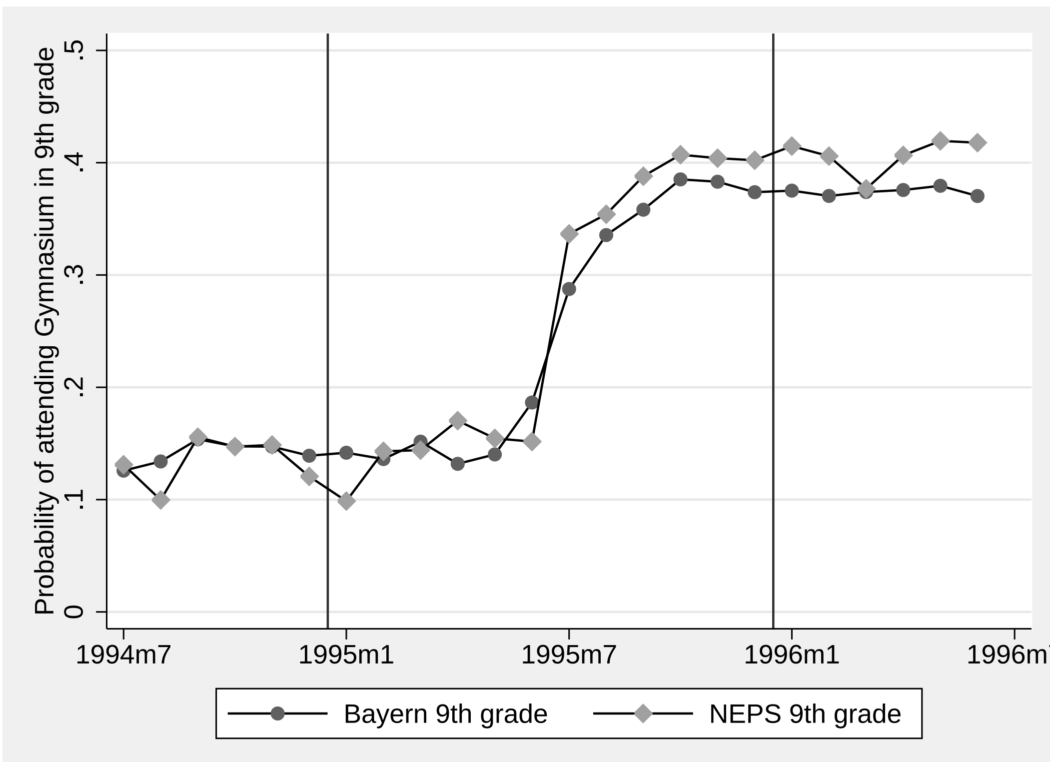
Figure 8: Reduced form: effect heterogeneity on ”Big-Five” personality skills.

a) Extraversion b) Agreeableness c) Conscientiousness d) Neuroticism e) Openness



Source: NEPS Starting Cohort 4. Based on full sample.

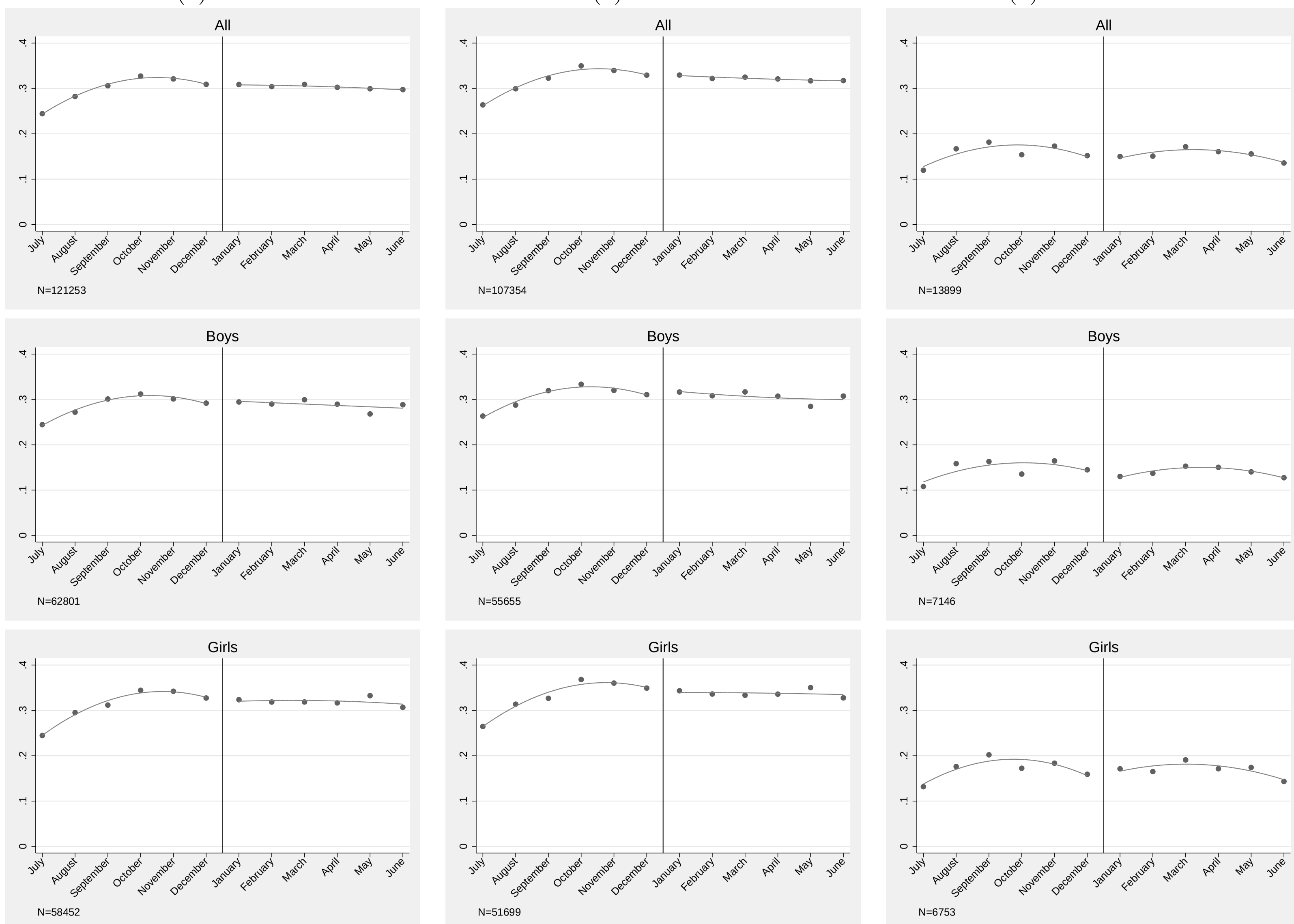
Figure 9: Reduced form: probability of attending academic school track (Gymnasium) in 9th grade.



Source: NEPS Starting Cohort 4 and administrative data from the Bavarian school census.

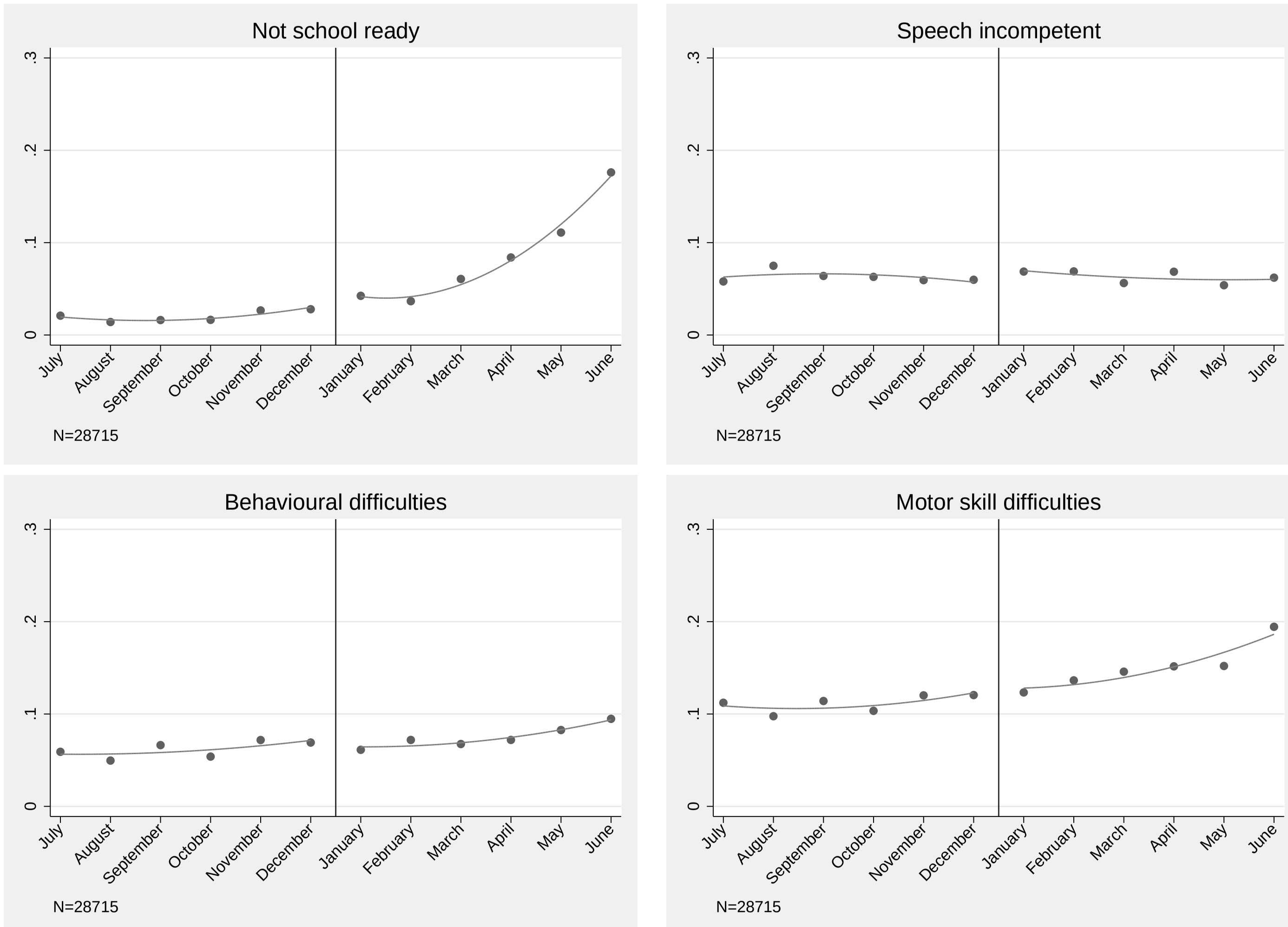
Figure 10: Reduced form: probability of attending academic school track (Gymnasium) in 9th grade.

(1) All (2) Natives (3) Non-natives



Source: Administrative data from the Bavarian school census.

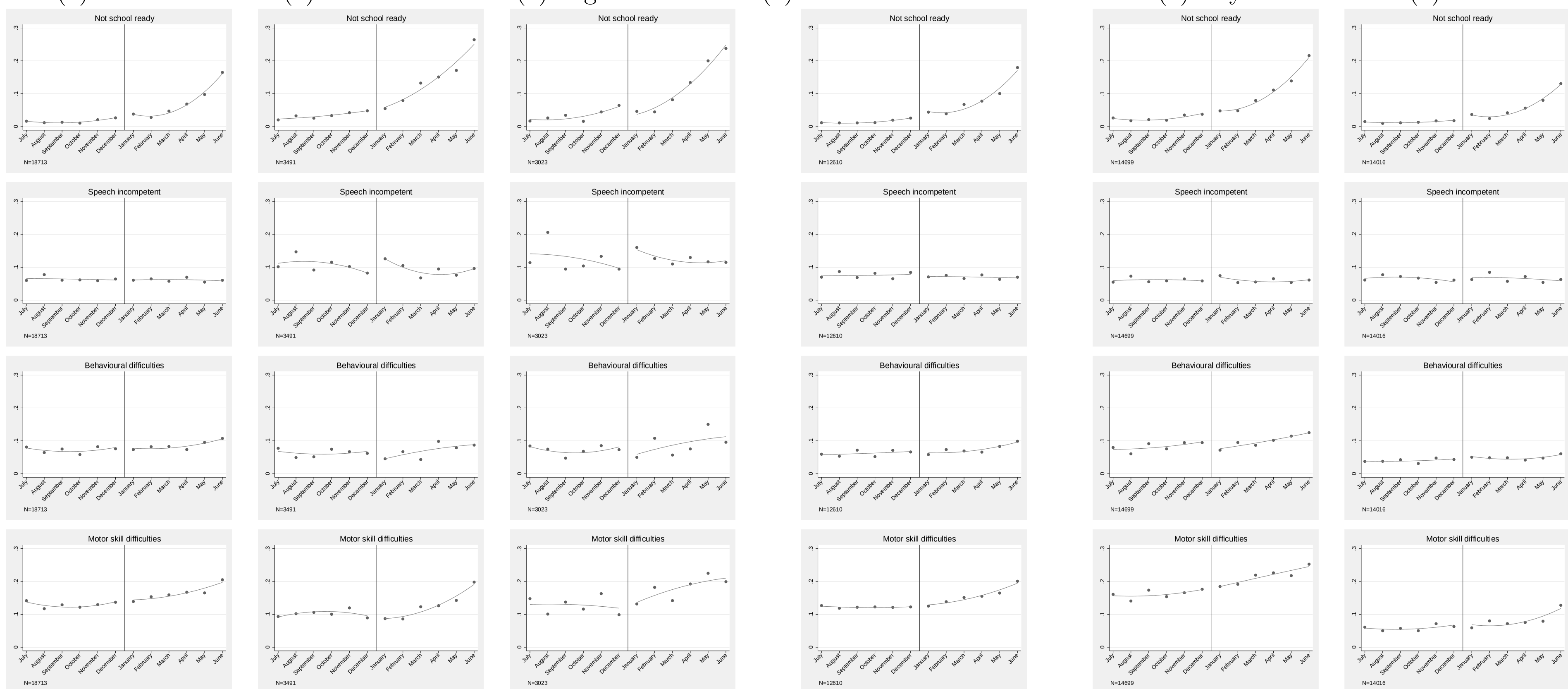
Figure 11: Reduced form: effect on skill development at age 6.



Source: Administrative data from school entrance examinations in Schleswig-Holstein. Birth cohorts 1995m7-1996m6.

Figure 12: Reduced form: effect on skill development at age 6.

(1) Natives (2) Non-natives (3) High education (4) Low-medium education (5) Boys (6) Girls



Source: Administrative data from school entrance examinations in Schleswig-Holstein. Birth cohorts 1995m7-1996m6.

Appendix A: Supplementary material

Skill measures in the NEPS

The NEPS provides standardised test scores to asses children's competencies in different dimensions, comprising the German language, maths, sciences, and computer literacy. German language skills are assessed on three domains: receptive vocabulary, reading speed, and reading competencies. The NEPS uses the German version of the Peabody Picture Vocabulary Test, where children need to select the right picture that corresponds to a given word. The test for reading speed is based on the Salzburg reading screenings (Auer et al., 2005) where the participants need to correctly judge the content of 51 sentences. Finally, the NEPS assesses children's reading competencies, i.e., the ability to understand and use written texts (Gehrer et al., 2012). We first standardise each measure to have mean 0 and standard deviation 1. We then generate an overall language skill index by averaging across the three standardised items for each individual, and standardising the resulting average language score.

The NEPS covers three out of the four STEM (Science, Technology, Engineering, Mathematics) subjects. The scientific test assesses student's competencies in problem solving, rather than reproduction of scientific knowledge. The test performs reliably with good psychometric properties (Schöps and Sass, 2013). The mathematical test requires students to solve mathematical problems, correctly use mathematical terms, and interpret tables or charts (Neumann et al., 2013). Finally, the information and communications technology (ICT) literacy test assesses students' ability to create, use, manage, and interpret information using digital media. For instance, students need to show proficiency in word processing, the use of the internet, search engines, and spreadsheet/presentation programs. As with the language skills, we again standardise each measure individually and compute a standardised average STEM score for each student.

Additionally, the NEPS survey administers two instruments that can be used as proxies for general intellectual abilities. First, the NEPS administers the Digit-Symbol-Test (DST) which builds on the Wechsler intelligence scale (Lang et al., 2007). The DST first provides individuals with a list of numbers and corresponding symbols; individuals are then required to match as many symbols as possible with the corresponding numbers within 90 seconds. Second, the NEPS administers a Raven Progressive Matrices test frequently used to proxy cognitive abilities, particularly fluid intelligence. In this test, individuals are presented with a series of matrices with each matrix displaying an array of objects, with one entry missing. Individuals need to use logical reasoning to fill in the void space of a matrix. We again standardise all scores to make effect sizes comparable.

Aside of cognitive skill measures, the NEPS also collects information on two well-established measures of non-cognitive skills, the Strength and Difficulties Questionnaire (SDQ) and the Big-Five personality traits. These skills are important outcomes as they strongly predict later educational outcomes, labour market performance, and risky behaviour, even conditioning on cognitive skills (e.g., see Bowles et al., 2001; Heckman and Kautz, 2012).

The SDQ (Goodman et al., 1998) is a widely used multi-dimensional measure for children's development. The NEPS collects information on Peer Problems and Pro-sociality. For each of these subscales, students are administered five questions yielding a score for each subscale that ranges from 0-10. We standardise each score with mean 0 and standard deviation 1.

Second, the NEPS administers the Big-Five personality inventory which is a widely accepted construct to describe differences in human character differences (Goldberg, 1981). The Big-Five inventory classifies personality traits into five broad factors: Openness to experience, Conscientiousness, Extraversion, Agreeableness, and Neuroticism (or Emotional Instability).[29] Numerous economic studies show that these personality traits strongly cor-

[29]For more information regarding these measures, see, e.g., Almlund et al. (2011).

relate with educational attainment (Lundberg, 2013), wages (Heineck and Anger, 2010), marital status (Lundberg, 2012), and health (Goodwin and Friedman, 2006). The NEPS administers a 10-item version of the Big-Five inventory (Gosling et al., 2003). We again standardise these measures.

Table A.1: Literature review

Study	Country	Treatment	Methodology	Age at testing	Main results
Baker et al. (2008)	Canada	Introduction of highly-subsidised child care for children between 2 and 4 years of age	Difference-in-differences (Quebec treated)	Ages 0–4	Negative effects on children's behavioural problems, physical and mental heath, and motor skills.
Berlinski et al. (2008)	Uruguay	Preschool attendance	Within-household estimator	Age 15	Positive effects on years of education
Berlinski et al. (2009)	Argentina	Expansion of access to pre-primary school	Difference-in-differences	Grade 3	Positive effects on behavioural outcome, mathematics and Spanish test scores.
Bernal and Keane (2011)	US	Attending child care (formal/informal) before age 5 for children of single mothers in the US	Exploits institutional changes in welfare rules	Ages 4-6	No effect of formal centre-based care; negative effects of informal care, i.e., non-centre based, children's cognitive achievements.
Blanden et al. (2016)	England	Free preschool education for 3 year olds	Difference-in-differences	Ages of 5 and 11	Very small short run effects which largely disappear by age 7, and fully by age 11.
Drange and Havnes (2015)	Oslo, Norway	Starting child care at 15 rather than 19 months	Assignment lottery of child care slots	7 years	Positive effects (+0.12SD) on maths and language test scores. The effects are stronger for children from low income backgrounds.
Esping-Andersen et al. (2012)	Denmark and US	Child care attendance at age 3	Control function	Age 11	Positive effects on cognitive skills and reading in Denmark; no effects on reading and maths scores in the US.
Felfe et al. (2015)	Spain	Subsided child care for 3-year olds	Difference-in-differences (using variation in expansion over time across states)	Age 15	positive effects on PISA reading test scores; stronger effects for girls and children from parents with low levels of education.

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Table A.1 – continued from previous page

Study	Country	Treatment	Methodology	Age at testing	Main results
Fitzpatrick (2008)	United States	Attending pre-Kindergarten (pre-K) at age 4	Difference-in-differences (Georgia treated)	Fourth grade (age 9)	Positive effects on grade retention, maths and readings test scores for children from disadvantaged families in rural areas.
Fort et al. (2016)	Bologna, Italy	Public daycare for children aged 0-2	Regression discontinuity design exploiting institutional details that govern whether a family receives free child care	Ages 8-14	Negative effects on IQ for girls, but no effects on conscientiousness or for boys.
Gormley et al. (2008)	Tulsa, Oklahoma (US)	Attending pre-Kindergarten (pre-K) at age 3-4	Regression discontinuity exploiting an age-based participation cut-off	Ages 4-5)	Positive effects on reading, writing, and maths scores children's cognitive skills.
Gupta and Simonsen (2010)	Denmark	Family day care and pre-school	IV using waiting lists that differ between municipalities as the IV	Age 7	No differential effect on SDQ between preschool and home care; negative effects of family day care, particularly for boys of mothers with low education.
Havnes and Mogstad (2011a)	Norway	Child care for children aged 3-6	Difference-in-differences	Age 30	Reform positively affected children's educational and labour market outcomes; the education effect is strongest for children of low educated mothers.
Havnes and Mogstad (2015)	Norway	Child care for children aged 3-6	(non-linear) difference-in-differences	Age 30	No effect on test scores, but on educational attainment; effects are generally large for lower quantiles of the conditional outcome distribution.

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Table A.1 – continued from previous page

Study	Country	Treatment	Methodology	Age at testing	Main results
Herbst (2013)	United States	Starting informal child care between 0-2 years	IV (instrument: interview timing and seasonal child care patterns)	9 and 24 months	Negative effects on cognitive test scores of non-parental care for children of high SES-families; no positive effects for children of low SES families.
Kottelenberg and Lehrer (2014)	Canada	Same as Baker et al. (2008), but differentiate at which age children start attending child care	Difference-in-differences (Quebec treated)	Ages 0–4	Negative effects for behavioural, developmental, and health outcomes for children who start child care before third birthday; no effects for children starting child care at ages 3 or 4.
Magnuson et al. (2007)	US	pre-Kindergarten programs	Teacher fixed-effects and instrumental variables	Ages 5-6	Positive effects on maths and reading scores, negative effects on behavioural outcomes. Larger and longer lasting effects for socially disadvantaged children.

Source: Own compilation.

Table A.2: External validity: comparison of socio-economic characteristics across federal states

	Schleswig-Holstein	Bavaria	West	West*
Age	44.07	42.8	43.32	43.33
Female	0.52	0.52	0.52	0.52
Unmarried	0.38	0.40	0.39	0.38
Married	0.47	0.47	0.47	0.48
Widowed	0.08	0.07	0.08	0.08
Divorced	0.07	0.06	0.06	0.06
Household size	2.67	2.77	2.75	2.76
Children in household	0.92	0.99	0.97	0.98
Born in Germany	0.89	0.86	0.85	0.85
Working	0.45	0.49	0.46	0.46
Unemployed	0.03	0.03	0.03	0.03
Out of the labour force	0.51	0.48	0.51	0.51
Highest level of education				
≤ISCED3	0.21	0.22	0.24	0.24
ISCED4	0.03	0.04	0.04	0.04
ISCED5	0.40	0.38	0.36	0.36
ISCED6	0.05	0.03	0.05	0.05
≥ISCED7	0.15	0.18	0.17	0.17
Personal monthly net income				
0 - 1,100	0.56	0.55	0.57	0.57
1,100-2,300	0.32	0.33	0.32	0.32
2,300-3,600	0.07	0.07	0.07	0.07
3,600-5,000	0.03	0.03	0.03	0.03
5,000-18,000	0.02	0.02	0.01	0.01
Household monthly net income				
0 - 1,100	0.13	0.11	0.12	0.12
1,100-2,300	0.36	0.33	0.37	0.37
2,300-3,600	0.23	0.24	0.23	0.23
3,600-5,000	0.17	0.18	0.17	0.17
5,000-18,000	0.11	0.13	0.10	0.10
Municipality size				
<2,000	0.19	0.08	0.04	0.04
2,000-5,000	0.11	0.2	0.06	0.06
5,000-10,000	0.12	0.18	0.10	0.1
10,000-50,000	0.33	0.27	0.37	0.39
50,000-100,000	0.08	0.04	0.11	0.12
100,000+	0.16	0.21	0.32	0.29
N	25,249	112,606	420,623	400,635

Notes: West includes only West-German federal states, less Schleswig-Holstein (SH) and Bavaria (B). West* includes only West-German federal states, less SH, B, Hamburg and Bremen.

Source: Own calculations based on German Mikrozensus 2009.

Table A.3: First stage: effect of being born before December 31st on child care starting age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
5-month window ($N = 1071$)	-0.565***	-0.565***	-0.581***	-0.578***	-0.606***	-0.616***	-0.701***
	(0.095)	(0.095)	(0.095)	(0.095)	(0.090)	(0.090)	(0.142)
First-stage F	35.40	35.65	37.21	37.13	45.41	46.84	24.44
3-month window ($N = 668$)	-0.603***	-0.598***	-0.597***	-0.591***	-0.636***	-0.633***	-0.581***
	(0.126)	(0.124)	(0.126)	(0.124)	(0.115)	(0.115)	(0.218)
First-stage F	22.75	23.10	22.29	22.57	30.79	30.10	7.10
Month of birth controls	Linear	Linear	Linear	Linear	Linear	Linear	Quadratic
interacted with cut-off	yes	yes	yes	yes	yes	yes	yes
interacted with cohort	no	no	yes	yes	no	yes	yes
Control variables	no	yes	no	yes	yes	yes	yes
district FE	no	no	no	no	yes	yes	yes

Notes: Control variables as in Table 1, Panel A. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Source: Own calculations based on NEPS Starting Cohort 4. Observations from Bavaria and Schleswig-Holstein only included.

Table A.4: First stage robustness: effect of being born before December 31st on child care starting age.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
5-months window ($N = 4296$)	-0.332***	-0.333***	-0.331***	-0.332***	-0.345***	-0.345***	-0.409***
	(0.046)	(0.046)	(0.046)	(0.046)	(0.045)	(0.045)	(0.073)
First-stage F	52.29	53.15	52.16	52.93	58.60	58.49	31.71
3-months window ($N = 2569$)	-0.313***	-0.319***	-0.314***	-0.320***	-0.358***	-0.360***	-0.424***
	(0.062)	(0.062)	(0.063)	(0.062)	(0.062)	(0.062)	(0.116)
First-stage F	25.11	26.24	25.19	26.34	33.79	33.97	13.26
Month of birth controls	Linear	Linear	Linear	Linear	Linear	Linear	Quadratic
interacted with cut-off	yes	yes	yes	yes	yes	yes	yes
interacted with cohort	no	no	yes	yes	no	yes	yes
Control variables	no	yes	no	yes	yes	yes	yes
district FE	no	no	no	no	yes	yes	yes

Notes: Control variables as listed in Table 1, Panel A. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Source: Own calculations based on NEPS Starting Cohort 4. Observations with inconsistent information on children's school careers dropped.

Table A.5: Reduced form effect of early child care attendance on test scores.

	Reduced form	Reduced form	Reduced form	2S-2SLS
Language overall (N=5045)	0.017	0.042	0.011	-0.028
	(0.054)	(0.050)	(0.047)	(0.175)
R-squared	0.046	0.187	0.349	
STEM overall (N=5044)	-0.032	-0.020	-0.023	0.055
	(0.056)	(0.052)	(0.049)	(0.180)
R-squared	0.045	0.211	0.363	
Cognition overall (N=4753)	-0.058	-0.046	-0.075	0.183
	(0.056)	(0.055)	(0.053)	(0.219)
R-squared	0.019	0.052	0.189	
Extraversion (N=4825)	0.069	0.068	0.057	-0.134
	(0.059)	(0.058)	(0.060)	(0.198)
R-squared	0.001	0.025	0.070	
Agreeableness (N=4825)	-0.060	-0.053	-0.056	0.131
	(0.057)	(0.057)	(0.058)	(0.188)
R-squared	0.001	0.024	0.074	
Conscientiousness (N=4825)	-0.042	-0.033	-0.033	0.077
	(0.059)	(0.057)	(0.058)	(0.189)
R-squared	0.003	0.054	0.105	
Neuroticism (N=4825)	-0.076	-0.060	-0.039	0.093
	(0.059)	(0.057)	(0.058)	(0.189)
R-squared	0.001	0.062	0.114	
Openness (N=4825)	0.075	0.094	0.097*	-0.229
	(0.059)	(0.057)	(0.059)	(0.205)
R-squared	0.002	0.079	0.124	
SDQ - Pro-Social (N=2405)	0.031	0.027	0.079	-0.213
	(0.080)	(0.078)	(0.083)	(0.444)
R-squared	0.003	0.083	0.190	
SDQ - Peer problems (N=2405)	-0.019	-0.006	-0.060	0.162
	(0.086)	(0.086)	(0.091)	(0.504)
R-squared	0.007	0.026	0.124	
Control variables	no	yes	yes	yes
District FE	no	no	yes	yes

Notes: Specification as in Column 6 of Table 2. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source: Own calculations based on NEPS Starting Cohort 4, parent sample, 5-months window around December.

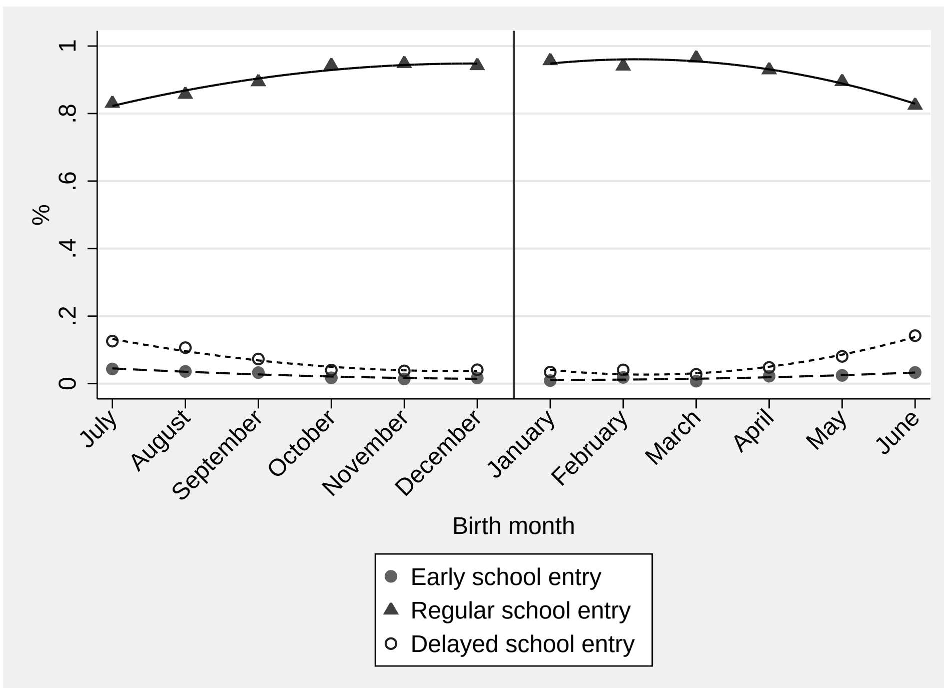
Table A.6: Reduced form effect of early child care attendance on track choice in Grade 5

	All	Native	Non-Native
Panel A: Gymnasium			
All	-0.009	-0.011	0.009
	(0.009)	(0.010)	(0.023)
N	107359	95731	11628
Boys	-0.008	-0.012	0.003
	(0.013)	(0.014)	(0.032)
N	55616	49531	6085
Girls	-0.009	-0.010	0.016
	(0.013)	(0.014)	(0.034)
N	51743	46200	5543
Panel B: Hauptschule			
All	0.000	-0.002	0.027
	(0.010)	(0.010)	(0.029)
N	107359	95731	11628
Boys	0.003	0.006	0.019
	(0.013)	(0.014)	(0.040)
N	55616	49531	6085
Girls	-0.003	-0.012	0.039
	(0.014)	(0.014)	(0.042)
N	51743	46200	5543

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

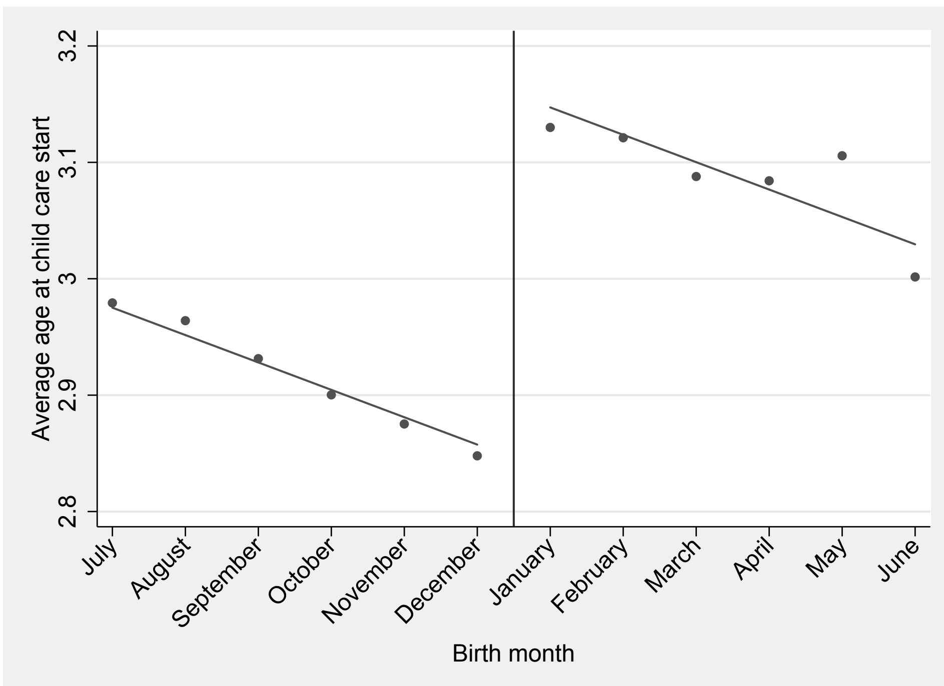
Source: Own calculations based on administrative data from the Bavarian school census. 5 months-window.

Figure A.1: School enrolment patterns, by month of birth.



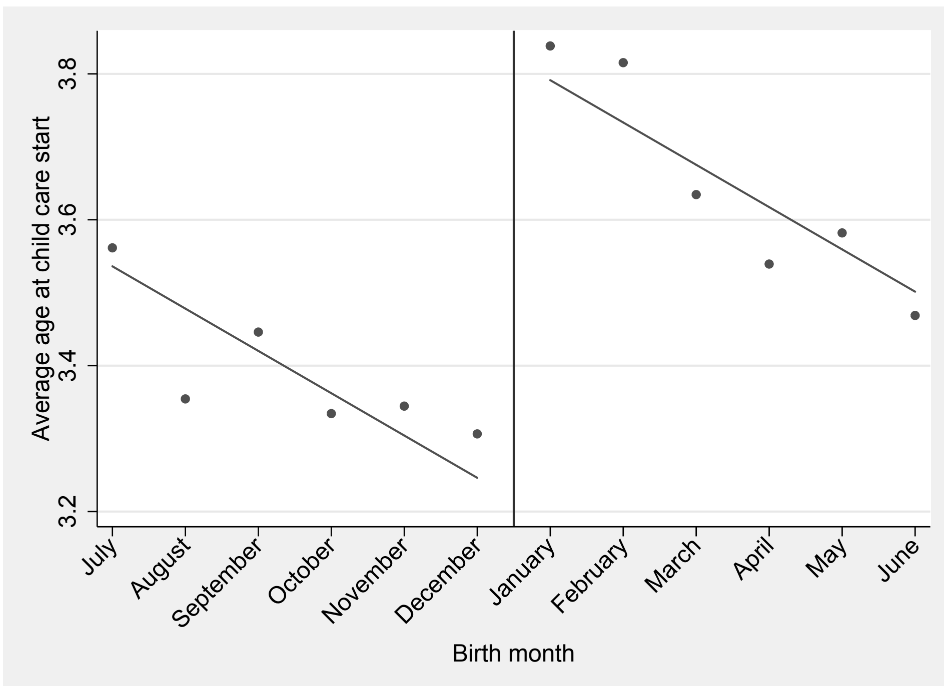
Source: NEPS Starting Cohort 4.

Figure A.2: First stage: average age at child care start, by month of birth.



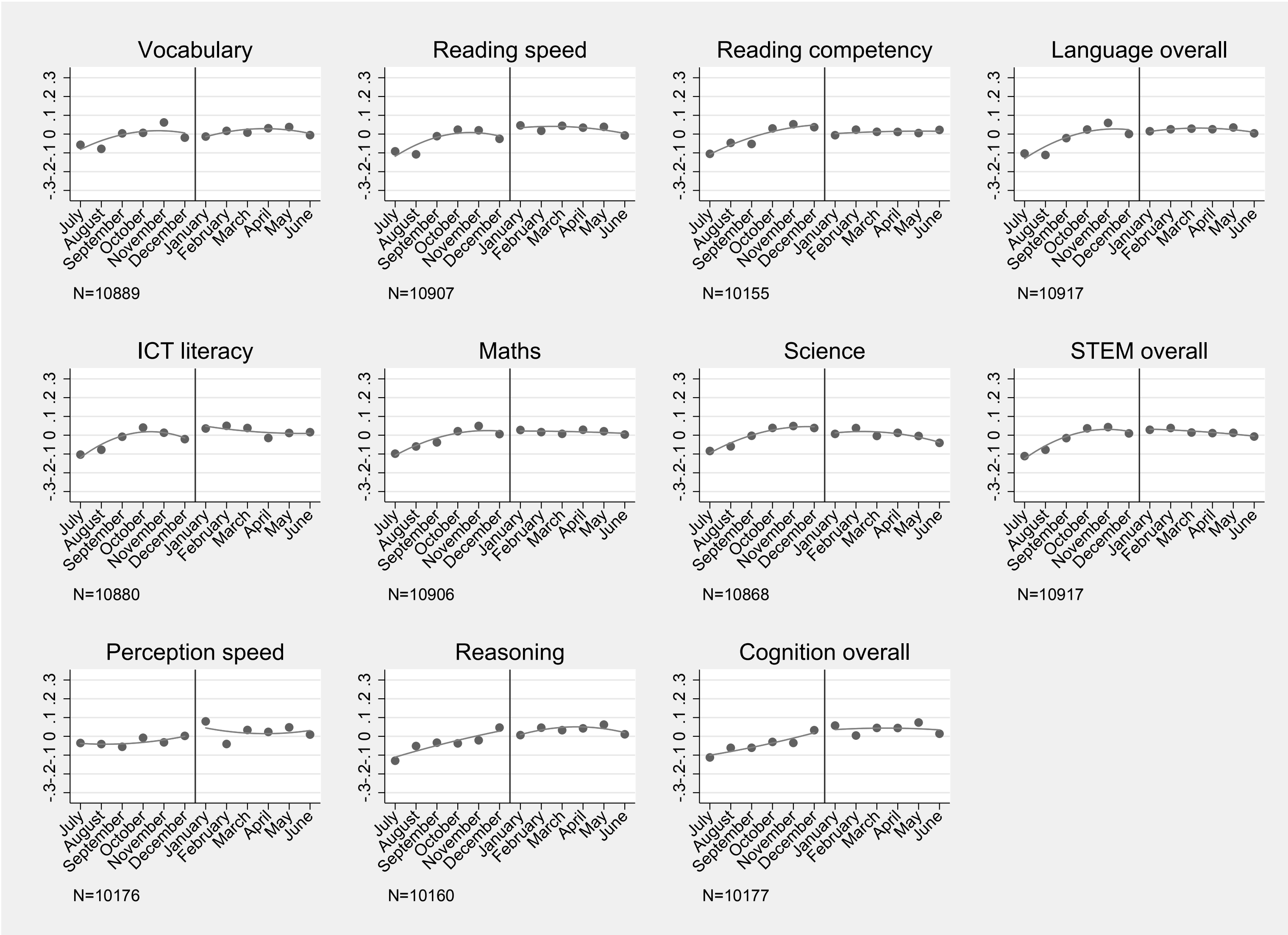
Source: NEPS Starting Cohort 2; covers children born between January 2005 and December 2006.

Figure A.3: First stage: average child care starting age, by month of birth.



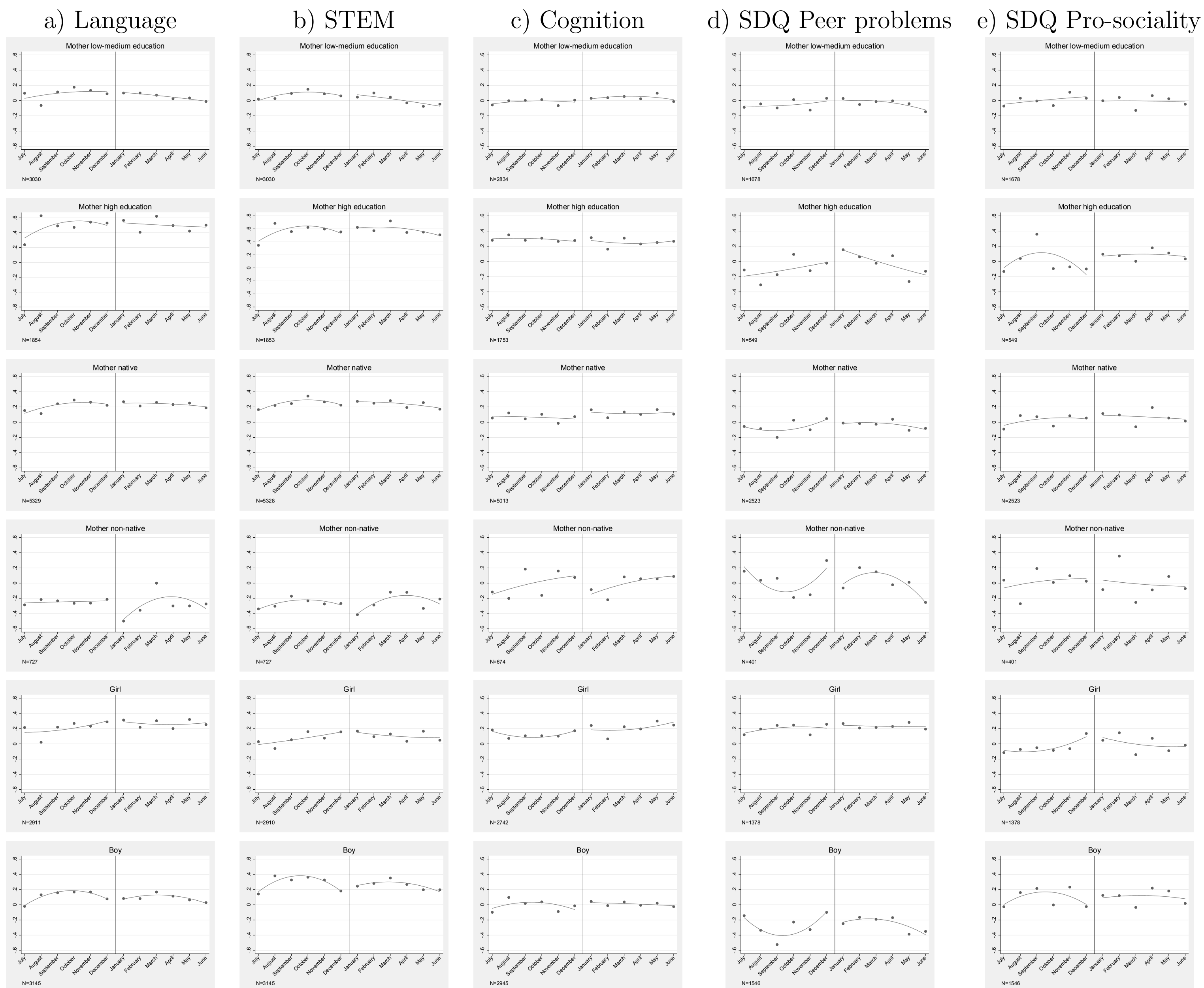
Source: NEPS Starting Cohort 4. Bayern and Schleswig-Holstein.

Figure A.4: Reduced form: individual test scores over birth months.



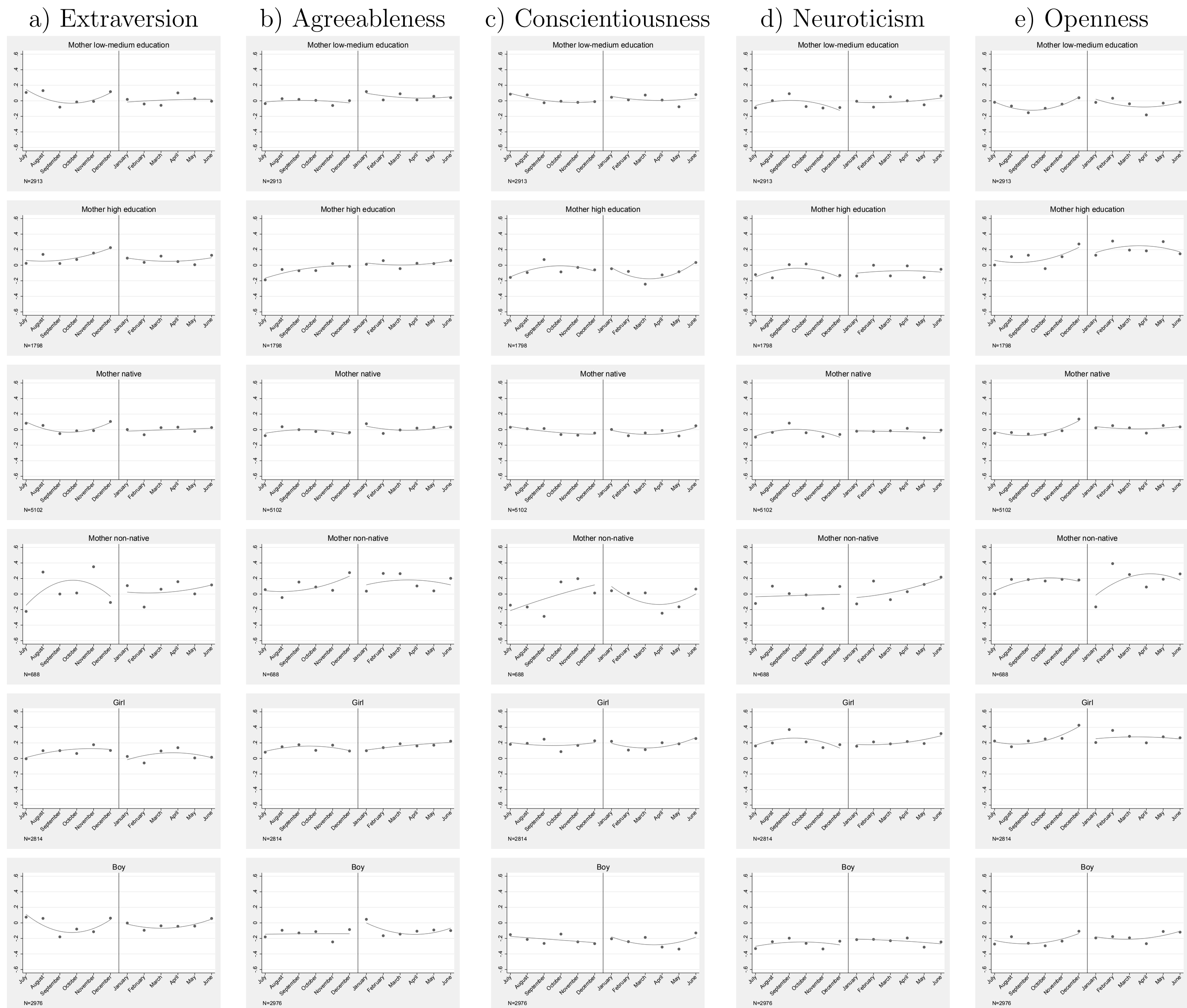
Source: NEPS Starting Cohort 4.

Figure A.5: Reduced form: effect heterogeneity on cognitive skills and SDQ.



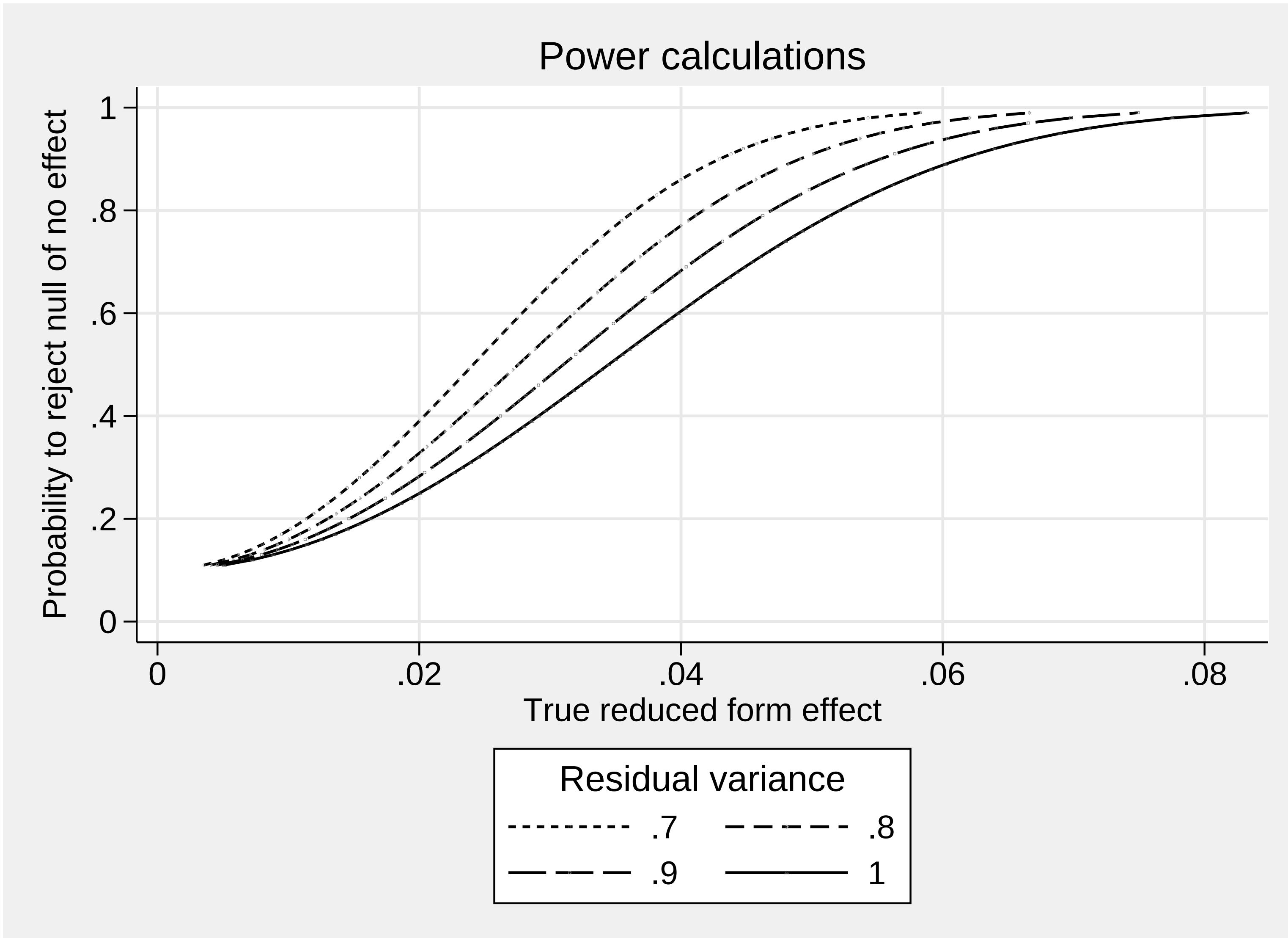
Source: NEPS Starting Cohort 4. Based on parent sample.

Figure A.6: Reduced form: effect heterogeneity on "Big-Five" personality skills.



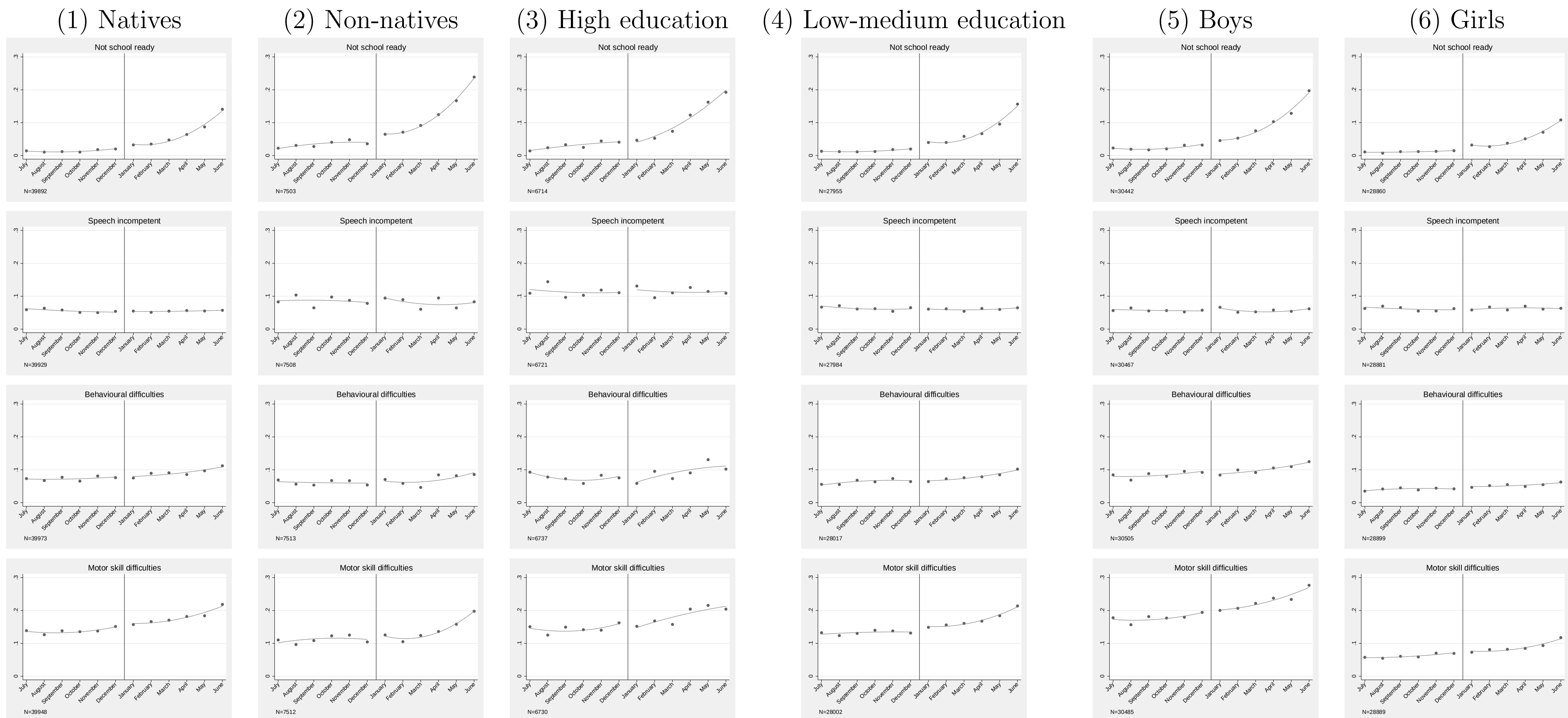
Source: NEPS Starting Cohort 4. Based on parent sample.

Figure A.7: Power calculations



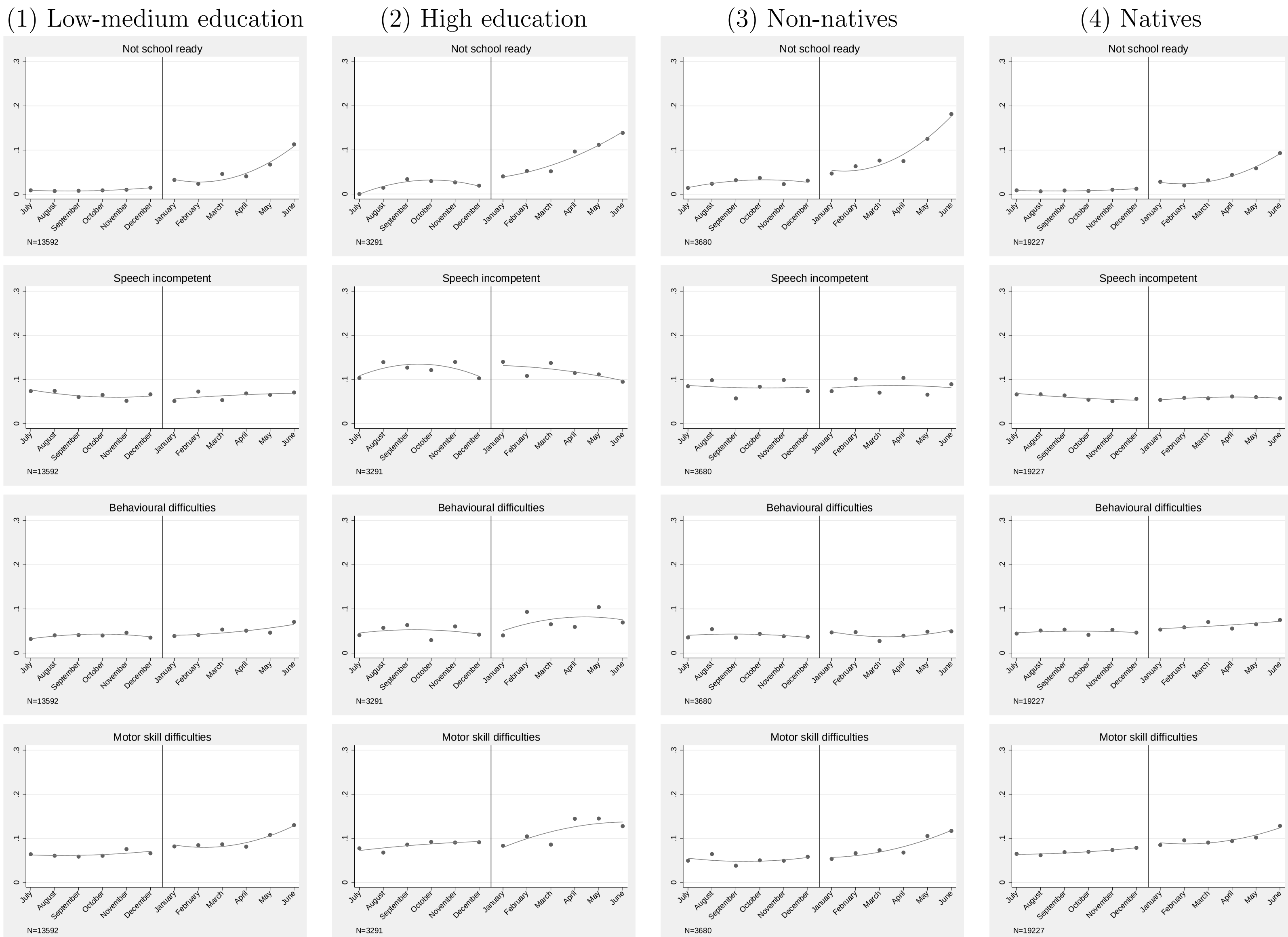
Notes: t-test for equality of group means assuming equal variance for N= 9100, groups comprising 4550 observations each. Testing with type I error probability of 10 per cent. Own calculations using STATA -power- command.

Figure A.8: Reduced form: effect on skill development at age 6.



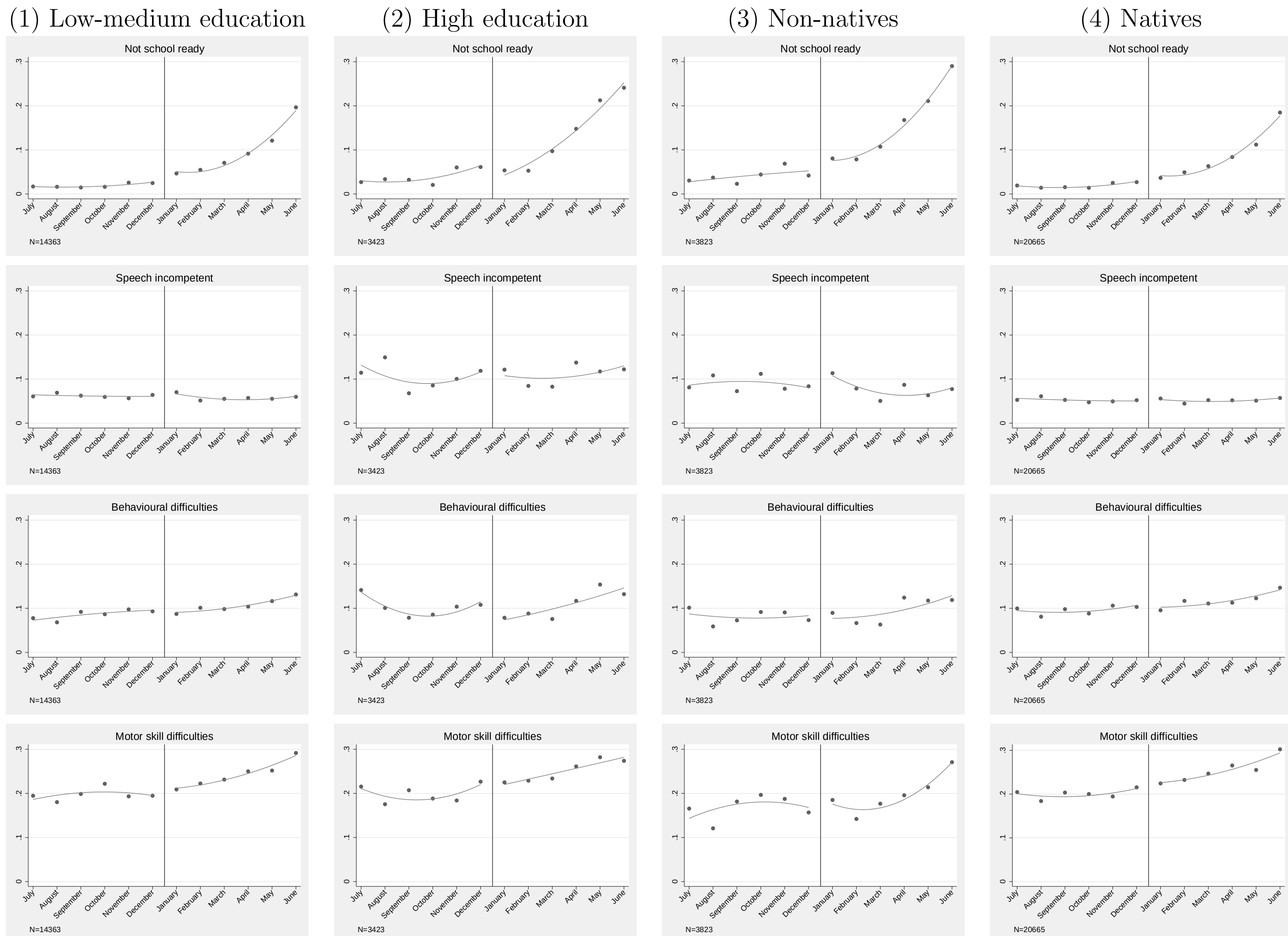
Source: Administrative data from school entrance examinations in Schleswig-Holstein. Birth cohorts 1995m7-1997m6.

Figure A.9: Reduced form SES heterogeneity: effect on skill development at age 6, girls.



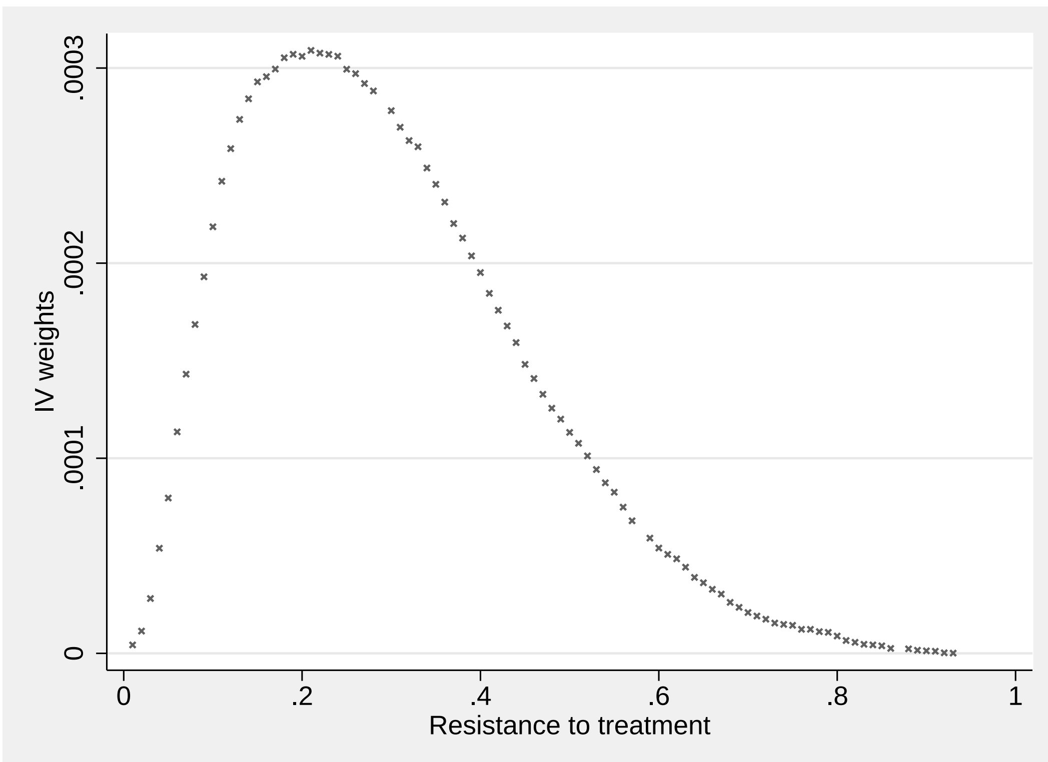
Source: Administrative data from school entrance examinations in Schleswig-Holstein. Birth cohorts 1995m7-1997m6.

Figure A.10: Reduced form SES heterogeneity: effect on skill development at age 6, boys.



Source: Administrative data from school entrance examinations in Schleswig-Holstein. Birth cohorts 1995m7-1997m6.

Figure A.11: IV weights for individuals shifted by the instrument



Source: NEPS Starting Cohort 4. IV weights calculated as in Heckman et al. (2006).

Appendix B: For potential online publication

Table B.1: Descriptives: covariate balancing.

	before=1	before=0	diff. (Δ)	t-statistic	p-value	normalised Δ
Panel A: Based on children's information						
Child male	.51	.49	.02	-1.28	.2	-.03
Child age	15.27	15.06	.21	-18.55	0	-.48
Mother non-native speaker	.19	.18	.01	-1.02	.306	-.03
Mother born abroad	.21	.2	.01	-1.24	.213	-.03
Partner non-native speaker	.15	.13	.02	-1.86	.063	-.05
Partner born abroad	.22	.2	.02	-1.98	.047	-.05
Mother's education						
Missing	.25	.26	0	.33	.739	.01
None/lower secondary	.18	.17	.01	-1	.315	-.03
Medium secondary	.32	.32	0	-.16	.87	0
Upper secondary	.14	.15	-.01	1.11	.266	.03
Any tertiary	.11	.1	0	-.25	.799	-.01
Partner's education						
Missing	.32	.31	.01	-.67	.5	-.02
None/lower secondary	.2	.2	0	.35	.73	.01
Medium secondary	.23	.22	.02	-1.55	.122	-.04
Upper secondary	.12	.14	-.03	3.07	.002	.08
Any tertiary	.13	.12	.01	-.64	.524	-.02
Mother's occupational status						
Missing	.39	.39	.01	-.48	.633	-.01
1st quartile	.12	.11	.01	-1.76	.079	-.05
2nd quartile	.16	.16	0	-.45	.649	-.01
3rd quartile	.15	.15	0	.16	.872	0
4th quartile	.18	.2	-.02	2.32	.021	.06
Father's occupational status						
Missing	.36	.35	.01	-.5	.615	-.01
1st quartile	.15	.15	0	-.32	.751	-.01
2nd quartile	.17	.17	.01	-.79	.43	-.02
3rd quartile	.15	.17	-.01	1.29	.199	.03
4th quartile	.16	.17	0	.48	.629	.01
N	2897	3002	.	.	.	.

continued on the next page.

Table B.1 - continued from previous page.

	before=1	before=0	diff. (Δ)	t-statistic	p-value	normalised Δ
Panel B: Based on parents' information						
Parent's age	44.79	44.78	.01	-.08	.94	0
Mother non-native speaker	.14	.12	.02	-1.54	.123	-.05
Mother born abroad	.15	.12	.03	-2.61	.009	-.09
Living arrangements						
Married/cohabiting	.76	.76	0	-.17	.869	-.01
Divorced/Separated	.18	.18	0	.06	.95	0
Widowed/Single	.06	.06	0	.2	.842	.01
Household size	4.03	4.04	-.01	.17	.862	.01
Mother's education						
Missing	.01	0	.01	-3.08	.002	-.11
None	.02	.02	0	-.94	.345	-.03
Lower secondary	.11	.09	.02	-1.66	.098	-.06
Medium secondary	.47	.49	-.02	1.28	.202	.04
Upper secondary	.14	.13	0	-.3	.766	-.01
Any tertiary	.17	.17	0	.38	.707	.01
Mother's work status						
Employed - Full time	.32	.31	.01	-.55	.58	-.02
Employed - Part time	.51	.53	-.03	1.71	.088	.06
Not employed	.17	.15	.02	-1.62	.105	-.05
Partner non-native speaker	.15	.13	.02	-1.42	.154	-.06
Partner born abroad	.16	.15	.01	-.72	.475	-.03
Partner's age	46.4	46.62	-.22	.96	.337	.04
Partner's education						
Missing	.01	.01	0	-1.06	.29	-.04
None	.01	.01	0	-.91	.365	-.04
Lower secondary	.1	.09	.01	-1.05	.295	-.04
Medium secondary	.45	.45	0	.23	.82	.01
Upper secondary	.18	.18	0	.1	.921	0
Any tertiary	.25	.26	-.01	.81	.42	.03
Partner's work status						
Employed - Full time	.77	.78	-.01	.61	.54	.02
Employed - Part time	.12	.12	0	-.17	.866	-.01
Not employed	.11	.1	.01	-.66	.512	-.03
Family income						
1st quartile	.11	.11	0	.01	.993	0
2nd-3rd quartile	.25	.24	.01	-.53	.593	-.01
4th quartile	.12	.12	0	-.31	.755	-.01
Missing	.52	.53	-.01	.66	.51	.02
N	1561	1631	.	.	.	.

continued on the next page.

Table B.1 - continued from previous page.

	before=1	before=0	diff. (Δ)	t-statistic	p-value	normalised Δ
Panel C: Official child care statistics						
Ratio child care spots per child in 1994	.86	.87	-.01	1.06	.291	.04
1st quintile	.26	.26	0	.01	.994	0
2nd quintile	.23	.22	.01	-.38	.707	-.01
3rd quintile	.15	.14	.02	-1.26	.209	-.04
4th quintile	.16	.16	0	-.13	.894	0
5th quintile	.2	.22	-.02	1.58	.114	.05
Ratio child care spots per child in 1998	1.04	1.04	0	.82	.412	.03
1st quintile	.17	.18	-.01	.86	.39	.03
2nd quintile	.24	.23	.01	-.63	.527	-.02
3rd quintile	.24	.21	.03	-1.92	.055	-.07
4th quintile	.13	.14	-.01	.64	.52	.02
5th quintile	.22	.24	-.02	1.23	.219	.04
Children per child care worker	7.69	7.63	.06	-1.23	.218	-.04
1st quintile	.14	.14	0	-.23	.82	-.01
2nd quintile	.24	.29	-.04	2.94	.003	.1
3rd quintile	.24	.21	.03	-2.08	.038	-.07
4th quintile	.25	.24	0	-.22	.826	-.01
5th quintile	.13	.12	.01	-.74	.456	-.03
N	2897	3002	.	.	.	.

Notes: Descriptive statistics using a 3-months window around December/January threshold. Before=1 if a child is born between August and December, 0 otherwise.

Source: Own calculations based on NEPS Starting Cohort 4 and Official Statistics.

Table B.2: First stage: effect of being born in last quarter on child care starting age

	All	Mother's education		Mother's ethnicity		Child's gender	
		Low-medium	High	Native	Non-native	Boys	Girls
Panel A: Child's information	-0.431***	-0.424***	0.058	-0.436***	0.049	-0.392***	-0.049
	(0.054)	(0.080)	(0.129)	(0.056)	(0.214)	(0.080)	(0.114)
N	3190	1584	947	2827	365	1653	1537
Panel B: Parents' information	-0.422***	-0.378***	-0.051	-0.429***	0.037	-0.382***	-0.051
	(0.054)	(0.072)	(0.114)	(0.056)	(0.216)	(0.080)	(0.114)
N	3190	1860	1330	2835	357	1653	1537

Notes: Control variables as listed in Panel 1 of Table B.1. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Source: Own calculations based on NEPS Starting Cohort 4, full sample, 3-months window around December/January threshold.

Table B.3: Reduced form effect of early child care attendance on test scores.

	Reduced form	Reduced form	Reduced form	2S-2SLS
Language overall (N=5535)	-0.018	0.012	0.004	-0.010
	(0.054)	(0.049)	(0.046)	(0.104)
R-squared	0.055	0.224	0.372	.
STEM overall (N=5535)	-0.043	-0.022	0.003	-0.007
	(0.055)	(0.050)	(0.047)	(0.106)
R-squared	0.058	0.232	0.380	.
Cognition overall (N=5191)	-0.013	0.011	0.021	-0.053
	(0.056)	(0.055)	(0.054)	(0.131)
R-squared	0.021	0.052	0.180	.
Extraversion (N=5243)	0.015	0.029	0.025	-0.055
	(0.057)	(0.057)	(0.058)	(0.125)
R-squared	0.001	0.024	0.068	.
Agreeableness (N=5243)	0.000	0.008	0.030	-0.067
	(0.056)	(0.056)	(0.057)	(0.123)
R-squared	0.000	0.025	0.073	.
Conscientiousness (N=5243)	0.008	0.021	0.013	-0.029
	(0.058)	(0.057)	(0.058)	(0.125)
R-squared	0.001	0.045	0.092	.
Neuroticism (N=5243)	-0.007	0.004	0.009	-0.020
	(0.057)	(0.056)	(0.057)	(0.124)
R-squared	0.002	0.050	0.090	.
Openness (N=5243)	0.067	0.100*	0.112**	-0.248**
	(0.057)	(0.056)	(0.057)	(0.126)
R-squared	0.001	0.061	0.103	.
SDQ - Pro-Social (N=4374)	-0.059	-0.031	-0.027	0.066
	(0.060)	(0.059)	(0.060)	(0.141)
R-squared	0.001	0.057	0.113	.
SDQ - Peer problems (N=4374)	0.028	0.016	0.018	-0.043
	(0.064)	(0.064)	(0.065)	(0.152)
R-squared	0.012	0.028	0.092	.
Control variables	no	yes	yes	yes
District FE	no	no	yes	yes

Notes: Specification as in Column 6 of Table 2. Robust standard errors in parentheses; standard errors on the 2S-2SLS estimates are calculated using the delta method. *** p<0.01, ** p<0.05, * p<0.1
Source: Own calculations based on NEPS Starting Cohort 4, full sample, 3-months window around December/January threshold.

Table B.4: Reduced form effect of early child care attendance on test scores.

	All	Low Edu	High Edu	Native	Non-Native	Boys	Girls
Language overall	0.004	-0.033	0.065	-0.050	0.273*	-0.042	0.036
	(0.046)	(0.062)	(0.101)	(0.049)	(0.143)	(0.065)	(0.068)
N	5535	2805	1445	4531	1004	2753	2782
R-squared	0.372	0.356	0.413	0.339	0.418	0.380	0.425
STEM overall	0.003	0.042	-0.069	-0.031	0.084	-0.052	0.068
	(0.047)	(0.063)	(0.110)	(0.052)	(0.123)	(0.071)	(0.064)
N	5535	2805	1444	4531	1004	2753	2782
Cognition overall	0.021	-0.017	0.065	-0.053	0.398**	-0.036	0.057
	(0.054)	(0.076)	(0.107)	(0.057)	(0.164)	(0.079)	(0.075)
N	5191	2611	1366	4251	940	2585	2606
Extraversion	0.025	-0.102	0.106	0.022	0.053	0.013	0.038
	(0.058)	(0.084)	(0.122)	(0.065)	(0.147)	(0.082)	(0.088)
N	5243	2682	1378	4315	928	2582	2661
Agreeableness	0.030	-0.038	0.035	-0.008	0.180	-0.058	0.117
	(0.057)	(0.080)	(0.126)	(0.062)	(0.167)	(0.085)	(0.080)
N	5243	2682	1378	4315	928	2582	2661
Conscientiousness	0.013	-0.002	-0.003	0.004	0.120	-0.033	0.051
	(0.058)	(0.081)	(0.121)	(0.064)	(0.162)	(0.084)	(0.084)
N	5243	2682	1378	4315	928	2582	2661
Neuroticism	0.009	0.075	-0.075	0.019	-0.021	-0.034	0.001
	(0.057)	(0.082)	(0.127)	(0.063)	(0.160)	(0.083)	(0.084)
N	5243	2682	1378	4315	928	2582	2661
Openness	0.112**	0.038	0.257**	0.081	0.232	0.107	0.140*
	(0.057)	(0.081)	(0.122)	(0.062)	(0.162)	(0.082)	(0.081)
N	5243	2682	1378	4315	928	2582	2661
SDQ - Pro-Social	-0.027	-0.120	-0.068	-0.044	0.024	-0.116	-0.002
	(0.060)	(0.084)	(0.137)	(0.066)	(0.171)	(0.093)	(0.081)
N	4374	2196	1092	3549	825	2115	2259
SDQ - Peer problems	0.018	0.128	-0.080	0.015	0.145	0.082	-0.035
	(0.065)	(0.093)	(0.137)	(0.073)	(0.165)	(0.099)	(0.088)
N	4374	2196	1092	3549	825	2115	2259

Notes: Specification as in Column 6 of Table 2. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source: Own calculations based on NEPS Starting Cohort 4, full sample, 3-months window around December/January threshold.

Table B.5: Reduced form effect of early child care attendance on track choice in Grade 9

	All	Native	Non-Native
Panel A: Gymnasium			
All	-0.001	-0.003	0.013
	(0.008)	(0.008)	(0.018)
N	62040	55157	6883
Boys	-0.003	-0.005	0.024
	(0.010)	(0.011)	(0.024)
N	31693	28146	3547
Girls	0.001	-0.000	0.002
	(0.011)	(0.012)	(0.027)
N	30347	27011	3336
Panel B: Hauptschule			
All	-0.001	-0.000	0.009
	(0.008)	(0.008)	(0.024)
N	62040	55157	6883
Boys	0.009	0.011	0.003
	(0.011)	(0.012)	(0.033)
N	31693	28146	3547
Girls	-0.011	-0.011	0.016
	(0.011)	(0.011)	(0.035)
N	30347	27011	3336

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source: Own calculations based on administrative data from the Bavarian school census. 3 months-window.

Table B.6: Reduced form effect of early child care attendance on school entrance exams.

	All	Low Edu	High Edu	Native	Non-Native	Boys	Girls
Panel A: Birth cohorts 1995m7-1996m6							
Not school ready	0.001	-0.001	0.048**	0.002	0.022	0.012	-0.010
(mean=0.052)	(0.007)	(0.010)	(0.024)	(0.007)	(0.026)	(0.010)	(0.008)
N	13901	6384	1585	9949	1419	7076	6825
Speech incompetent	-0.003*	0.000	-0.002	-0.001	-0.006	-0.002	-0.003*
(mean=0.063)	(0.001)	(0.002)	(0.002)	(0.001)	(0.006)	(0.002)	(0.002)
N	13901	6384	1585	9949	1419	7076	6825
Behavioural difficulties	0.014*	0.013	0.022	0.010	0.013	0.029**	-0.000
(mean=0.068)	(0.009)	(0.013)	(0.026)	(0.011)	(0.026)	(0.014)	(0.011)
N	13901	6384	1585	9949	1419	7076	6825
Motor skill difficulties	0.009	0.003	-0.029	0.003	0.022	0.012	0.008
(mean=0.131)	(0.012)	(0.017)	(0.034)	(0.014)	(0.033)	(0.019)	(0.012)
N	13901	6384	1585	9949	1419	7076	6825
Panel B: Birth cohorts 1995m7-1997m6							
Not school ready	-0.005	-0.010	0.014	-0.004	-0.016	0.002	-0.013**
(mean=0.048)	(0.004)	(0.006)	(0.015)	(0.005)	(0.018)	(0.007)	(0.005)
N	28724	13852	3370	20720	2976	14709	14015
Speech incompetent	-0.002	0.001	-0.007	0.000	-0.008	-0.004*	-0.001
(mean=0.060)	(0.002)	(0.002)	(0.005)	(0.002)	(0.006)	(0.002)	(0.002)
N	28724	13852	3370	20720	2976	14709	14015
Behavioural difficulties	0.007	0.007	0.024	0.009	-0.019	0.014	0.000
(mean=0.071)	(0.006)	(0.009)	(0.018)	(0.008)	(0.018)	(0.010)	(0.007)
N	28724	13852	3370	20720	2976	14709	14015
Motor skill difficulties	0.001	-0.017	0.002	-0.004	-0.011	0.003	0.001
(mean=0.140)	(0.008)	(0.012)	(0.026)	(0.010)	(0.024)	(0.014)	(0.009)
N	28724	13852	3370	20720	2976	14709	14015

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.

Source: Own calculations based on administrative school entrance examinations from Schleswig-Holstein. 3-months window.

Table B.7: Subgroups analysis: reduced form effect of early child care attendance on school entrance exams.

	All	Low Edu	High Edu	Native	Non-Native
Panel A: Boys (Birth cohorts 1995m7-1997m6)					
Not school ready	0.002	-0.010	0.047**	0.002	-0.010
(mean=.062)	(0.007)	(0.009)	(0.024)	(0.007)	(0.028)
N	14709	7050	1711	10643	1520
Speech incompetent	-0.004*	-0.003	-0.008	-0.001	-0.013
(mean=.057)	(0.002)	(0.003)	(0.006)	(0.002)	(0.010)
N	14709	7050	1711	10643	1520
Behavioural problems	0.014	0.009	0.037	0.014	-0.013
(mean=.093)	(0.010)	(0.014)	(0.029)	(0.012)	(0.029)
N	14709	7050	1711	10643	1520
Motor skills	0.003	-0.024	0.005	-0.004	-0.011
(mean=.203)	(0.014)	(0.020)	(0.042)	(0.017)	(0.041)
N	14709	7050	1711	10643	1520
Panel B: Girls (Birth cohorts 1995m7-1997m6)					
Not school ready	-0.013**	-0.009	-0.020	-0.009	-0.019
(mean=.032)	(0.005)	(0.008)	(0.018)	(0.006)	(0.023)
N	14015	6802	1659	10077	1456
Speech incompetent	-0.001	0.005	-0.005	0.002	-0.003
(mean=.063)	(0.002)	(0.003)	(0.008)	(0.002)	(0.007)
N	14015	6802	1659	10077	1456
Behavioural problems	0.000	0.004	0.011	0.003	-0.021
(mean=.047)	(0.007)	(0.010)	(0.021)	(0.009)	(0.022)
N	14015	6802	1659	10077	1456
Motor skills	0.001	-0.010	-0.001	-0.007	0.004
(mean=.074)	(0.009)	(0.013)	(0.029)	(0.011)	(0.024)
N	14015	6802	1659	10077	1456

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.

Source: Own calculations based on administrative school entrance examinations from Schleswig-Holstein. 3-months window.