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# What a difference three years of economics education make: Evidence from lower stream schools in Germany

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## ABSTRACT

A large body of literature documents that school-based financial education generally improves financial knowledge, yet little is known about the effect of instruction in the broader economic domain. This paper evaluates the effect of a curriculum reform introducing mandatory economic education on economic competence and knowledge in German lower stream schools, in which students have lower socio-economic status and end up having lower incomes when entering the workforce. While we find small but positive effects on basic economic knowledge and interest in economic matters, we observe insignificant effects on competences, i.e., factual and procedural knowledge in the economic domain. Quantile regressions reveal that the effect on students' knowledge is widely consistent across the entire distribution. With regard to socio-demographic characteristics, we observe strong gender differences already before adulthood.

## 1. Introduction

Economic and financial knowledge are key requirements for teenagers and young adults to make confident and independent decisions within economically shaped life situations. A large body of literature documents that these domain-specific capabilities are significantly associated with retirement preparedness (Bucher-Koenen and Lusardi, 2011; Lusardi and Mitchell, 2008), stock market participation (Almenberg and Dreber, 2015; van Rooij et al., 2012), and wealth accumulation in general (Lusardi et al., 2017). Thus, national economic and financial education initiatives have been spurred around the globe with the goal to implement domain-specific education into general school curricula (OECD, 2020).

While interventions in the narrower financial domain have shown to improve financial knowledge and enhance financial decision-making among teenagers (see Kaiser and Menkhoff, 2020 for a meta-analysis of school-based interventions), evidence in the context of natural policy experiments is mixed: Some studies conclude that mandatory financial education is ineffective in fostering financial knowledge and long-term credit card behaviors (Brown et al., 2016; Cole et al., 2016), whereas Urban et al. (2018) document positive effects of U.S. high school mandates on both outcomes once heterogeneity in teacher training and quality of implementation are sufficiently addressed in the econometric framework. Nevertheless, most natural experiments on the effectiveness of educational mandates address the narrow financial domain with focus on consumer problems (such as handling debts and savings) without incorporating broader economic phenomena. Also, as most of these studies are conducted in the U.S., due to the growing number of federal states with high school financial education mandates, quasi-experimental evidence stemming from Europe remains particularly

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scarce.

In Germany, in particular, state-wide implementations of financial or economic education mandates have been limited in the past (Kaiser et al., 2021). Economics was either included in curricula of related subjects (e.g., social sciences) or offered as an elective in certain school types. While there are explorative studies on financial capabilities among German youths (Schuhen and Schürkmann 2014; Erner et al., 2016, Förster et al., 2018, 2019; Happ and Förster 2019) as well as on economic competences (e.g., Happ et al., 2016, 2018, 2021; Förster and Happ, 2019; Oberrauch and Kaiser, 2020), there is almost no evidence on the impact of state-wide curriculum reforms relying on a (plausible) exogenous variation.

Recently, the German federal state of Baden-Wuerttemberg broke new ground and introduced economics as a mandatory stand-alone subject for grades 7–10 in lower stream schools.<sup>1</sup> We use this exogenous variation in exposure to economic education relative to the previous cohort not affected by this mandate to study its impact on economic knowledge and competence among lower stream secondary school students. For this purpose, we defined three main outcomes and formulated related research hypotheses. First, we investigate the impact of the new curriculum on factual economic knowledge. Domain-specific knowledge, especially with focus on the narrower financial subdomain (“financial literacy”), has been shown to be a reliable predictor of various financial outcomes, such as planning for retirement (Bucher-Koenen and Lusardi, 2011; Lusardi and Mitchell, 2008) or wealth accumulation in general (Lusardi et al., 2017). Further, domain-specific knowledge enables young individuals to participate in economically-shaped life situations and may foster the acquisition of domain-specific competences. As rigorous meta-evidence suggests that educational interventions affect adolescents’ knowledge in the narrower financial domain (Kaiser and Menkhoff, 2020), we hypothesize that students affected by the new curriculum in the German federal state Baden-Wuerttemberg show higher levels of economic knowledge. Second, as most educational large-scale assessments seek to survey competences (i.e., declarative and procedural knowledge in a specific domain) instead of content-oriented knowledge, we additionally explore curriculum effects on economic competences that incorporate cognitive dispositions to cope with economically shaped life situations. A recent study has shown that the new curriculum fosters economic competences in German higher stream schools (Kaiser and Oberrauch, 2021), which is why we hypothesize that the reform shows similar effects in lower stream schools. Third, we examine the effects of the new mandate on interest in economic matters. Previous studies have shown that financial education interventions are also effective in fostering domain-specific interest (e.g., Lührmann et al., 2015). Therefore, our hypothesis suggests a positive impact of the curriculum on interest in the broader economic domain, as the new materials for teachers and students may have a motivating and engaging effect on interest in learning.

By surveying two distinct representative cohorts (those affected and those not affected by the reform) of 9th graders in 2019 and 2020, we seek to investigate differences between the two groups and provide a comparison into the ex-post levels of students’ economic capabilities. The natural design of our study alleviates the possibility of self-selection into the treatment group (Oberrauch and Seeber, 2022). As opposed to the 9th grade surveyed in 2019, the 9th grade surveyed in 2020 received three years of mandatory economic instruction. We conducted the natural experiment with 1829 students in 98 schools.

Our study results in four main findings: First, three years after implementing the mandate, we tentatively find positive treatment effects on *economic knowledge* and on *interest in economic topics* of the order of 0.12 and 0.1 standard deviation units, respectively. The extent of these effects is robust to controlling for various individual characteristics, and relying on a matched-sample, but is only statistically significant at the ten-percent level. While the effect sizes on *economic competence* appear similar, the estimated 90-percent confidence interval includes the possibility of zero-effects. Second, quantile regression estimation reports that the effects are consistent along different distribution moments indicating that the mandate may be suitable for students of all ability levels. Third, we observe meaningful variation of economic knowledge scores across socio-demographic characteristics. Specifically, economic knowledge and competence appear to be significantly lower among migrants and students of lower socio-economic status. In line with previous findings (e.g., Amagir et al., 2020; Driva et al., 2016; Kalwij et al., 2019; Lusardi et al., 2010; Oberrauch and Seeber, 2022), we observe a substantial gender gap in favor of male respondents already at these young ages. As we do not observe heterogeneous treatment effects in those potentially disadvantaged subgroups, we conclude that the reform cannot address pre-existing achievement gaps documented in earlier work (Oberrauch and Kaiser, 2020).

This paper adds to a body of literature that has recently investigated the impact of the aforementioned curriculum reform on capabilities, financial behavior, and economic preferences among students attending higher stream schools in Southwestern Germany (Kaiser and Oberrauch, 2021). This paper rather focuses on the effect of the reform among students in lower-tier schools. Most adolescents in this specific group are about to finish their school education and therefore face many important economic decisions that may have a strong influence on their future economic well-being. For instance, they decide whether to continue their schooling, start vocational training, or enter the workforce directly. While Kaiser and Oberrauch (2021) find large treatment effects on students’ skills in higher stream schools, the effects in lower stream schools appear to be small. Consequently, educational planners need to address this heterogeneity across school types in future curriculum adjustments.

The paper proceeds as follows: The next section describes the curriculum reform. Section 3 provides information about the measurement instruments as well as psychometric procedures to measure economic capabilities. Section 4 describes the used sampling procedures and descriptive sample characteristics. Section 5 presents main results as well as results on heterogeneous treatment effects. Section 6 discusses the results and concludes the paper.

<sup>1</sup> Lower secondary schools in Germany prepare students for vocational training after graduation, whereas upper stream school types (Gymnasium) prepare for university studies.

## 2. Curricular reform

The natural experiment relies on a curriculum reform introducing mandatory economic education for all general education schools of the federal state of Baden-Wuerttemberg. The new distinct school subject (Economics/Career and Study Orientation) is taught from grade seven to ten in order to enable students to recognize, cope with, and design economically shaped life situations (KM and Kultusministerium, 2016). This new curriculum is fully based on a conceptual model of economic competence described in Retzmann and Seeber (2016), which fulfills the requirements of the Standing Conference of the Ministers of Education and Cultural Affairs of the German federal states.

### 2.1. Conceptual framework

The starting point of the model are three different life situations young individuals need to master: consumers of goods and services, earners of income (wage- or self-employed), and tax-paying and voting citizens. For each life situation, the model encompasses three competence areas referring to the overall goals of general education, where competence area A (decision-making and rationality) refers to the individual perspective, competence area B (relationships and interaction) refers to the social perspective, and competence area C (system and order) refers to the societal and systemic perspective. The model combines competence areas with economically-shaped life situations into a matrix (see Fig. B3 in Appendix B). In essence, the structure of the new curriculum follows the structure of the model, with references to other domains to acquire interdisciplinary competences. Although schools are independent in organizing curricular contents, an exemplary sequence of the Ministry of Education starts with consumer problems and rational choices, followed by vocational orientation and societal topics.

### 2.2. Treatment intensity

The German school system sorts children into different ability tracks at the end of grade 4. While the most sophisticated school type (Gymnasium) (grades 5–12) provides in-depth general education and prepares students for future university studies, the lower-tier school types (Realschule and Werkrealschule) usually lead to a vocational entrance qualification. The recently introduced comprehensive school type (Gemeinschaftsschule) seeks to accommodate students of all ability levels. The intensity in lower stream schools including the comprehensive school type is five contact hours (i.e., 45 min) over the course of four years (grades 7–10). The total amount is about 160 h, with 45 min per week on average.

### 2.3. Exogeneity assumptions

The natural experiment uses plausible exogenous variation in exposure to a new mandate introducing mandatory economic education. One key requirement for the causal interpretation of treatment effects is that treated students must not self-select into the new mandate. Although selection effects are possible in theory (e.g., students may intentionally repeat a class or parents may decide to move to this federal state due to the reform), we see it as very unlikely in practice. Compared to mathematics or foreign languages, economics is a subject of little importance (see Kaiser and Oberrauch, 2021). Another important exogeneity assumption is balance in observed and unobserved characteristics between the two cohorts before treatment. As this study lacks a baseline cohort prior to the implementation of the mandate, we test for differences between the two (cross-sectional) cohorts across individual observables. We tackle small imbalances in certain characteristics by including all observables into the covariate vector in the regression analyses as well as using propensity score matching (PSM).

## 3. Measurement of economic capabilities

This section describes the measurement of the main outcomes: content-oriented economic knowledge, economic competence, and interest in economic matters. While interest may serve as a prerequisite to acquire economic competence, domain specific knowledge is important to develop and use cognitive strategies. As the curriculum reform aspires to foster economic competences among adolescents, we additionally capture domain-specific competences using an established competence test (Kaiser et al., 2020). We standardized all three measures to have a mean of zero and a standard deviation of 1.

### 3.1. Economic knowledge

To measure factual knowledge in the economic domain, we employed a ten-item performance test covering current economic conditions (e.g., unemployment rate) and content-oriented knowledge (e.g., the required minimum age to take out a loan) (Table B2 in Appendix B). The selection was based on a professional examination of domain-specific knowledge content, using curricula from all over the country and material from a previous study from the specific federal state (Würth and Klein, 2001). This performance test was especially developed for secondary school students and its ten items showed valid psychometric characteristics (for a translated version of the item set, see Table B2 in Appendix B). We analyze factual knowledge using an Item Response Theory model (IRT) (Baker and Kim, 2017; Hambleton and Swaminathan, 1985) that is frequently employed in international large scale assessments. To extract item characteristics and person abilities, we use a two-parameter logistic model (Birnbaum, 1968), formally expressed as:

$$P(X_i = 1 | \theta, \sigma, a_i) = \frac{\exp\{a_i(\theta_v - \sigma_i)\}}{1 + \exp\{a_i(\theta_v - \sigma_i)\}}, \quad (1)$$

where  $\theta_v$  denotes estimated ability for person  $v$ , and  $\sigma_i$  estimated item difficulty for item  $i$  on a common logit scale.  $\sigma_i$  defines a discrimination parameter evaluating how accurate item  $i$  discriminates between low-ability and high-ability students. With regard to model fit statistics (S-X2) (Ames and Penfield, 2015), the chi-square statistics are insignificant for 9 of 10 items indicating that the (one-dimensional) IRT model with two parameters is suitable for our item set. Additionally, results on the model fit statistics serve as suggestive evidence for the unidimensionality of the scale (i.e., only one type of ability is being measured). Table A1 in Appendix A shows item characteristics obtained by the IRT model as well as descriptive statistics of Classical Test Theory (CTT). Overall, the administered test items have shown to have evenly distributed difficulties and to be sufficiently useful for differentiating between high and low-achievers. Further evidence on the construct validity of the scale came from positive correlations between scores on the knowledge test and scores on the Test of Economic Competence (TEC) shown in Kaiser et al. (2020, 235).

### 3.2. Economic competences

Most educational large-scale assessments (such as PISA or TIMSS) seek to capture competences (i.e., cognitive dispositions to solve domain-specific problems) rather than content-orientated knowledge. To measure the cognitive facet of economic competences in our sample, we additionally administered the Test of Economic Competence (TEC) which is described and validated in Kaiser et al. (2020). The test focuses on the cognitive component of economic competence.

The Test of Economic Competence (TEC) is based on a conceptual model of economic competence described in Retzmann and Seeber (2016). The model represents the underlying framework of the new curriculum reform (see Section 2) and defines economic competence as "the ability to act and decide adequately in economically-shaped life situations" (Kaiser et al., 2020, 229). Based on the competence areas, the authors created items that resemble a fictitious decision-making situation (Decision & Rationality), a judgment of actions of other economic agents (Relationship & Interaction) or the analysis systemic relationships (System & Order). With regard to the construct validity of the test, the authors showed evidence on the unidimensionality of the test using Principal Component Analysis (PCA). Evidence on the convergent validity came from meaningful correlations with test scores from adjacent test instruments, such as the Test of Economic Literacy (TEL) (Soper and Walstad, 1987). Moreover, the authors established content validity by means of expert validations and think-aloud studies. Overall, the performance 31-item scale (selected and constructed responses) showed valid psychometric properties and was able to capture the underlying concepts (see Kaiser et al., 2020 for more details). In line with the economic knowledge test described in Section 3.1, we use the two-parameter logistic model displayed in Eq. (1) to extract competence scores.

### 3.3. Interest in economic matters

Interest is considered to be a major factor influencing learning outcomes across all domains (e.g., Krapp and Prenzel, 2011). It is typically defined as the relationship between the person and a specific object, which can be either short-term, i.e., situational, or long-term (Siegfried and Wuttke, 2021). To expand the two cognitive outcomes described in Sections 3.1 and 3.2, we additionally captured students' interest in economic matters, as domain-specific interest may serve as a prerequisite to acquiring economic knowledge and competences. At the same time, interest in economic matters may be considered as a facet of domain-related competences (Retzmann and Seeber, 2016). Various studies have shown that interest in economic topics is a key predictor of economic competences (e.g., Siegfried and Wuttke, 2021; Oberrauch and Kaiser, 2020; Förster et al., 2017) and varies substantially across demographic subgroups, such as gender (e.g., Förster and Happ, 2019; Förster et al., 2018).

To measure economic interest, we use a single item that asks respondents to rate their interest in economic topics on a scale from one (not interested at all) to five (very interested). The item is part of the Survey on Economic Attitudes (SEA) (Walstad and Soper, 1983), which measures general attitudes towards economic topics. With regard to convergent validity, scores on the single item substantially correlated with scores on the SEA ( $r = 0.68$ ;  $p < 0.01$ ) (Oberrauch and Seeber, 2022), i.e., the single item captures the underlying construct in a very efficient way.

## 4. Data

The target population consists of all students in 9th grade attending a public school of the lower stream in the German federal state of Baden-Wuerttemberg. To draw a representative sample, we stratified the whole population into subgroups by school type and level of urbanization (see Oberrauch and Kaiser, 2020 for details). We then employed a two-stage cluster sampling procedure the selection of schools being the first stage. In the second stage, we randomly drew one 9th grade in each school. The number of selected schools in each stratum is adapted to the proportion of the strata in the target population. To reduce the remaining disproportionalities, we used sampling weights calculated by the inverse of the selection probability.

The final sample contains information about 1829 secondary school students (1102 affected by the reform, 727 not affected) out of

107 classes in 98 schools. While the 9th grade not affected by the reform was surveyed in summer 2019, the 9th grade affected by the reform was surveyed one year later in late summer 2020. Due to the pandemic situation in 2020, we administered the survey for affected students two months later than initially planned.<sup>2</sup> The study was conducted during regular school lessons and carefully supervised by the teachers. Detailed information on the sample composition in comparison to official school statistics is listed in [Table A2 in Appendix A](#).

Aside from the three outcome-domains (knowledge, competence, and interest), we additionally captured a variety of demographic observables ([Table 1](#)). To control for students' socio-economic status, we used the number of books at students' homes which has been shown to be a reliable predictor of parents' income levels and educational status (e.g., [Hanushek and Woessmann, 2011](#); [Schütz et al., 2008](#)). Further, we assessed school performance by asking students for their math and reading abilities on a scale from 1 (very low) to 5 (very high), with students affected by the reform showing slightly higher reading scores. Several studies have shown that self-assessed abilities are a reliable predictor of academic achievement (e.g., [Marsh et al., 2005](#); [Ackerman and Wolman, 2007](#)). Due to baseline imbalances in variables such as age, gender, reading ability, and geographic region (i.e., whether the school is located in a rural area), we control for all indicators in the subsequent regression analysis (OLS). We additionally employ propensity score matching (PSM) as a robustness check to compare two homogenous cohorts. The matched sample operates with a total of 1779 observations containing all possible values, not only the nearest-neighbors pairs ([Ho et al., 2007](#), [Appendix Table A3](#)).

## 5. Results

### 5.1. Average effects on factual economic knowledge

This section reports the main effects of the new mandate introduce mandatory economic education on the main outcomes of interest, with results of OLS regressions and Propensity score matching (PSM) shown in [Table 2](#). To account for the hierarchical data structure, we clustered standard errors at school level. The self-assessed ability variables for reading and math are mean-centered. When including several individual-level and school-level variables in the regression, multicollinearity may be a potential problem. Results on bivariate correlations and variance inflation factors (VIF) show that the variables included are widely independent of each other ([Tables A4 and A5 in Appendix A](#)).

As hypothesized, our data reveal suggestive evidence that exposure to the new curriculum improves students' knowledge scores. Regarding the entire sample, students affected by the reform score higher by 0.12 standard deviation units (column 1). While we find slightly larger, but statistically insignificant curriculum effects on economic competence (column 3), the effect on interest in economic matters is significant at a 10-percent level (column 5).

Regarding socio-demographic characteristics, we find a significant gender gap in favor of male students in both economic knowledge and competence which corresponds with most studies evaluating financial and economic education interventions in schools (e.g., [Lührmann et al., 2015](#); [Oberrauch and Seeber, 2022](#)). Next, we document lower test scores among students who spoke a foreign language during their childhood and students of low socio-economic status (i.e., having 25 or fewer than 25 books at home).

Due to sample imbalances described in [Section 4](#), we additionally employed propensity score matching (PSM) as a robustness exercise. In the matched sample with 1410 matched pairs, our results do not change qualitatively. Moreover, we ran a hierarchical regression (random intercept model) to account for the hierarchical data structure. As shown in [Table A6 in Appendix A](#), results remain qualitatively the same.

### 5.2. Distributional effects

As results in [Table 2](#) only rely on average effects of factual knowledge on factual economic knowledge, we also investigated heterogeneous effects at different moments of the conditional distribution of scores on cognitive tests (i.e., economic knowledge and competence). As shown in [Fig. 1](#), across the entire distribution, factual knowledge improved, benefiting both low- and high-achieving students displayed by the small rightward shift in the distribution, while competence scores are only affected among high-achieving students.

Additionally, we implemented simultaneous quantile regressions on the conditional distribution moments  $M$  (0.2, 0.4, 0.5, 0.6, 0.8), i.e., the 20th, 40th, 50th, 60th, and 80th percentile of all outcome domains, with results shown in [Table A7 in Appendix A](#). The methodical procedure follows the minimization problem specified in [Koenker and Bassett \(1978\)](#). Basically, the results mirror the results displayed in [Fig. 1](#), with consistent effects on knowledge scores of the order of 0.25 SDs to 0.28 SDs. Regarding competences, the new curriculum only affects test scores in the 80th percentile.

### 5.3. Heterogeneous treatment effects

This section investigates heterogeneous treatment effects across socio-demographic observables, with results shown in [Table A8 in Appendix A](#). We focus on four subgroups that are known to have different ex-ante levels regarding economic capabilities. First, strong gender differences in economic and financial capabilities among adolescents have been reported in many studies (e.g., [Bucher-Koenen](#)

<sup>2</sup> Due to school closures, we assume lower gains in domain-specific skills among treated students, especially among children of parents of low socio-economic status ([Hammerstein et al., 2021](#)).

**Table 1**  
Descriptive statistics and balance on observables.

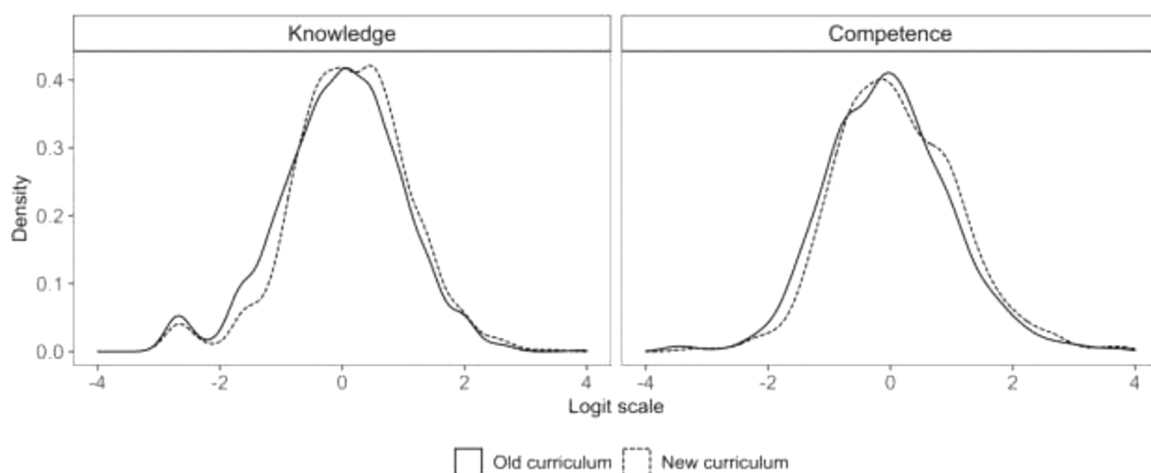
	Full sample (Mean/%) N = 1829	Control (Mean/%) N = 1102	Treatment (Mean/%) N = 727	Diff.	t-test (p-value)	N
Age	15.8	15.7	15.9	-0,2	0.000	1829
Male	51.0 %	53.2 %	47.7 %	5,5	0.031	1829
Foreign language	42.0%	42.0 %	42.0 %	0	0.993	1810
≤ 25 Books	35.4 %	35.7 %	35.0 %	0,7	0.817	1812
Reading ability	3.81	3.77	3.86	-0,09	0.077	1828
Math ability	3.37	3.35	3.38	-0,03	0.520	1825
Rural area	35.42 %	31.67 %	41.17 %	-9,5	0.312	1821

Notes: This table reports percentages and means of individual characteristics of students affected by the curriculum reform (Treatment) and those who are not affected (Control). Diff. displays the differences between the control and treatment group. P-values are based on a t-test in which the coefficient for Treatment in a linear regression on each characteristic is zero, with standard errors clustered at school-level.

**Table 2**  
Regression estimates.

Variable	Economic knowledge		Economic competence		Economic interest	
	OLS	PSM	OLS	PSM	OLS	PSM
<i>Treatment (subject)</i>	0.120 + (0.068)	0.125 + (0.069)	0.125 (0.095)	0.108 (0.095)	0.104 + (0.062)	0.111 + (0.063)
<i>Individual characteristics</i>						
Age (in years)	0.003 (0.032)	0.004 (0.040)	0.021 (0.042)	-0.016 (0.061)	0.011 (0.030)	0.062 + (0.037)
Male	0.254*** (0.055)	0.274*** (0.051)	0.167** (0.059)	0.215** (0.067)	0.090 (0.059)	0.081 (0.061)
Foreign language	-0.098* (0.048)	-0.102 + (0.053)	-0.319*** (0.061)	-0.326*** (0.068)	0.028 (0.050)	0.027 (0.053)
≤ 25 books at home	-0.279*** (0.054)	-0.276*** (0.057)	-0.282*** (0.050)	-0.265*** (0.059)	-0.223*** (0.058)	-0.176** (0.066)
Reading ability mc (1 = low; 5 = high)	0.146*** (0.035)	0.209*** (0.042)	0.160*** (0.040)	0.191*** (0.048)	0.241*** (0.033)	0.207*** (0.041)
Math ability mc (1 = low; 5 = high)	0.113*** (0.019)	0.092*** (0.024)	0.160*** (0.027)	0.172*** (0.033)	0.138*** (0.031)	0.131*** (0.031)
<i>Rural area</i>	0.166* (0.063)	0.145* (0.069)	0.165 + (0.090)	0.116 (0.098)	0.111 (0.069)	0.102 (0.068)
Constant	-0.075 (0.492)	-0.079 (0.612)	-0.217 (0.643)	0.384 (0.939)	-0.163 (0.480)	-0.994 + (0.593)
N	1792	1410	1792	1410	1779	1410
N (schools)	98	96	98	96	98	96
Adjusted R-squared	0.084	0.096	0.116	0.121	0.074	0.053
F-statistic	20.97	20.84	16.91	20.02	14.49	13.60

Notes: This table shows OLS regressions and robust standard errors clustered at school level with respect to school type. The dependent variable 'Economic knowledge' is derived from the IRT model specified in Section 2.1, with scores of the control group being standardized to have a mean of 0 and a standard deviation of 1. Control variables (individual and school-level) are defined as presented in Table 1. To emulate randomization, the chosen PSM method follows the approach of Ho et al. (2007) and uses nearest neighbor matching. +  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .



**Fig. 1. Distribution of knowledge and competence scores.** Notes: This figure shows density plots for students' economic knowledge in students affected by the new curriculum (solid line) and the control group (dashed line). Plots for students attending higher stream schools as well as for students attending lower stream schools are shown separately.

et al., 2017; Driva et al., 2016; Kaiser et al., 2021), with male students showing higher average test scores. Second, in line with previous studies (Oberrauch and Kaiser, 2020), Table 1 displayed lower test scores for participants speaking a foreign language at home. Third, we examine heterogeneous treatment effects with respect to the number of books at home. Parental background has been identified as main predictor for acquiring domain-specific capabilities in various contexts (e.g., Grohmann et al., 2015; Schütz et al., 2008).

In line with previous studies (e.g., Oberrauch and Seeber, 2022; Frischno, 2020), we find no significant interaction between the new curriculum and being male (Panel A). Also, with regard to socio-economic characteristics, the new curriculum does not particularly affect respondents growing up speaking a foreign mother tongue (Panel B) and students of low socio-economic status, i.e., having 25 or fewer than 25 books at home (Panel C). Collectively, treatment effects of the new curriculum shown in Section 5.1 appear to be universal across individual characteristics rather than affecting a particular subgroup.

## 6. Discussion

Recent studies have shown that the implementation of formal economic education in schools is effective in fostering economic competences (Oberrauch and Kaiser, 2020) and interest in economic matters (Oberrauch and Seeber, 2022) among students attending German higher-tier schools. In this paper, we add to the existing body of literature by investigating effects of the new mandate introducing mandatory economic education in lower stream schools. Previous studies have shown that children with parents of lower socio-economic status are more likely to attend lower stream school types and have lower incomes when entering the workforce. Mandatory economic education may therefore help these individuals to make informed decisions in a demanding economic environment (Lusardi et al., 2010). The study was implemented into a curriculum reform in Southwestern Germany that provides mandatory economic education for all general education schools from grade 7–10. The reform allows researchers to leverage exogenous variation in exposure to formal economic education to identify its impact on a variety of outcomes relevant to economic understanding and decision-making in economically shaped life situations.

The new mandate only has small effects on domain-specific knowledge and interest in economic topics for the entire sample of the order of 0.12 and 0.1 standard deviation units, respectively. We also observe higher competence scores among treated students. However, 90 % confidence intervals cannot rule out zero treatment effects. The effect on knowledge remains widely consistent across all distribution moments indicating that the mandate is suitable for all ability levels, while competence scores seem to improve only among high ability students. As previous research documented, large positive treatment mostly affects high-achieving students two years after implementing the new mandate (Kaiser and Oberrauch, 2021). When comparing the average treatment effects on the two cognitive outcomes with existing meta-evidence (Kaiser and Menkhoff, 2020), we find that the effect of the curriculum is below the empirical benchmarks. This holds also when we compare effect sizes with benchmarks from other domains such as mathematics or reading (Hill et al., 2008). Taken together, we conclude that the new curriculum appears to be less suited for students in lower stream schools.

However, our research design encounters two major limitations: First, as the estimates are based on a cross-sectional sample, the study is missing a baseline cohort prior to the implementation of the mandate (i.e., knowledge scores at the end of grade 6). A longitudinal setting with panel data may help researchers to identify curriculum effects in a more rigorous way. Second, as students affected by the reform were surveyed one year after the control cohort, exogenous economic or political events may confound our identification strategy. Especially school closures and distant learning during the 1st wave of the COVID-19 pandemic may be associated with losses in learning time and cognitive skills (Grewenig et al., 2021). Since the treatment group was surveyed in 2020, the average treatment effects shown in Section 5.2 are potentially downward-biased.

Finally, in line with previous research, our data reveal meaningful variation in economic capabilities across individual observables. Students of lower socio-economic status or migration background score significantly lower in the knowledge and competence test, with larger (negative) effects on economic competences than on knowledge. This indicates that adapting content-oriented knowledge is easier for disadvantaged students than exhibiting broader operational and judging competences. Further, our results indicate that the gender gap remains a worrying phenomenon, as we find no significant interaction between being affected by the mandate and being male. As shown by the analysis of heterogeneous treatment effects in Section 5.3, treatment effects are not driven by these disadvantaged subgroups. Therefore, future research needs to i) identify relevant factors driving these performance gaps with regard to school types and individual characteristics and ii) investigate the impact of new teaching formats and materials that may have the potential to equalize the disparities.

### CRedit authorship contribution statement

**Mira Eberle:** Software, Methodology, formal analysis, Validation, Conceptualization, Data curation, Writing – original draft, Writing – review & editing. **Luis Oberrauch:** Investigation, Data curation, Writing – original draft, Writing – review & editing, Formal analysis.

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## Declarations of interest

None.

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## Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at [doi:10.1016/j.iree.2022.100259](https://doi.org/10.1016/j.iree.2022.100259).

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