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## How does education improve cognitive skills? Instructional Time versus Timing of Instruction

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# How does education improve cognitive skills?

## Instructional Time versus Timing of Instruction

February 28, 2015

VERY PRELIMINARY DRAFT – PLEASE DO NOT CITE OR CIRCULATE

### Abstract

This paper investigates two mechanisms through which education may affect cognitive skills in adolescence: the role of instructional quantity and the timing of instruction with respect to age. To identify causal effects, I exploit a school reform carried out at the state level in Germany as a quasi-natural experiment: between 2001 and 2007, academic-track high school (*Gymnasium*) was reduced by one year in most of Germany's federal states, leaving the overall curriculum unchanged. To investigate the impact of this educational change on students' cognitive abilities, I conduct two separate analyses: first, I exploit the variation in the curriculum taught to same-aged students at academic-track high school over time and across states to identify the effect of the increase in instructional time on students' crystallized and fluid intelligence scores. Using rich data on seventeen year-old adolescents from the German Socio-Economic Panel (SOEP) study, the estimates show that fluid intelligence remained unaffected, while crystallized intelligence improved for male students. Second, I compare students' competences in their final year of high school using data from the German National Educational Panel Study (NEPS). Preliminary results suggest that students affected by the reform catch up with their non-affected counterparts in terms of their competences by the time of graduation. However, they do not provide any evidence for the timing of instruction to matter in cognitive skill formation. Overall, secondary education therefore seems to impact students' cognitive skills in adolescence especially through instructional time and not so much through age-distinct timing of instruction.

**Keywords:** Cognitive Skills, Crystallized Intelligence, Fluid Intelligence, Skill Formation, Education, High School Reform

**JEL Classification:** I21, I28, J24

# 1 Introduction

Cognitive skills are important determinants of many economic and social outcomes: Higher cognitive skills have been associated for example with increased health and better old-age functioning mental abilities. Increased cognitive abilities are also linked to higher wages (see e.g. Heckman et al., 2006, or Heineck and Anger, 2010) and better education. The latter association is however a two-way relationship. On the one hand, individuals with higher cognitive abilities are likely to be better educated as they more often choose to or better meet the requirements to continue education, e.g. tertiary education at universities. On the other hand, education itself also improves cognitive skills. Most studies use changes in compulsory schooling laws as an exogenous variation to identify causal positive effects of an additional year of schooling (e.g. Banks and Mazzonna, 2012). However, there is no evidence on the underlying mechanisms. This paper therefore investigates the roles of instructional time and timing of instruction as two potentially important channels through which secondary education may affect cognitive skills.

My research question is therefore two-fold: First, I assess the impact of an increase in instructional time on cognitive skills of adolescents in Germany. Second, I investigate whether the timing of instruction influences cognitive skill development, i.e. whether the allocation of class hours at a younger age changes cognitive skills after obtaining the same level of education. In both analyses, I allow for gender heterogeneity to further investigate whether such educational changes are mitigating or aggravating factors for gender skill differences.

To address these questions, I exploit a recent reform in German high schools that shortened total years of schooling from thirteen to twelve, leaving the overall curriculum unchanged. As a result, the number of weekly class hours significantly increased. Hence, while still in school, affected students have covered a greater share of the overall curriculum than non-affected students of the same age. I use this intensified curriculum as an exogenous increase in the instructional quantity received up to the age of seventeen and exploit the variation over time and region in the implementation of the reform to identify its causal effect on adolescents' cognitive skills. Using rich data on adolescents from

the German Socio-Economic Panel (SOEP) study, I find that crystallized intelligence improved, while fluid intelligence remained largely unaffected. However, the former impact significantly differs by gender: whereas male students' scores improved especially in mathematical skills, female students' skills hardly improved at all. I further use the variation in the age at which students received instruction as a quasi natural-experiment to investigate the impact of educational timing on students' competences. Using extensive data from the German National Educational Panel Study (NEPS) for the federal state of Baden-Wuerttemberg on students in their final grade, preliminary estimations suggest that the earlier knowledge transfer did not significantly affect the development of competences among students affected by the reform. Here, the potential benefit of early investment and age effects seem to offset each other. As a result, students affected by the reform catch up with their non-affected counterparts in terms of their competences by the time of graduation, apart from potential age effects resulting in slightly decreased fluid intelligence scores.

The next section gives an overview of the theoretical background and the existing literature. Section 3 explains the high school reform in more detail and elaborates on potential mechanisms and anticipated effects. Section 4 describes the data. The empirical strategy is elaborated on in Section 5 and Section 6 presents the results. Section 7 deals with robustness checks to confirm the findings, before Section 8 concludes and discusses the implications.

## **2 Theoretical Background and Previous Literature**

Cognitive skills shape a variety of later-life outcomes. Together with non-cognitive skills they form an important part of an individual's human capital as they constitute personal skills. A common approach to describe the formation and development of such skills has been proposed by Cunha and Heckman (2007). They argue that an individual's present stock of skills depends on his or her past stock of skills, previous investment and environmental factors. More specifically, they suggest the following model:

$$\theta_{t+1} = f_t(\theta_t, I_t, h_t) \quad (1)$$

where a vector of skill stocks at age  $t + 1$  depends in some positive functional form  $f(\cdot)$  on the past vector of skills (with initial endowment  $\theta_1$ ), on the investment in period  $t$ ,  $I_t$ , and on parental, or more generally environmental, characteristics  $h$ . In this model, Cunha and Heckman propose a multiplier effect driven by two mechanisms, *self-productivity* and *dynamic complementarity*. Self-productivity occurs whenever  $\partial f_t(\theta_t, I_t, h)/\partial \theta_t > 0$ . This implies that skills persist such that higher skills at one point in time create higher skills in the subsequent period, and is not restricted to one and the same skill but also includes cross effects between different skills. Dynamic complementarity occurs whenever  $\partial^2 f_t(\theta_t, I_t, h)/\partial \theta_t \partial I_t' > 0$  implying that the productivity of investment is increasing with higher existing skills. Cunha and Heckman (2008) test and verify both propositions empirically. Hence, the resulting multiplier effect suggests that investments are most productive in early stages in life, making childhood the critical period for skill formation. Skills may therefore be malleable through e.g. educational interventions, especially at an early stage in life. However, there are important differences across dimensions of skills to distinguish.

Cognitive skills are usually distinguished into different aspects. Two major ones in the empirical literature are fluid intelligence and crystallized intelligence. Fluid intelligence relates to innate abilities which people are genetically endowed with. These include for example the ability to reason or the level of comprehension or processing information, and are usually not influenced by environmental factors. Crystallized intelligence, in contrast, denotes explicitly or implicitly learned knowledge or behavior. It therefore covers any specific knowledge of certain facts for example, as well as certain behavioral traits, the ability to read or calculate, or the like. Unlike fluid intelligence, crystallized intelligence is therefore determined through environmental factors like education or breeding throughout life. Several studies have therefore shown that education indeed improves the crystallized component of cognitive skills.

For Scandinavian countries, several studies have used data on males between the ages

18 and 20 from military cognitive assessment tests: Brinch and Galloway (2012) use an increase in compulsory schooling from seven to nine years in Norway between 1955 and 1972. Their Difference-in-Differences results suggest an increase in cognitive ability of 0.6 of a standard deviation due to the reform, while their Instrumental Variable results translate an additional year of schooling into an increase of 3.7 IQ points. For Sweden, Carlsson et al. (2012) exploit a random variation in test dates to find one additional year of schooling leading to a 0.21 standard deviation increase in crystallized intelligence. Fluid intelligence seems not to be affected by schooling, but positively by age. Instrumenting schooling and initial IQ, Falch and Massih (2011) find cognitive returns of one additional year of schooling between 2.9 and 3.8 IQ points for the Swedish population in Malmö enrolled in the military in 1947 and 1948. Cascio and Lewis (2006) use data from the 1979 National Longitudinal Survey of Youth (NLSY79) to estimate returns to schooling on the Armed Forces Qualifying Test (AFQT) scores of males and females aged 15 to 19 years in the United States. Exploiting variation in the date of birth and school entry regulations, they find positive effects of 0.32 standard deviations; however only for racial and ethnic minorities. Setting up a regression discontinuity design to analyze the effects of a compulsory schooling reform in England, Banks and Mazzonna (2012) find an increase in memory functioning of between 0.35 and 0.6 standard deviations among males and females above age 50. Executive functioning in turn increased for males only, with effect sizes ranging from 0.37 to 0.63 of a standard deviation. Using SHARE data on Austria, Czech Republic, Denmark, France, Germany, Italy, Schneeweis et al. (2012) exploit the variation in compulsory schooling across the different countries to investigate cognitive ability of above-fifty-year-olds. They find positive effects of 0.15 to 0.19 standard deviations on memory functioning, but no effects on word fluency and numeracy, and a decrease in risk of dementia only across women. Lastly, Kamhöfer and Schmitz (2013) investigate the impact of education in Germany on word fluency among males and females born between 1940 to 1970 using data from the German Socio-Economic Panel (SOEP) study in 2006. They use different instruments for schooling to estimate local average treatment effects but find no effects. They argue that the institutional setting

may be different in Germany than in other countries and may therefore be responsible for this surprising result. However, their outcome is limited in the sense that it is a single-edged view on cognition as it does not cover further dimensions of cognition next to word fluency, and that it is based on an ultra-short test which is conducted in only 90 seconds. Furthermore, weak instruments may be threat to their identification and age and cohort specific effects cannot be disentangled, which may bias their results.

Hence, with the exception of the study on Germany, all studies find clearly positive effects of an additional year of schooling on cognitive abilities. Most of these studies use a change in overall school duration by one year to identify positive effects on cognition. However, the underlying mechanisms remain unresolved. For policy conclusions it is however indispensable to understand whether there are further driving forces than simply school duration behind this relationship. School duration per se cannot be changed infinitely much; the existence of underlying channels would however open up new possibilities for decision makers to target cognitive ability when designing educational policies.

A change in school duration may have different consequences related to skill formation. On the one hand, an additional year of schooling may induce a larger curriculum to cover i.e. constitute a direct increase in time and material of instruction. On the other hand, a change in the overall years of schooling may as well only lead to a redistribution of covered material and instruction over the different grades, i.e. over different age spans of the students. While the former constitutes a direct increase in investment  $I_t$  in the skill formation model by Cunha and Heckman (2007), the latter implies a shift in the timing of investment  $I_t$ . Both may therefore impact cognitive skills: on the one hand, keeping age and past skills constant, an increase in investment, i.e. an increase in instruction quantity, may directly improve cognitive abilities. On the other hand, keeping overall instruction quantity constant, the timing of to which age instruction is allocated may influence cognitive abilities as well. Here controversial mechanisms could interact, where earlier instruction is assumed to improve cognitive skills through the multiplier effect proposed by Cunha and Heckman (2007) and because skills are more malleable at younger ages, but later instruction could benefit from maturity or time required to digest instruction.

It therefore still remains to investigate whether either instructional time or timing of instruction drive the positive relationship between schooling and cognitive skills or whether it is an interaction of the two mechanisms. To the best of my knowledge, this study is the first to investigate and disentangle these two mechanisms. To identify causal effects, I use a unique variation in the German schooling system which allows me to conduct two separate analyses to provide a complete picture. First, keeping age constant, the causal effect of an increase in instructional time is identified. Second, keeping the educational level constant, the role of instructional timing and age is analyzed. Furthermore, the rich datasets contain extensive tests of cognitive ability allowing to distinguish between different cognitive dimensions. Next to that, the inclusion of female respondents enables the investigation of gender heterogeneity to uncover whether education may be a mitigating or aggravating factor in gender skill differences. Lastly, this study extends the literature on Germany, especially given the puzzle that Kamhöfer and Schmitz (2013) find no effects while for all other countries investigated there exist positive cognitive returns to education.

## **3 The German High School Reform**

### **3.1 Institutional Background and Change**

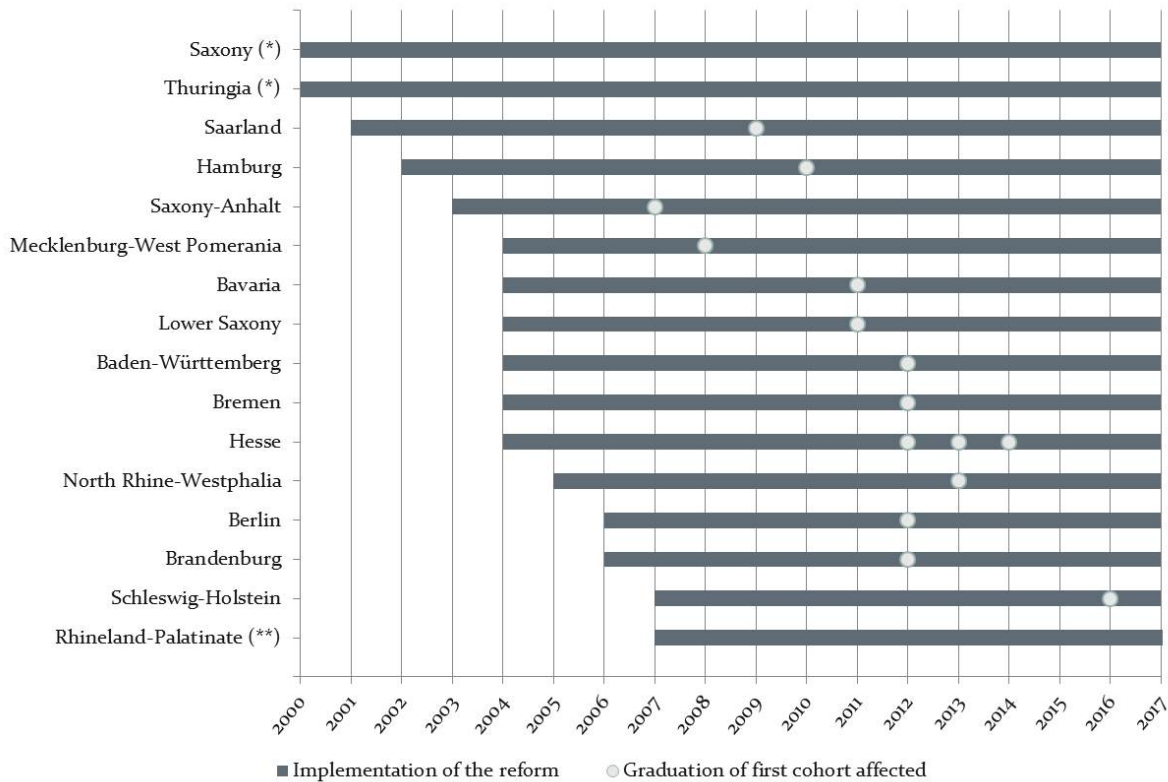
In Germany, educational policy is at the responsibility of the federal states. In all cases however, children enter elementary school at the age of six and follow on to secondary education usually after four years.<sup>1</sup> Secondary education in Germany is provided at three different levels, listed in ascending order by their level of education provided: Hauptschule (basic track), Realschule (intermediate track) and Gymnasium (upper track). Of these three, only successful completion of Gymnasium (henceforth referred to as academic-track or simply high school) leads to the Abitur (henceforth referred to as high school diploma), the university entrance qualification.

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<sup>1</sup>Exceptions hereto are Berlin, Brandenburg and Mecklenburg-West Pomerania, where primary school encompasses the first *six* grades. The assignment to different types of secondary school therefore takes place at grade seven.



Figure 1: Introduction of the Reform by Federal State



(\*) Saxony and Thuringia already had established a 12-year school system since the 1990ies.

(\*\*) In Rhineland-Palatinate the reform has only been introduced in selected schools so far.

Source: Autorengruppe Bildungsberichterstattung (2010)

Traditionally, time at high school lasted nine years, implying a total of thirteen years of schooling. Starting in 2001, several German states reduced this time at high school by one year, enabling graduation after a total of only twelve years of schooling.<sup>2</sup> However, the overall curriculum remained unchanged.<sup>3</sup> As a result, weekly class hours significantly increased<sup>4</sup> and school days prolonged. Although the reform was implemented almost in the entire country, its time of introduction differs by state. An overview of the implementation of the reform by federal state is depicted in Figure 1.

<sup>2</sup>An extensive discussion on the reasons for this reform can be found in Dahmann and Anger (2014).

<sup>3</sup>From grade five to the final grade 265 year-week hours have to be fulfilled. This restriction was kept even when shortening high school duration from nine to eight years. In the states where primary school encompasses the first six grades, the reform reduced time at high school from seven to six years. The year-week hours requirement holds in the same way, however, as for other states, counting class hours from grade five onwards.

<sup>4</sup>With nine years at high school, the average week hours amounted to  $265/9=29.44$  hours; with only eight years at high school they increased to  $265/8=33.13$  hours.

## 3.2 Anticipated Effects of the Reform on Cognitive Skills

The reform may have affected students' cognitive skills through several channels. I aim to disentangle two important mechanisms: the effect of an increase in instruction time (keeping age constant) and the role of earlier instruction (keeping reached educational level constant). According to the skill formation model by Cunha and Heckman (2007), both cases should be assumed to lead to higher cognitive skills. Still, it is an empirical question whether and to which extent these mechanisms can be verified to lead to higher cognition, especially during adolescence. Even if they were to hold, further aspects may hinder or even offset their positive effects. I therefore conduct two separate analyses; the results of which will shed light on the mechanisms behind the relation between schooling and cognitive abilities.

The first analysis compares same-aged, i.e. seventeen-year-old students, where the students affected by the reform have accumulated significantly more class hours. This increase in instructional quantity should raise especially crystallized measures of intelligence, while fluid intelligence is generally assumed not to be affected.<sup>5</sup> Nonetheless, it may also be that either the additional knowledge taught cannot be absorbed anymore by the students<sup>6</sup> or simply that cognitive skills are not malleable anymore at this age in adolescence, bringing no particular change in cognition at all. Lastly, the reform may come at the cost of further, e.g. non-cognitive, skills or extracurricular activities important for skill development, offsetting the positive effects on cognition or even negatively impacting them.<sup>7</sup>

The second analysis compares students in their final year of high school, of different ages though. At this point in time, both, students affected by the reform and students not affected, have reached the same educational level and accumulated the same number of class hours. However students affected by the reform have received this instruction at a

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<sup>5</sup>Note that it is not possible to completely separate these two dimensions of intelligence in a test environment. As soon as e.g. speed is introduced to give specific knowledge, fluid and crystallized skills are required simultaneously.

<sup>6</sup>Whether this is the case may especially differ between distinct types of students, as e.g. students with lower initial skills may have more difficulties with keeping up at the new pace.

<sup>7</sup>Dahmann and Anger (2014) show that the reform indeed had an effect on some personality traits. The participation in extracurricular activities seems however not to be affected.

Table 1: Anticipated Effects of Increased Instruction Quantity on Cognitive Skills of Same-aged Students

Cognitive Skills	Effect	Potential Mechanisms
Crystallized	+	Increase in taught curriculum until age 17 Homework more efficient/productive due to supervision at school within long school days Earlier course choice (based on strengths/preferences)
	0	Cognitive skills are not malleable anymore in adolescence Additional knowledge taught cannot be absorbed anymore
	-	Long school days come at cost of extra-curricular activities which may be important for skill development (direct effects, indirect effects through changes in non-cognitive skills)
Fluid	0	Not malleable
	+/-	Cross effects of changes in crystallized intelligence

relatively younger age. According to Cunha and Heckman (2007) this earlier investment – presumably leading to higher cognitive skills at an earlier stage in life, which will be tested in the first analysis – increases a person’s stock of skills at an earlier stage making any investment thereafter even more productive. As a result, students affected by the reform may have acquired higher cognitive skills through this multiplier effect of early investment, at least in crystallized dimensions of cognition. However, these students are one year younger in turn when attending the final grade as are students not affected by the reform, which may have negative consequences for cognition, including both crystallized and fluid dimensions.

Table 2: Anticipated Effects of Earlier Instruction on Cognitive Skills of Students in Final Grade

Cognitive Skills	Effect	Potential Mechanisms
Crystallized	+	Multiplier effect making instruction more productive (if indeed the increase in instruction time increased cognitive skills at a younger age)
	-	Age effects
Fluid	0	Not malleable
	-	Age effects
	+/-	Cross effects of changes in crystallized intelligence

## 4 Data

To investigate both potential mechanisms, I conduct two analyses. The first compares same-aged students to evaluate the impact of an increase in instructional time on cognitive skills: Students affected by the reform have accumulated a larger number of class hours than students prior to the reform. The second investigates a sample of students at the same educational level, i.e. at completion of secondary school, to identify the role of earlier knowledge transfer implied by the reform for affected students. By nature, these two samples differ and pose different requirements to the underlying dataset to enable identification. Therefore the two analyses will be based on different datasets.

### 4.1 The German Socio-Economic Panel (SOEP) study

The first analysis is based on data from the German Socio-Economic Panel (SOEP) study, which is a representative household panel surveyed annually (Wagner et al., 2007) with around 25,000 individuals in almost 15,000 households in the most recent wave. In addition to various individual and household characteristics, including family background and childhood environment, the SOEP includes measures of cognitive potential for its different subsamples since 2006. The cognitive abilities of adolescents aged seventeen, who are respondents of the SOEP so-called youth questionnaire, are assessed in every wave since 2006. Hence, I use waves 2006 to 2013 and include all adolescent respondents aged seventeen<sup>8</sup> who attend *Gymnasium* in my sample. To identify whether a student is affected by the reform, I use the information on the federal state of residence and the year of school entry. In case information on the latter is not provided, the year of school entry is imputed from the date of birth. As in Saxony and Thuringia a twelve-year-school system was already established since before Germany's reunification, I consider all students in these two states as affected.<sup>9</sup> I exclude students from Rhineland-Palatinate where the

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<sup>8</sup>In 2006, when the test of cognitive abilities was conducted for the first time, also adolescent respondents from the waves 2004 and 2005 were tested. To increase the sample size, I will include these individuals (aged eighteen and nineteen) as well in my preferred specification. Birth year dummies control for potential age effects. Still, a robustness check including only seventeen year-olds is conducted to confirm the results.

<sup>9</sup>In a robustness check students from these two states are excluded.

reform has not been implemented state-wide so far. To not add noise to the amount and level of education received so far, I exclude all students for whom I know that they have repeated one grade.<sup>10</sup> Lastly, I restrict the sample to those who have successfully completed the cognitive assessment test and with valid information on background and family characteristics. The final sample consists of 707 students, of whom 285 are affected by the reform.

In the SOEP adolescent questionnaire, cognitive skills are measured through a 30 minutes test which is a short form of the I-S-T 2000 R (see Amthauer et al., 2001). This test consists of three parts with 20 questions each.<sup>11</sup> The first part consists of word analogies and measures verbal skills: participants are asked to find a matching word according to a specific rule. In the second part numerical skills are measured, where the respondent has to fill in the correct arithmetic operators in incomplete equations. Together, these two (verbal and numerical) tasks resemble crystallized intelligence as they reflect an individual's explicitly learned competences. In contrast, the third task serves to measure fluid intelligence: here three abstract figures are displayed according to a specific rule. Participants have to match a fourth one following this rule by choosing out of five proposed figures. On each of these three test components adolescents answer as many questions as possible, in the given amount of time. The scores then measure the number of correct answers (out of 20 possible questions). To facilitate the interpretation of results, I standardize all scores separately by gender to mean zero and variance one.

To account for individual characteristics which may influence cognitive abilities as well, I control for several pre-reform characteristics in my preferred specification. These include socio-economic and demographic variables like gender, migration background and

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<sup>10</sup>This procedure would threaten my identification if repetition rates changed with the reform. This would be the case if repeating a class results from the increased learning intensity induced by the reform such that after the implementation of the reform weaker students are more likely to repeat a grade and therefore drop from my sample. However, Huebener and Marcus (2015) find that repetition rates up to grade ten remained unchanged by this reform. Even though they changed in the final years before graduation, for my sample of seventeen-year olds repetition rates should therefore be similar before and after the reform. In my sample only 54 students drop because of grade repetition, of which 29 are affected by the reform and 25 are not. Due to this low number of occurrence, I refrain from statistical comparison of repetition rates between the two groups and assume this unlikely to bias the estimation results.

<sup>11</sup>For an extensive overview of the measurement and assessment of adolescents' cognitive potential in the SOEP see Schupp and Herrmann (2009).

term of birth<sup>12</sup>. Furthermore I capture a student's previous performance by the teacher's recommendation after primary school. Family variables include parental characteristics based on education, work status and occupational status, and further capture whether a student grew up with only one parent.

## 4.2 The German National Educational Panel Study (NEPS)

The second analysis is based on data from the German National Educational Panel Study (NEPS), which is a longitudinal dataset that aims at mapping competence development and learning environment over the life cycle. It follows a multicohort sequence design starting with a total number of more than 60,000 target persons from six cohorts (Blossfeld et al., 2011). Next to these original six cohorts, it includes a cross-sectional additional study in the German federal state of Baden-Wuerttemberg, which targeted all students at academic-track high school in their final year.<sup>13</sup> In the year 2012, this target population therefore consisted of the last cohort which was not affected by the high school reform yet and the first cohort which was affected. Hence, I use wave 2012 and include all respondents in my sample who attend the final grade of *Gymnasium* in my sample. Again, I exclude all students who have repeated at least one grade. The final sample consists of 2,128 students, of whom 1,113 are affected by the reform.

Cognitive abilities are measured through an extensive 2 hours 40 minutes test covering different educational dimensions. Reflecting explicitly learned knowledge, achievement tests in Physics, Biology and English<sup>14</sup> constitute different measures of crystallized intelligence. On each of these achievement tests, students are given a set of questions

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<sup>12</sup>Students born in the second half of the year, are usually only sixteen years old at the point in time of the interview, as interviews take place primarily in the first quarter of the year. Therefore they mostly attend grade ten at this age. In contrast, students born in the first half of the year usually enter school comparatively young and are therefore, on average, one grade more advanced at the point in time of the interview.

<sup>13</sup>This paper uses data from the National Educational Panel Study (NEPS): Additional Study Baden-Wuerttemberg, doi:10.5157/NEPS:BW:2.0.0. 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.

<sup>14</sup>Furthermore, mathematical knowledge is assessed in this NEPS study. However, for the sample under investigation, the scores are not released yet. As soon as they will be, mathematics will be included as a further dimension in this analysis.

with a maximum of 21 to 36 questions. Most of these questions have multiple choice options as answers; others partly have to be answered in an open format. For the time being, though not ideal, I use the percentage of all correct answers on each crystallized dimension as the relevant score.<sup>15</sup> Fluid intelligence is covered by measures of general cognitive abilities, i.e. perceptual speed and reasoning. Perceptual speed is assessed by a picture symbol test where respondents are required to enter correct figures for the preset symbols according to an answer key (see Lang et al., 2007), with a total of  $3 \times 31$  items to be solved in  $3 \times 30$  seconds. Reasoning is measured in the same way as figural skills in the SOEP adolescent questionnaire: Based on Raven’s matrices, students have to fill in a missing geometrical element that fits the other elements of the matrix, in a total of  $3 \times 4$  cases with  $3 \times 3$  minutes time. For both of these fluid cognitive skills measures, the total score is calculated as the sum of correctly solved items. Again, for both crystallized and fluid measures of cognitive ability, I standardize all scores separately by gender to have mean zero and variance one.

Next to achievement tests, the survey includes further individual and school characteristics. Whether or not a student is affected by the reform is given in the survey. Individual characteristics include demographics as gender and migration background. Furthermore, the number of books at home, parental education, the father’s work classification and the mother’s occupational status characterize a student’s socio-economic origin and home environment.

## 5 Empirical Strategy

I exploit the German high school reform introduced in almost all federal states between 2001 and 2007 as a quasi-natural experiment to identify a causal effect of education on cognition. The control group consists of students who entered high school before the reform was introduced and therefore graduate within nine years of high school. In contrast,

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<sup>15</sup>This score based on the simple sum of correct answers is not an ideal measure due to the multi-matrix design in the test structure. Hence, an Item Response Theory approach should actually be used instead. This will be incorporated as soon as possible; until then this draft version only includes simple sum scores as cognitive measures. Results are therefore only very preliminary and should be interpreted with caution.

the treatment group consists of students entering high school after the implementation of the reform and thus graduating within only eight years.

## 5.1 Difference-in-Differences (DID) Estimation using SOEP

For the first analysis, all students in the selected SOEP sample usually attend either grade ten or grade eleven at the time of the interview, hence have spent either more than 5<sup>1/2</sup> or 6<sup>1/2</sup> years in high school so far.<sup>16</sup> However, the amount of education received during this time differs between the control group and the treatment group, as the reform provides an exogenous variation in the number of class hours received until this age. Students affected by the reform should have accumulated at least between 800 and 945 class hours of education more, on average, than their non-affected counterparts at the same stage of secondary education.<sup>17</sup>

To assess the impact of this intensified curriculum on cognitive skills, I estimate the following equation:

$$y_{ist,17} = \alpha \text{REFORM}_{st} + X_i \beta + \sum_s \gamma_s \text{STATE}_s + \sum_t \delta_t \text{YEAR}_t + \varepsilon_{ist}, \quad (2)$$

where  $y_{ist,17}$  is a measure of cognitive ability at age 17 of student  $i$  living in state  $s$  and born in year  $t$ . The variable of interest,  $\text{REFORM}_{st}$  indicates whether students belong to the treatment or the control group. It equals 1 if students entering school in state  $s$  in year  $t+6$  (or  $t+7$  respectively, depending on their month of birth)<sup>18</sup> are affected by the reform once following onto high school (i.e. belong to the treatment group) and 0 otherwise (i.e.

<sup>16</sup>In the SOEP, adolescents are interviewed in the year they *turn* seventeen. Thus, the age of seventeen results from defining age simply as the difference between the year of survey and the year of birth. Note however, that their *real* age at the time of interview therefore is either sixteen or seventeen, depending on their date of birth and the date of the interview. As the interviews take place almost exclusively in the first quarter of the year, students of this age usually attend either grade ten (in case they were born between July and December) or grade eleven (in case they were born between January and June) at the time of the interview.

<sup>17</sup>The numbers are calculated as follows:  $(265/8-265/9)$ [average weekly increase in class hours due to the reform]\*39.5[weeks of school per year]\*5.5[years in high school so far] (or \*6.5 years respectively).

<sup>18</sup>For the sample under consideration, the cutoff date was equal among all federal states and set to June 30. Hence, students born between January and June entered first grade six years after their year of birth, and students born between July and December entered first grade seven years after their year of birth.



belong to the control group).  $STATE_s$  is a set of state dummies and  $YEAR_t$  dummies indicate the year of birth.  $X_i$  is a vector of pre-reform individual characteristics, including the student's own demographic characteristics as well as childhood and family variables.

Crucial to the identification of the prime parameter of interest  $\alpha$  as a causal impact of education on cognitive skills, is the assumption that in absence of the reform, cognitive skills of students from the treatment group and of students from the control group do not differ significantly, i.e.  $\alpha = 0$ . This implies that cognitive skills develop similarly among students across states. While this so-called Common Trend Assumption is not testable, it should be reasonable and not too restrictive in this case: here, students of the same school type are compared across different states. Since students select into different school types very likely based on initial abilities and socio-economic background, I assume that students differ severely across different types of secondary school and therefore also in their development of competences. In contrast, students *at high school* but living in different states can be expected to possess rather similar initial characteristics.

Furthermore, self-selection should not be possible to enable a causal interpretation of the results. As the reform was introduced state-wide at the same time,<sup>19</sup> students did not have a choice on whether to be affected by the reform or not. Hence selection *within* the sample, i.e. between treatment and control group, was hardly possible.<sup>20</sup> It could however be, that students attended high school in a different state instead which had not yet introduced the reform. This however imposes high moving costs for the entire family and therefore seems unlikely. In contrast, selection *out of* the sample is possible by attending a different type of secondary school instead of high school. With the exception of comprehensive schools in some states, the graduation from the lower and intermediate secondary school does not lead to the high school diploma. Given the ever

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<sup>19</sup>The only exception to this is Hesse where the reform was gradually introduced for students newly entering high school in the school year 2004/05 (10% of all schools), 2005/06 (60%), 2006/07 (30%). Therefore, I only include students from Hesse who entered high school in 2003 or earlier and were therefore not affected, and students who entered high school in 2006 or later and were therefore affected.

<sup>20</sup>As the reform has only been announced and implemented *after* these students had entered primary school already, there is no way students could change their grade level apart from repeating or skipping a class. Note that when being in the first cohort affected, skipping a class to escape the reform would not have made sense as one would graduate at the exact same time as originally. The same holds true when repeating a class with the reverse aim. In any other cohort which was neither the first affected nor the last non-affected cohort, no such behavior would have changed the treatment status.

growing importance of educational certificates on the labor market, this is a far-reaching decision and can therefore be assumed to be relatively rarely a direct implication of the newly introduced reform.

Lastly, the timing of the implementation of the reform may be related to certain state specific characteristics. According to Black et al. (2005) it is not crucial for my identification for the reform to be unrelated to these as I control for state fixed effects in the analysis. Nevertheless, see Dahmann and Anger (2014) who investigate the pattern of implementation of the reform. They find that it is unrelated to the percentage of high school students in a state's population, to whether the government is conservative, to whether the next state elections were scheduled for 2001/2002, or to the state's GDP per capita. There is suggestive evidence that states with a higher median age of residents implemented the reform slightly earlier; an artifact caused by the older population in East German states.

## 5.2 Quasi-natural Experiment using NEPS

For the second analysis, all students in the selected NEPS sample are in their final grade of high school. Students affected by the reform are therefore in grade twelve, while students not affected by the reform attend grade thirteen. However, both groups are at the same educational stage as they have accumulated the same quantity of instruction received, i.e. the accumulated number of class hours. At each school, both of these groups therefore attend the same class together in this final year. However, the students affected by the reform have received this educational instruction at a younger age compared to their non-affected counterparts. As this earlier instruction may have increased their cognitive skills at a younger age<sup>21</sup>, students affected by the reform may possess a higher stock of skills than same-aged student not affected by the reform. According to Cunha and Heckman (2007), this higher stock of already existing skills is assumed to make investment more productive. Hence the instruction received thereafter may have larger benefits. Due to this multiplier effect, students affected by the reform may possess higher cognitive

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<sup>21</sup>This is the case if the reform induced higher cognitive skills for same-aged students. Looking at the age of seventeen, this can be deduced from the results described in section 6.1.

skills than non-affected students at the end of secondary education. However, affected students are also, on average, one year younger than non-affected students, at the end of secondary education. Hence it is lastly an empirical question whether and to what extent the multiplier effect can be found or is offset by potential age effects.

To estimate this relationship between the timing of education and students' cognitive skills, I estimate a reduced version of equation (2) as there is no variation over time and across state in this sample:

$$y_{ij} = \alpha \text{REFORM}_i + X_i \beta + \varepsilon_{ij}, \quad (3)$$

where  $y_{ij}$  is a measure of cognitive ability of person  $i$  at school  $j$ ,  $\text{REFORM}_i$  is a dummy indicating whether person  $i$  is affected by the reform ( $\text{REFORM}_i = 1$ ) or not ( $\text{REFORM}_i = 0$ ), and  $X_i$  is a vector of individual characteristics. The error terms  $\varepsilon_{ij}$  are clustered at the school level. The prime parameter of interest  $\alpha$  indicates the role of the timing of the instruction received in students' cognitive skill development: a positive  $\alpha$  could prove the existence of the multiplier effect, proposed by Cunha and Heckman (2007) for early life interventions, even in adolescence. A negative  $\alpha$  in turn, could stem from potential age effects. Of course both effects may not be present at all or offset each other, yielding inconclusive results.

To interpret this relationship as causal, it is crucial that the reform indeed constitutes a quasi-natural experiment. For this to hold, no selection should be possible and the treatment and control group should be comparable in terms of observable and unobservable characteristics. As the reform was introduced state-wide at the same time, students did not have a choice on whether to be affected by the reform or not, hence selection to treatment or control group *within* this sample can be ruled out.<sup>22</sup> However, selection *out of* the sample may have been possible, but is again assumed to be unlikely.<sup>23</sup> Fur-

<sup>22</sup>Similarly to the discussion on selection in section 5.1, again the only way to change from the treatment to control group (or vice versa) would be by skipping a class (repeating a class). As both groups however end up in the exact same class, this would not have made sense.

<sup>23</sup>Cases where this could happen are if students drop out of this double cohort either by repeating or skipping a class. The latter is however extremely rare. And the former would not have changed the fact of being affected by the reform, hence it would be selection unrelated to the implementation of the reform, which should therefore not pose a threat to the identification. A further possibility would be

thermore, Table 12 shows that treatment and control group are comparable with respect to the selected observable characteristics. While the comparability of unobserved characteristics in turn cannot be tested formally, the well-balanced observed characteristics together with the absence of self-selection point to the validity of this assumption.

## 6 Results

### 6.1 The Impact of Instructional Time on Cognitive Skills

The results of the estimation of model (2) are presented in Table 3. The few salient effects of the individual control variables that reveal statistical significance<sup>24</sup> are in line with expectations: while students with migration background show comparably less fluid skills, students who did not receive a recommendation to follow onto high school after primary school, have acquired less crystallized competences.

It can be seen that, on average, the increased instruction quantity induced by the reform has no significant impact on students' cognitive abilities.<sup>25</sup> Yet, the positive sign across all dimensions is in line with theory. However, when allowing for effect heterogeneity by gender (see Table 4), it can be seen that these are driven by improvements among male students: While there is virtually no effect across female students, male students' numerical skills clearly improve by more than a quarter of a standard deviation following the reform. Given that male students in this sample outperformed female students in numerical abilities even before the introduction of the reform, it is notable that here the increased instruction quantity is an aggravating, rather than mitigating, factor in gender skill differences. The initial dominance of male students in numerical skills also provides two potential reasons for why instructional time may be more beneficial for male students than for female students. On the one hand, the initially higher numerical skills can shape

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to move to a different state where the reform had not been introduced so far. Involving high moving costs for the entire family, this option seems highly unlikely; nonetheless see 5.1 for a more extensive discussion of potential selection.

<sup>24</sup>Note that due to the small sample size, effects here may fail to exhibit statistical significance, even though they would in a larger sample.

<sup>25</sup>Note that the absence of statistical significance does not necessarily imply a zero effect. Given the relatively small sample size, a lack of statistical power can naturally be expected.

Table 3: Average Effects of the Reform

	Outcome Variables: Cognitive Skills		
	Crystallized		Fluid
	Verbal	Numerical	Figural
Reform	0.095 (0.095)	0.127 (0.138)	0.103 (0.111)
Female	0.012 (0.052)	0.022 (0.078)	0.014 (0.044)
Migration background	-0.161 (0.179)	-0.175 (0.168)	-0.312*** (0.063)
Birth term Jan-Jun	0.083 (0.064)	-0.010 (0.091)	0.082 (0.108)
Low-performing student	-0.276*** (0.091)	-0.178** (0.080)	-0.049 (0.086)
Rural area	0.011 (0.100)	0.031 (0.072)	0.063 (0.077)
High parental education	-0.009 (0.102)	-0.074 (0.104)	-0.013 (0.079)
Working-class father	-0.196* (0.094)	0.104 (0.115)	-0.098 (0.096)
Working mother	0.032 (0.052)	0.088 (0.106)	0.015 (0.080)
Non-intact family	-0.005 (0.096)	0.034 (0.071)	0.072 (0.111)
R <sup>2</sup>	0.099	0.075	0.100
Observations	706	706	706

*Notes:* SOEPv30 waves 2006 to 2013. OLS regressions. A maximum set of state dummies, year of birth dummies, dummies for the different SOEP samples, and a constant are included. Standard errors, reported in parentheses, are clustered at the state level.\* p<0.1, \*\* p<0.05, \*\*\* p<0.01.

male preferences for choosing especially mathematically demanding subjects.<sup>26</sup> On the other hand, the high initial numerical skills especially among male students constitute a higher stock of already existing skills. According to the assumption of Cunha and Heckman (2007) that investment is more productive when existing skills are higher, the increase in instruction quantity benefits especially those with already higher skills, i.e. male students in the domain of numerical skills. Like this, education seems to improve

<sup>26</sup>In Germany the choice of the major fields of study is only possible in the last two years of high school. After the reform this choice therefore takes place one year earlier than it did before the reform. However, as interviews are largely conducted in the first quarter of the year a student turns seventeen years old, most students in my sample are not able to choose major fields yet (or did so only very recently). Even if they were, note that as mathematics belongs to the core subjects it cannot be eliminated by any of the students from their timetable.

Table 4: Heterogeneous Effects of the Reform by Gender

	Outcome Variables: Cognitive Skills		
	Crystallized		Fluid
	Verbal	Numerical	Figural
Reform	0.104 (0.113)	0.277** (0.125)	0.151 (0.119)
Reform*Female	-0.018 (0.145)	-0.294*** (0.088)	-0.094 (0.093)
R <sup>2</sup>	0.099	0.080	0.101
Observations	706	706	706

*Notes:* SOEPv30 waves 2006 to 2013. OLS regressions. A maximum set of state dummies, year of birth dummies, dummies for the different SOEP samples, and a constant are included. Individual characteristics controlled for include female, migration background, birth term Jan-Jun, low-performing student, rural area, high parental education, working-class father, working mother, and non-intact family. Standard errors, reported in parentheses, are clustered at the state level.\* p<0.1, \*\* p<0.05, \*\*\* p<0.01.

especially domains of skills with comparative advantages among the respective group of students. With regard to the remaining domains, verbal as well as figural skills show slightly better improvements among male students than among females following the reform. For both groups however effects fail to exhibit statistical significance.

## 6.2 The Impact of Timing of Instruction on Cognitive Skills

Table 5 shows the preliminary results of the estimation of equation (3). Again, it should be kept in mind that the crystallized measures of cognitive skills are simple percentage scores of the number of correctly solved items and may therefore not necessarily reflect the true domain-specific cognitive ability due to the multi-matrix design in the assessment tests. Results are therefore very preliminary and should be interpreted with caution.

The effects of the individual control variables on cognitive abilities are in line with expectations: Whereas children with migration background score relatively lower on the competence measures, students from high socio-economic backgrounds, measured by parental education or the number of books at home, have acquired higher skills especially in the crystallized domains of competences.

However, the reform exhibits no statistically significant effects on the different di-

Table 5: Average Effects of the Reform

	Outcome Variables: Cognitive Skills				
	Crystallized			Fluid	
	<i>Physics</i>	<i>English</i>	<i>Biology</i>	Speed	Reasoning
Reform	<i>0.022</i> (0.047)	<i>-0.038</i> (0.050)	<i>-0.018</i> (0.037)	-0.048 (0.066)	-0.082* (0.047)
Female	<i>-0.000</i> (0.050)	<i>-0.006</i> (0.037)	<i>-0.002</i> (0.051)	-0.002 (0.055)	0.004 (0.050)
Migration background	<i>-0.171**</i> (0.071)	<i>0.039</i> (0.057)	<i>-0.115**</i> (0.056)	-0.102 (0.061)	-0.167*** (0.055)
Birth term Jan-Jun	<i>0.062</i> (0.043)	<i>0.038</i> (0.040)	<i>0.042</i> (0.041)	0.087* (0.047)	-0.025 (0.049)
High parental education	<i>0.090*</i> (0.051)	<i>0.084**</i> (0.042)	<i>0.203***</i> (0.044)	-0.067 (0.046)	0.000 (0.055)
Working-class father	<i>-0.053</i> (0.070)	<i>-0.081</i> (0.079)	<i>-0.045</i> (0.069)	0.052 (0.076)	0.061 (0.070)
Working mother	<i>-0.034</i> (0.072)	<i>0.013</i> (0.061)	<i>-0.106*</i> (0.058)	-0.032 (0.063)	0.054 (0.056)
Books at home	<i>0.104**</i> (0.047)	<i>0.147***</i> (0.048)	<i>0.238***</i> (0.049)	0.010 (0.055)	0.013 (0.046)
R <sup>2</sup>	<i>0.014</i>	<i>0.012</i>	<i>0.037</i>	0.005	0.007
Observations	<i>2123</i>	<i>2126</i>	<i>2124</i>	2128	2128

*Important Remark:* Regression results in *italic* are very preliminary as the outcome variables are simple percentage scores and may not necessarily reflect the true domain-specific cognitive ability.

*Notes:* NEPS:BW:2.0.0 wave 2011/2012. OLS regressions. Further a constant is included. Standard errors, reported in parentheses, are clustered at the school level. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01.

mensions of crystallized intelligence, neither on average nor when allowing for gender heterogeneity.<sup>27</sup> This indicates that students affected by the reform have caught up with their non-affected counterparts in terms of crystallized competences. However, these estimates also reveal that the age-respective timing of instruction does not influence skill formation in crystallized domains, for two potential reasons: on the one hand, there may be neither a positive multiplier effect of earlier investment present nor an age effect benefiting older students' competences. On the other hand, both effects may be present, but offset each other. While it is not possible to disentangle these two mechanisms in this setting, the effects on fluid intelligence may give additional valuable insights.

<sup>27</sup>Even though a lack of statistical significance could be a consequence of the sample size, the 95% confidence intervals range between -0.14 and +0.11. Hence, in any case effect sizes would not be comparable to the impact of the increased instruction time.

Table 6: Heterogeneous Effects of the Reform by Gender

	Outcome Variables: Cognitive Skills				
	Crystallized			Fluid	
	<i>Physics</i>	<i>English</i>	<i>Biology</i>	Speed	Reasoning
Reform	<i>0.003</i> (0.069)	<i>0.046</i> (0.073)	<i>-0.036</i> (0.060)	-0.066 (0.087)	-0.099 (0.067)
Reform*Female	<i>0.034</i> (0.088)	<i>-0.149</i> (0.092)	<i>0.031</i> (0.087)	0.031 (0.095)	0.031 (0.089)
R <sup>2</sup>	<i>0.014</i>	<i>0.013</i>	<i>0.038</i>	0.005	0.007
Observations	<i>2123</i>	<i>2126</i>	<i>2124</i>	2128	2128

*Important Remark:* Regression results in *italic* are very preliminary as the outcome variables are simple percentage scores and may not necessarily reflect the true domain-specific cognitive ability.

*Notes:* NEPS:BW:2.0.0 wave 2011/2012. OLS regressions. Individual characteristics controlled for include female, migration background, birth term Jan-Jun, high parental education, working-class father, working mother, and books at home. Further a constant is included. Standard errors, reported in parentheses, are clustered at the school level. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01.

Investigating the reform's effects on fluid measures of intelligence, the estimates show that there is no significant impact on processing speed but that reasoning ability as measured by Raven's matrices test is significantly lower for students affected by the reform. As fluid intelligence is assumed not to be directly affected by any type of investment, no positive multiplier effect of earlier instruction could be expected. Nonetheless, fluid intelligence *is* assumed and found to change with age where it is increasing during childhood and adolescence. The estimated decrease of around eight percent of a standard deviation therefore most likely stems from the age difference of one year, on average, between affected and non-affected students, but should not be related to the curriculum covered at which age. Furthermore, these results on fluid intelligence can be taken into account to interpret the zero effects on crystallized dimensions: Given the students' performance in the tasks to assess reasoning ability, age effects in cognitive skill formation seem still to be present in late adolescence benefiting older students. If this was true for all dimensions of cognition, age effects can be expected to influence crystallized dimensions as well. Hence, the zero effects of the reform among students of the same educational level, may likely be the result of an interaction of this age effect offset by a positive multiplier effect of earlier investment. The latter is underlined by the first analysis showing, based on the SOEP



data, that the earlier instruction could indeed be absorbed, at least by some students, leading to higher crystallized abilities among affected students at the age of seventeen.

## 7 Sensitivity Analyses and Outlook

To confirm the positive effect of the increased instructional time, at least among male students, I conduct several robustness checks. First, I exclude all students aged eighteen or nineteen years from my sample. I have included birth year dummies in all specifications; however, this is another way to rule out any potential age effects. The results (see Table 13) show that the positive impact of the instruction time on males' numerical skills is completely preserved. Fluid intelligence scores however, increase as well in this specification. Important to note is though again, that it is unlikely that crystallized and fluid skills can be assessed completely separately in a test environment. Especially among male students of my sample under investigation, the correlation between numerical and figural skills amounts to 0.39. Second, I exclude students from Saxony and Thuringia from my sample, as in these states the reform was actually not introduced but they continued their tradition of a twelve-year-schooling system as before (see Table 14). Although effect sizes and statistical significance decrease, potentially due to a loss in sample size, males' numerical skills still benefit compared to female students' cognitive skills. Lastly, I use the sample of students who follow the secondary school tracks *other* than high school in a Placebo estimation to verify the empirical strategy. As the high school reform did not affect these students, it should show no effects on these students' cognitive skills. Results indicate that this is indeed not the case on average (see Table 15). When allowing for heterogeneity, there appear to be slight gender differences; however, in no case these effects are statistically significant individually.

At this point in time, there are however still further robustness checks and extensions to the existing analyses in order, if allowed by the sample size:

1. To rule out any self-selection in the SOEP sample, I could include only late-adopter states of the reform, which are neighboring states where the reform has been imple-

mented already. This rules out any self-selection from the treatment into the control group by students or their parents moving to a late-adopter state to avoid the reform.

2. In some states, the university entrance qualification can still be obtained after a total of 13 years of schooling at comprehensive schools. Although students from comprehensive schools are excluded in my sample, the option could have affected selection into high school. However, comprehensive schools are not equally common to all federal states. Hence, regarding only students in states where comprehensive schools typically do not exist, would further rule out selection.
3. To shed more light on the timing of instruction, further heterogeneous effects apart from gender could be analyzed using the NEPS sample. Differential effects of timing especially with respect to socio-economic background, could give valuable insights for policy makers to decrease inequality.

## 8 Conclusion

As cognitive skills are important determinants of many economic and social outcomes, higher cognitive skills are often correlated with higher education. However, it is not only recognized that individuals with higher cognitive abilities are likely to be better educated, but also that education improves cognitive skills. Most studies use changes in compulsory schooling laws as an exogenous variation to identify causal positive effects of an additional year of schooling. However, there is no evidence on the underlying mechanisms.

This paper provides first evidence on the mechanisms through which education may improve cognitive skills in adolescence. I exploit a German high school reform carried out at the state-level between 2001 and 2007 as a quasi-natural experiment to estimate causal effects of this educational change on adolescents' cognitive abilities. Based on two separate analyses using SOEP and NEPS data, this study successfully disentangles the differential effects of instruction by focusing on quantity on the one hand and allocation with respect to age on the other hand. The improvement of crystallized intelligence

through instructional time among seventeen-year old male students by up to 0.3 standard deviations is comparable to the effect size of one additional year of schooling in other countries. To the best of my knowledge, it is the first study pointing at important heterogeneous effects by gender indicating that educational quantity aggravates, instead of mitigates, gender skill differences by extending comparative advantages. In contrast, a positive multiplier-effect that could result from this skill acquisition at younger ages does not seem to outweigh potential age effects until graduation. The differential age-respective timing of educational instruction does therefore not significantly alter cognitive skill development when comparing crystallized measures of competences of students affected by the reform and students not affected by the reform at the end of high school. As fluid intelligence is generally not assumed to change over the life cycle in response to factors other than age, no positive multiplier effect can be expected for the reform to increase fluid components of intelligence. The age gap therefore even yields lower scores for students affected by the reform compared to their non-affected counterparts. Lastly, these results can be drawn onto for the evaluation of the reform: they may justify the maintenance of the curriculum while shortening high school duration as students seem to absorb the higher load of subject matters taught. Apart from lower reasoning scores, which may be attributable to the age difference, preliminary results suggest that high school graduates are just as equally off before and after the reform in terms of acquired competences.

I conclude from these analyses that in the positive impact education has on cognitive skills the relevant factor is not (only) school duration but especially the amount of content taught. There is, however, important effect heterogeneity: With respect to gender, initial skill differences are further aggravated through an increased curriculum. For decision makers this opens up new possibilities to target cognitive ability, other than simply changing overall school duration when designing educational policies. However, differential effects need to be taken into consideration to avoid increasing inequality.

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## A Variables

Table 7: Description of Variables in SOEP

Variable	Description
Verbal	Standardized measure for verbal skills
Numerical	Standardized measure for numerical skills
Figural	Standardized measure for figural skills
Age	Age (in years)
Female	Dummy for female
Migration background	Dummy for student with migration background
Birth term Jan-Jun	Dummy for being born between January and June
Low-performing student	Dummy for not having received a recommendation for high school after the fourth grade
Rural area	Dummy for having lived most of the childhood until age 15 in rural area
High parental education	Dummy for at least one of an individual's parents having an upper secondary school degree or higher
Working-class father	Dummy for father having blue-collar occupation when student is aged 15, reference category encompasses <i>all</i> others
Working mother	Dummy for working mother (both full-time and part-time) when student is aged 10
Non-intact family	Dummy for not having lived with <i>both</i> parents for the entire time up to age 15

*Notes:* SOEPv30 waves 2006 to 2013.

Table 8: Description of Variables in NEPS

Variable	Description
<i>Physics</i>	<i>Standardized measure of percentage of correctly answered questions on Physics test</i>
<i>English</i>	<i>Standardized measure of percentage of correctly answered questions on English test</i>
<i>Biology</i>	<i>Standardized measure of percentage of correctly answered questions on Biology test</i>
Speed	Standardized measure for processing speed
Reasoning	Standardized measure for reasoning ability
Female	Dummy for female
Migration background	Dummy for at least one of the student's parents born abroad
Birth term Jan-Jun	Dummy for being born between January and June
High parental education	Dummy for at least one of an individual's parents having an upper secondary school degree or higher
Working-class father	Dummy for father currently having blue-collar occupation, reference category encompasses <i>all</i> others
Working mother	Dummy for mother currently working (both full-time and part-time)
Books at home	Dummy for having a book shelve of at least average size at home

*Important Remark:* The crystallized dimensions of cognitive ability denoted in *italic* are simple percentage scores and may not necessarily reflect the true domain-specific cognitive ability for which scores should be computed based on Item Response Theory.

*Notes:* NEPS:BW:2.0.0 wave 2011/2012.

## B Summary Statistics

Table 9: Summary Statistics of (non-standardized) Scores on Cognitive Skills Tests in SOEP

	Observations	Mean	Std. Dev.	Minimum	Maximum
<b>Males</b>					
Verbal	331	11.290	3.200	3	20
Numerical	331	15.792	3.766	3	20
Figural	331	11.202	3.123	3	18
<b>Females</b>					
Verbal	376	10.471	3.117	3	18
Numerical	376	14.122	4.102	3	20
Figural	376	11.388	2.854	1	18

*Notes:* SOEPv30 waves 2006 to 2013, sample: adolescents aged 17 to 19 attending high school.

Table 10: Summary Statistics of Individual Characteristics in SOEP

	Mean		Equality of Means
	Control	Treatment	t-stat
Age	17.429	17.063	8.105
Female	0.545	0.512	0.855
Migration background	0.164	0.169	-0.193
Birth term Jan-Jun	0.550	0.460	2.357
Low-performing student	0.166	0.193	-0.926
Rural area	0.270	0.330	-1.709
High parental education	0.628	0.539	2.372
Working-class father	0.159	0.214	-1.874
Working mother	0.735	0.800	-2.003
Non-intact family	0.199	0.193	0.199
Observations	422	285	

*Notes:* SOEPv30 waves 2006 to 2013, sample: adolescents aged 17 to 19 attending high school. Note that age differs by construction of the sample, as only in wave 2006 eighteen and nineteen year-olds were included in the adolescent sample as well. As at this point in time, in most states the reform was not implemented yet, the age is higher among the control group.



Table 11: Summary Statistics of (non-standardized) Scores on Cognitive Skills Tests in NEPS

	Observations	Mean	Std. Dev.	Minimum	Maximum
<b>Males</b>					
<i>Physics</i>	930	0.521	0.186	0.118	1
<i>English</i>	932	0.786	0.188	0	1
<i>Biology</i>	930	0.606	0.150	0.147	1
Speed	933	63.174	12.576	0	93
Reasoning	933	10.937	1.136	5	12
<b>Females</b>					
Physics	1193	0.403	0.152	0	0.947
English	1194	0.822	0.169	0	1
Biology	1194	0.582	0.139	0.121	0.971
Speed	1195	66.300	10.548	0	93
Reasoning	1195	10.643	1.321	1	12

*Important Remark:* The crystallized dimensions of cognitive ability denoted in *italic* are simple percentage scores and may not necessarily reflect the true domain-specific cognitive ability for which scores should be computed based on Item Response Theory.

*Notes:* NEPS:BW:2.0.0 wave 2011/2012, sample: high school students in final grade born between 1991 and 1995.

Table 12: Summary Statistics of Individual Characteristics in NEPS

	Mean		Equality of Means
	Control	Treatment	t-stat
Age	19.431	18.486	40.530
Female	0.563	0.561	0.089
Migration background	0.207	0.202	0.271
Birth term Jan-Jun	0.472	0.438	1.591
High parental education	0.634	0.614	0.990
Working-class father	0.170	0.186	-0.935
Working mother	0.864	0.853	0.752
Books at home	0.665	0.635	1.439
Observations	1015	1113	

*Notes:* NEPS:BW:2.0.0 wave 2011/2012, sample: high school students in final grade born between 1991 and 1995.

## C Estimation Results

Table 13: Subsample Age 17 – Heterogeneous Effects of the Reform by Gender

	Outcome Variables: Cognitive Skills		
	Crystallized		Fluid
	Verbal	Numerical	Figural
Reform	0.212 (0.159)	0.258** (0.118)	0.244* (0.121)
Reform*Female	-0.071 (0.168)	-0.260** (0.108)	-0.127 (0.115)
Female	0.088 (0.121)	0.141 (0.106)	0.142* (0.070)
Migration background	-0.136 (0.207)	-0.164 (0.206)	-0.229** (0.080)
Birth term Jan-Jun	0.110 (0.076)	-0.017 (0.105)	0.080 (0.101)
Low-performing student	-0.312** (0.117)	-0.233** (0.107)	-0.059 (0.122)
Rural area	0.074 (0.095)	-0.003 (0.084)	0.099 (0.090)
High parental education	-0.014 (0.119)	-0.042 (0.108)	0.020 (0.079)
Working-class father	-0.273* (0.129)	0.089 (0.114)	-0.223** (0.103)
Working mother	-0.015 (0.073)	0.065 (0.136)	0.037 (0.102)
Non-intact family	-0.031 (0.106)	-0.001 (0.070)	0.065 (0.135)
R <sup>2</sup>	0.096	0.083	0.095
Observations	569	569	569

*Notes:* SOEPv30 waves 2006 to 2013. OLS regressions. A maximum set of state dummies, year of birth dummies, dummies for the different SOEP samples, and a constant are included. Standard errors, reported in parentheses, are clustered at the state level.\* p<0.1, \*\* p<0.05, \*\*\* p<0.01.

Table 14: Subsample of Federal States (without Saxony and Thuringia) – Heterogeneous Effects of the Reform by Gender

	Outcome Variables: Cognitive Skills		
	Crystallized		Fluid
	Verbal	Numerical	Figural
Reform	0.136 (0.142)	0.164 (0.097)	0.146 (0.132)
Reform*Female	0.041 (0.154)	-0.350*** (0.085)	-0.074 (0.109)
Female	0.023 (0.051)	0.144 (0.088)	0.051 (0.057)
Migration background	-0.166 (0.185)	-0.168 (0.172)	-0.274*** (0.064)
Birth term Jan-Jun	0.048 (0.073)	-0.069 (0.078)	0.047 (0.110)
Low-performing student	-0.253** (0.095)	-0.190** (0.081)	-0.068 (0.098)
Rural area	-0.015 (0.109)	0.003 (0.073)	0.061 (0.090)
High parental education	-0.075 (0.091)	-0.047 (0.117)	0.015 (0.087)
Working-class father	-0.253** (0.095)	0.195 (0.122)	-0.142 (0.100)
Working mother	0.037 (0.053)	0.089 (0.115)	0.013 (0.081)
Non-intact family	0.043 (0.108)	0.070 (0.081)	0.126 (0.124)
R <sup>2</sup>	0.100	0.089	0.106
Observations	648	648	648

*Notes:* SOEPv30 waves 2006 to 2013. OLS regressions. A maximum set of state dummies, year of birth dummies, dummies for the different SOEP samples, and a constant are included. Standard errors, reported in parentheses, are clustered at the state level.\* p<0.1, \*\* p<0.05, \*\*\* p<0.01.

Table 15: Placebo Estimation – Average Effects of the Reform on Students from Lower and Intermediate Secondary School

	Outcome Variables: Cognitive Skills		
	Crystallized		Fluid
	Verbal	Numerical	Figural
Reform	-0.090 (0.079)	0.032 (0.140)	0.041 (0.101)
Female	0.030 (0.038)	0.015 (0.051)	-0.019 (0.083)
Migration background	-0.205*** (0.057)	-0.238*** (0.060)	-0.265*** (0.067)
Birth term Jan-Jun	0.059 (0.057)	0.058 (0.063)	0.049 (0.048)
Low-performing student	-0.374*** (0.069)	-0.274*** (0.055)	-0.285*** (0.041)
Rural area	0.157* (0.080)	0.114 (0.073)	0.048 (0.060)
High parental education	0.318*** (0.093)	0.087 (0.070)	0.174** (0.079)
Working-class father	-0.287*** (0.082)	-0.065 (0.059)	-0.133* (0.069)
Working mother	0.095 (0.062)	0.167*** (0.055)	0.167*** (0.053)
Non-intact family	-0.064 (0.053)	-0.096 (0.061)	-0.146** (0.052)
R <sup>2</sup>	0.160	0.099	0.101
Observations	1153	1153	1153

*Notes:* SOEPv30 waves 2006 to 2013. OLS regressions. A maximum set of state dummies, year of birth dummies, dummies for the different SOEP samples, and a constant are included. Standard errors, reported in parentheses, are clustered at the state level.\* p<0.1, \*\* p<0.05, \*\*\* p<0.01.