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# **Economic education at the expense of indoctrination? Evidence from Germany**

Tim Kaiser and Luis Oberrauch

## **Abstract**

We study the impact of a recent curriculum reform introducing mandatory economic education in higher-track secondary schools in Southwest Germany. The curriculum reform provides the opportunity to leverage the exogenous variation in exposure to economic education relative to the previous cohort not affected by the reform. One year after exposure to the mandate, we observe positive treatment effects on test scores measuring cognitive elements of economic competence only for students with high test scores at baseline. Two years after exposure to the mandate, we find positive treatment effects on test scores across the entire distribution, as well as socio-emotional skills relevant to financial decision making while we do not observe effects on self-reported financial behaviors. At the same time, we find no changes in social preferences and normative attitudes that could give rise to concerns of indoctrination effects regarding students' views on profit maximization and the market mechanism.

JEL Codes: A21, G53, I21

Keywords: Economic education, financial literacy, impact evaluation, social preferences, financial behaviors

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# **Economic education at the expense of indoctrination?**

## **Evidence from Germany**

### **1 Introduction**

Globalization and changing economic landscapes have elevated economic and financial education onto the policy agenda of governments in both emerging and advanced economies (see OECD 2015, Lusardi et al. 2010, Lusardi and Mitchell 2014). Previous impact evaluations of financial education interventions in schools have shown that the implemented curricula are, on average, effective in fostering financial knowledge, and, to some extent individual financial decision-making (Batty et al. 2015, 2020, Bruhn et al. 2016, Bover et al. 2018, Brown et al. 2016, Frisanchi 2018, 2020, Kaiser and Menkhoff 2020, Kaiser et al. 2021, Lührmann et al. 2015, 2018, Walstad et al. 2010, Urban et al. 2020). While this body of evidence covers financial education interventions and mandates, i.e., a subset of general economic education, the literature on the effectiveness of broader economic education in schools is more limited (see Rhine 1989, Walstad 2001, Grimes et al. 2010, Gill and Gratton-Lavoie 2011, Walstad 2013, Walstad and Watts 2015 for work on U.S. high school curriculum mandates). Part of the limitations of the literature on formal high school economic education in the U.S. stems from the fact that economics is implemented heterogeneously across and within federal states (for example in an integrative approach within social studies curricula, as an elective or as a mandatory stand-alone course with a graduation requirement), making it sometimes difficult to clearly distinguish between selection and treatment effects and to link exposure to high school economic education to student achievement in nationally representative assessments or external outcomes such as financial behaviors.

In the context of the German educational system, economic education has long been limited in the past, with most federal states following a similar approach to many federal states in the U.S. by integrating economic aspects in curricula of adjacent school domains (i.e., akin to social studies curricula in the U.S.) or by offering economics as an elective course (see

Oberrauch and Kaiser 2020, Kaiser et al. 2020). Recently, the German federal state of Baden-Wuerttemberg passed a curriculum reform introducing mandatory economic education as a standalone school subject in all general education schools, providing the opportunity to study the effects of mandatory economic education within the setting of a natural experiment.

Against this backdrop, this paper studies the impact of economic education in German (higher track) secondary schools in the context of the curriculum reform requiring schools to offer economic education as a mandatory school subject. We can rely on a large sample of 3,097 students in 92 schools and use the quasi-experimental variation of exposure to the mandate between cohorts to arrive at estimates of the impact on cognitive elements of economic competence, (antecedents of) financial behaviors, individual and social preferences, as well as normative attitudes. The identification strategy rests on the assumptions of parallel trends, students not self-selecting into the cohort by voluntarily leaving their original cohort, and policymakers not targeting this cohort specifically based on static quality or a forecast of cohort outcomes. We address these threats to identification in the later section 3.1 and are able to show pre-treatment balance on observables as well as additional details about the process of policy implementation that render targeting based on a forecast of outcomes for a specific cohort unlikely.

Our main results are clear-cut: One year after exposure to the mandate, test scores increase only for high performing students (i.e., those with high test scores at baseline). Two years after exposure to the mandate, we find positive average effects on test scores across the entire distribution, as well as positive effects on (self-reported) preferences and socio-emotional skills relevant to financial decision making but no effects on (self-reported) financial behaviors. At the same time, we find no changes in social preferences or normative attitudes regarding students' views on profit maximization and the market mechanism.

With these results, we contribute to the literature on school based economic education in three ways: First, we present evidence of the impacts of mandatory economic education in



Germany, a country where the implementation and intensity of economic education has been limited in the past. While the mandate appears to increase existing achievement gaps in the short term (i.e., after one year of instruction), the longer-term effects (after two years of instruction) appear to result in an increase in test scores across the entire distribution. We also observe positive effects on students self-reported interest in economic matters after one year, but these effects appear to wear off two years after the initial exposure to the mandate.

Second, the observed effects on antecedents of financial behaviors, i.e., (self-reported) time and risk preferences, provide corroborative evidence to the findings of the literature on treatment effects of educational interventions for children and youth (Alan and Ertac 2018; Bover et al. 2018; Lührmann et al. 2018, Frisanco 2018, 2020, Sutter et al. 2019, Horn et al. 2020). However, realized treatment effects on self-reported financial behaviors as well as the financial autonomy scale (Bruhn et al. 2016, Frisanco 2018, 2020) appear to be much smaller than what is found in interventions focusing more narrowly on personal finance (see Kaiser and Menkhoff 2020 for a meta-analysis of school-based interventions). This suggests more targeted financial education may be an important complement to general economic education when the goal is to foster individual financial decision-making of youth (Lusardi and Mitchell 2010).

Third, we make a novel contribution to the literature on potential indoctrination effects of educational exposure to ideas and models usually taught in mainstream economics, especially neoclassical assumptions about individual decision-making, Pareto efficiency and their implications for government intervention (see Marwell and Ames 1981, Carter and Irons 1991, Frank et al. 1993, 1996, Frey et al. 1993, Yezer et al. 1996, List et al. 2001, Frey and Meier 2003, Rubinstein 2006, Cipriani et al. 2009, Haucap and Just 2010, Bauman and Rose 2011, Ifcher and Zarghamee 2018 Chen et al. 2021). While this literature generally provides evidence for individuals trained in economics behaving differently from untrained individuals in experimental settings (such as cooperative or competitive games) as well as evidence for differences in normative attitudes and policy preferences, the question of causality (i.e., whether

economic education causes these differences in behaviors and normative attitudes) has not been convincingly addressed in many empirical applications. Most studies rely on variation in exposure to university-level economics coursework and thus cannot easily distinguish between selection and treatment effects: An entire strand of this literature aims to address this question by discussing evidence for these two competing hypotheses, i.e., the “self-selection hypothesis” (students with ex ante different behaviors, preferences, and normative attitudes self-selecting into economics coursework) vs. the “indoctrination hypothesis” (economics coursework having a treatment effect on students normative attitudes and behaviors). As our setting effectively rules out the possibility of students self-selecting into the curriculum (due to its mandatory nature and exogenous implementation date), we provide the first direct test of the indoctrination hypothesis in a secondary school setting. Our results provide no evidence for such an (short-term) effect. Our results are in line with recent evidence relying on quasi-experimental evidence from China showing that university-level economics coursework does not affect social preferences but may affect individual risk taking in incentivized tasks (Chen et al. 2021).

This article is structured into five further sections: Section 2 provides additional information about the setting and the curriculum reform. Section 3 describes the research design and random sampling of schools and students. Section 4 presents descriptive statistics and discusses balance between treated and control students regarding observable characteristics at baseline. Section 5 presents our main results on economic competence, financial behaviors, individual and social preferences as well as normative attitudes. Section 6 discusses these results and concludes.

## **2 Background**

In the past, economic and financial education mandates in Germany have been limited with most federal states implementing economic aspects in curricula of adjacent school domains (i.e., akin to social studies curricula in the U.S.). Consequently, Germany did not take part in

the *Program for International Student Assessment* (PISA) financial literacy assessments (OECD 2014) and nationally representative data on economic and financial literacy among youth is lacking. Recently, the federal state of Baden-Wuerttemberg passed a curriculum reform introducing mandatory economic education as a discrete school subject in all general education schools beginning with the 7<sup>th</sup> grade cohort of 2017/2018 (Ministerium für Kultus, Jugend und Sport Baden-Wuerttemberg 2016). This exogenous policy change provides the opportunity to study the effect of introducing mandatory economic education in school relative to a situation where economic aspects were limited to fewer contact hours within an integrative school subject.

< Table 1 about here >

Table 1 provides a comparison of the main differences between the two curricula. While both curricula are designed without economic coursework in 7<sup>th</sup> grade (providing the opportunity to assess baseline balance), important details in the later grades (8 and 9) exist: In the pre-reform curriculum, economic education is limited to grade 8 and is integrated into the combined subject course “Geography, Economics, and Social Studies”. Thus, economics is part of the social studies curriculum and taught by social studies teachers. The intensity amounts to approximately one lesson (45 min) per week per academic school year but may vary depending on the mode of implementation in schools.<sup>1</sup> Pre-reform, there is no economic education in grade 9.

In contrast, post-reform cohorts are not taught in a combined subject course but in a standalone subject entitled “Economics and Vocational/Academic Orientation”. It is offered in grades 8 and 9 and contents are aligned with competence goals described in a conceptual model

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<sup>1</sup> The combined subject course “Geography, Economics, and Social Studies” is ought to be offered at the extent of 14 lessons (per week) over the course of three school years (grades 6, 8, and 10). Thus, the average amount of weekly lessons is about 4 to 5 for the combined subject course (covering three subjects). Since economics is part of the social studies curriculum in grade 8 (about 2 weekly lessons), assuming that a maximum of half of the contact hours are spent on teaching economics is realistic (i.e., max. 45 min per week). Note, that schools and teachers have the discretion to also implement the curriculum as two lessons per week (90 min) per term (i.e., half of the school year) (see Ministerium für Kultus, Jugend und Sport Baden-Wuerttemberg. (2004)).

of economic competence underlying the curriculum (see Retzmann and Seeber 2016). While the official syllabus provided by the ministry of education does not encompass a breakdown of contents by class-level, curricula used in teacher training suggest focusing on different life situations, i.e., (a) individuals as consumers of goods and services (8<sup>th</sup> grade), (b) individuals as wage-laborer or self-entrepreneurs (9<sup>th</sup> grade), and (c) individuals as tax-paying and voting citizens (10<sup>th</sup> grade). In addition to the differences in content, the reform effectively doubles the intensity of instruction from about twenty hours to about 40 hours in the considered grades.

### **3 Research design**

#### **3.1 Quasi-experimental design**

We utilize the introduction of the new curriculum beginning with the cohort of 2017 and compare the performance relative to the previous cohort (of 2016) in assessments relying on representative cross-sections. A causal interpretation of the curriculum effect on the observed between cohort differences relies on the following exogeneity assumptions:

First, students in the two cohorts must not differ systematically on observable or unobservable traits, and student outcomes of both cohorts should have evolved in parallel in the absence of the new curriculum. While it is impossible to test for pre-treatment trends in our setting, we can compare the two cohorts regarding their balance on observables and main outcomes at baseline (i.e., at the end of grade 7) (see section 4).

Second, the implementation date of the curriculum reform must not be a function of between cohort differences in static quality or a forecast of cohort outcomes. While we can probe pre-exposure differences in static quality of cohort outcomes in our setting (see section 4), the latter endogeneity concern may be alleviated by the multi-year bureaucratic process starting in 2012 which involved piloting with 100 schools, revision of curricula, as well as involvement of teachers, parents, students, unions, business representatives, and academic researchers via a dedicated participation process before the new curriculum was being passed

and implement.<sup>2</sup> This lengthy implementation process implies a low likelihood of policymakers explicitly targeting cohorts which are one year apart based on a forecast of different cohort outcomes. Thus, the effective implementation date may indeed be considered exogenous.

Third, students must not self-select into the new curriculum by voluntarily leaving their original cohort and instead joining the cohort affected by the new curriculum mandate. While this is possible in theory, this is very unlikely in practice, since the school subject is a minor subject of relatively little importance, and, thus, unlikely to influence the hypothetical decision to repeat an entire school year. Additionally, observing this mechanism at scale should result in a baseline imbalance in student age between the two cross sections and should possibly provide evidence of duplicate student survey submissions between the cohorts. Our data does not provide evidence to allow such conclusions, suggesting our comparison may be considered as internally valid.

### **3.2 Sampling and procedures**

The sampling frame consists of students in the high-tier school track (about 40 percent of all students) in the German federal state Baden-Wuerttemberg. We conduct a two-stage sampling procedure (stratified by degree of urbanization) with random selection of schools in the first stage and a random selection of one class per school at baseline (i.e., in 7<sup>th</sup> grade) for each cohort. The number of sampled schools is adapted to the proportion of the relevant stratum in the whole population (i.e., the sampling probability is proportional to the size of the strata) (see also Kaiser et al. 2020, Oberrauch and Kaiser 2020) and we account for any remaining sample disproportionalities by including design weights defined as the inverse of the selection probability in our analyses. The survey was subsequently administered as a computer-based assessment during the regular school lessons and carefully supervised by the respective teachers.

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<sup>2</sup> See: <https://beteiligungsportal.baden-wuerttemberg.de/de/informieren/projekte-und-berichte/lp-15/bildungsplanreform-2016/>, last checked: October, 28<sup>th</sup> 2021.

### 3.3 Measurement of student outcomes

*Economic competence.* To measure economic competence, we employ the *Test of Economic Competence* (TEC) (Kaiser et al. 2020). The TEC is a performance test based on a competence model underlying economic education standards in Germany and the economic education curriculum in the federal state our study is set in (Retzmann and Seeber 2016). The scale was developed for secondary school students and its 31 items show valid psychometric characteristics (see detailed information about test characteristics in Kaiser et al. 2020). We analyze economic competence using an Item Response Theory model (IRT) (Baker and Kim 2017) frequently employed in international large scale assessments. Specifically, to estimate item characteristics and person abilities, we employ a 2-parameter logistic Model (Birnbaum 1968).<sup>3</sup> Appendix Table B1 shows item characteristics from the IRT model as well as descriptive statistics from Classical Test Theory (CTT). Overall, the administered test items show sufficient discrimination and evenly distributed difficulties. As estimated individual ability scores are generally subject to measurement error<sup>4</sup>, we implement a multiple imputation approach for the error term correction based on a latent regression model (a procedure termed “Plausible values”) (Marsman et al. 2016) which has been well established in international large-scale assessments (see Oberrauch and Kaiser 2020 for more details).

One challenge when examining and comparing test scores from different test administrations or respondent groups (e.g., cohorts) is to bring them onto a common metric (i.e., test equating). Equating methods in the context of item response theory (IRT) aim to obtain

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<sup>3</sup> Formally, test scores are estimated within the following IRT-framework:  $P(X_i = 1|\theta, \sigma, a_i) = \frac{\exp\{a_i(\theta_v - \sigma_i)\}}{1 + \exp\{a_i(\theta_v - \sigma_i)\}}$ , where  $\theta_v$  denotes estimated ability for person  $v$  and  $\sigma_i$  estimated item difficulty for item  $i$  on a common logit scale.  $a_i$  defines a discrimination parameter evaluating how accurate item  $i$  discriminates between low-ability and high-ability students.

<sup>4</sup> I.e.,  $\hat{\theta} = \theta_v + \varepsilon$ , potentially leading to biased regression estimates.

a common scale for test scores by linking item parameters from different test administrations (IRT linking). So far, several linking methods have been developed under the IRT model family (for a comparison see Lee and Ban 2010). Simulation studies have shown the superiority of characteristic curve methods (Kolen and Brennan 2014) which is why we rely on the approach described in Haebara (1980) that minimizes the (quadratic) deviations of probabilities for solving an item correctly between original item parameter and transformed parameter. Test scores are scaled to have a mean of zero and a standard deviation of one for the control group at baseline (i.e., at the end of seventh grade).

As our competence assessment relies on low-stake-testing, we additionally control for students' effort using the well-established time-response-effort measure described in Wise and Kong (2005) which takes the respondents' response times for each item into account. Specifically, the approach defines a time threshold that separates rapid guessing from solution behavior. Following Wise and Ma (2012) we use a normative time threshold of ten percent of the average (question-specific) student response time. The final *effort score* for each respondent represents the relative share of solution behavior shown across all items.

*Interest in economic matters.* In addition to performance on the test (capturing cognitive aspects of economic competence), we aim to capture a non-cognitive aspect of economic competence (i.e., interest and motivation) through a simple rating scale measuring interest in economic matters. The item asks for interest in economic matters on a scale from 1 (not interested at all) to 5 (very interested) and is standardized to have a mean of zero and a standard deviation of one.

*Financial autonomy scale and behaviors.* To study changes in (antecedents of) financial behaviors, we employ the “financial autonomy scale” detailed in Bruhn et al. (2016) and Frisanco (2018, 2020) (see Appendix C for individual items). The scale measures antecedents of financial behavior and the index and individual index components are scaled to have a mean of zero and a standard deviation of one. We also ask for self-reported savings and loans.

*Individual and social preferences.* We assess risk and time-preferences as well as social preferences such as altruism by relying on self-assessments based on survey items (see Dohmen et al. 2010 and Falk et al. 2016 for details about the items). We provide additional information in the discussion of the results and items are presented in the Appendix.

*Normative attitudes.* Finally, we look at impacts of the curriculum reform on normative attitudes. Since the setting did not allow incentivized experiments, we implemented several survey items used in the previous literature on the indoctrination hypothesis (see Frey et al. 1993, Cipriani et al. 2009, Rubinstein 2006, Haucap and Just 2010). Details about the single items are discussed in section 4 and items are presented in full in the Appendix C.

### 3.4 Empirical models

To estimate the effects of the curriculum mandate, we estimate a random-intercept model accommodating the clustered data structure. We compare the change in outcomes between the baseline (end of 7<sup>th</sup> grade) and the follow-up surveys (end of 8<sup>th</sup> and end of 9<sup>th</sup> grade) between the treatment and control group using repeated cross-sections. The model takes the form

$$y_{is} = \gamma_{00} + \beta_1 T_i + \sum_k \beta_k grade_{ik} + \sum_{j,k} \beta_j (grade_{ik} \times T_i) + \sum_m \gamma_m X_{mis} + u_{0,s} + \varepsilon_{is}, \quad (1)$$

where  $y_{is}$  denotes outcome dimensions of individual  $i$  in school  $s$ .  $\gamma_{00}$  denotes the overall mean value and  $T_i$  is a treatment dummy which takes the value 1 if student  $i$  is affected by the curriculum mandate and 0 otherwise, to display differences in the baseline cohorts (7<sup>th</sup> grade).  $grade_{ik}$  indicates whether student  $i$  is in the 8<sup>th</sup> grade (i.e., received one year of instruction) or the 9<sup>th</sup> grade (received two years of instruction).

Since only test scores on economic competence and interest in economic matters are available as outcomes for each year, we study the impact of the curriculum reform on financial behaviors, individual and social preferences as well as normative attitudes in a cross-sectional



sample (data on these outcomes was collected exclusively at the end of 8<sup>th</sup> grade). Using the cross-sectional sample, we estimate

$$y_{is} = \gamma_{00} + \beta_1 T_i + \sum_m \gamma_m X_{mis} + u_{0,s} + \varepsilon_{is}, (2)$$

where  $y_{is}$  denotes outcomes on savings and debt behaviors, scores on the financial autonomy index, individual and social preferences as well as attitudes towards profit maximization and allocation mechanisms of individual  $i$  in school  $s$ . We adjust for individual and school-level characteristics in the covariate vector  $X_{mis}$  in both models (1) and (2).  $u_{0,s}$  represents the school-dependent variation in the random intercepts model. Standard errors are clustered at the classroom level. For item non-response in covariate variables, such as mother tongue or parental educational background, we implement 20 multiple imputations by chained equations (MICE, van Buuren and Groothuis-Oudshoorn 2011) that allow conducting the imputation process iteratively for all variables affected by item non-response.

#### 4 Descriptive statistics and baseline balance

Our sample consists of 3,097 students (1,594 affected and 1,503 not affected by the reform) from 157 classes in 92 schools (see Figure 1).

< Figure 1 about here >

The spatial distribution of sampled schools is proportional to the population density in the federal state of Baden-Wuerttemberg, indicating our stratified random sampling of schools was successful.

< Table 2 about here >

Table 2 shows individual characteristics regarding the distinct cohorts. Tests for mean differences at baseline indicate that demographic characteristics on individual and school levels

are balanced across treated and control students.<sup>5</sup> The mean *age* is 14.55, and 48.4 percent of the whole sample are male. Regarding socio-economic characteristics, a share of 32 percent has a *migration background*, i.e., at least one of the students' parents being born abroad. 12.3 percent of all respondents reported to have 25 or less than 25 *books at home* (magazines and textbooks excluded). We additionally ask for students' self-reported *reading* and *math abilities* on a scale from 1 (low) to 5 (high)<sup>6</sup> and ask whether students have current income from student jobs (*own salary*). Additionally, most respondents exhibit high *effort* on the test and there is no significant difference between the cohort affected by the new curriculum and the control cohort.

Regarding school-level characteristics, we control for *school size*, i.e., the number of students per school, and *degree of urbanization* as defined above. All the school-level variables are balanced between groups as well.

## 5 Results

This section reports on the main effect of mandatory economic education on cognitive and non-cognitive outcomes. As described in section 3, we study the effects on economic competence, interest in economic matters, financial autonomy and behaviors, and changes in normative attitudes.

### 5.1 Economic competence and interest in economic matters

Table 3 reports determinants of economic competences (column 1 and 2) and interest in economic matters (column 3 and 4) following the specification described in section 4.3.

< Table 3 about here >

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<sup>5</sup> We also probe balance on observables at baseline in the cross-section of students used for the assessment of outcomes only collected at the end of 8<sup>th</sup> grade. Testing for joint orthogonality using the cross-sectional sample results in low predictive power and a p-value of 0.27, thus confirming balance in this sample, as well.

<sup>6</sup> We probe the validity of these self-reported variables by running a linear regression on these two outcomes with gender as predictor. The results are in line with previous studies, with male respondents being more proficient in math and female respondents being more proficient in reading.

*Average effects on economic competence.* First, we probe baseline balance of the two main outcomes. The “new curriculum” dummy indicates balanced groups at the end of 7<sup>th</sup> grade (i.e., before instruction began) for both outcomes presented in Table 3. Next, while we do not find a significant difference in economic competence after the first year, i.e., at the end of grade 8, we see a large effect (0.45 SD) of the mandate two years after its implementation. The estimated effect is very similar when controlling for various student and school-level controls (column 2). Regarding these socio-demographic predictors (see Appendix A), we observe a substantial gender gap already at these young ages (0.23 SD) which is in line with a previous study on financial knowledge in Germany (Driva et al. 2016). Note that the gender gap increased slightly after adjusting for time-response-effort that is typically higher among female respondents (Balart and Oosterveen 2019). In line with previous studies in Germany (e.g., Oberrauch and Kaiser 2020, Kaiser et al. 2020, Lührmann et al. 2015), we find economic competence to be, on average, significantly lower among children of migrants and children of parents with lower socio-economic status (SES). Despite these achievement gaps, we find no evidence of heterogenous treatment effects depending on these observables (see Appendix A).

*Average effects on interest in economic matters.* Next, regarding treatment effects on the interest dimension, our data reveal a substantial effect after one year of instruction, but no effect after the second year. This result indicates that interest may serve as a prerequisite to acquiring economic knowledge (Oberrauch and Seeber 2021). Analogous to the effects on competence, we find a substantial gender gap in economic interest that has already been documented in previous studies (e.g., Driva et al. 2016; Lührmann et al. 2015). Yet again, no heterogenous treatment effects by gender (or other observable characteristics) exist.

*Distributional effects.* As results in Table 3 only reflect average effects of the new curriculum on economic competences, we are also probe the existence of heterogenous effects on different moments of the conditional distribution of economic competences.

< Figure 2 about here >

As shown in Figure 2, one year after treatment, economic competences improved only among high-achieving students. Two years after treatment, however, test scores improved across the entire distribution benefiting both low- and high-achieving students displayed by the rightward shift in the distribution of economic competences for treated students. In addition, we implement simultaneous quantile regression (Koenker and Bassett 1978) on the conditional distribution moments  $M(0.2, 0.4, 0.5, 0.6, 0.8)$ , i.e., the 20<sup>th</sup>, 40<sup>th</sup>, 50<sup>th</sup>, 60<sup>th</sup>, and 80<sup>th</sup> percentile, with results shown in Table A3 in Appendix A.

Results from quantile regressions reveal that effects of the curriculum mandate are heterogeneous one year after its implementation. While there are no effects in lower percentiles of the distribution, we find large and significant effects above the median., with 0.32 SD in the 60<sup>th</sup> and 0.53 SD in the 80<sup>th</sup> percentile. By contrast, the effects of the curriculum two years after implementation appears significant across the entire distribution, with average curriculum effects being especially driven by relatively large effects in the 40<sup>th</sup>, 50<sup>th</sup> and 60<sup>th</sup> percentile. Collectively, these results indicate that the curriculum mandate is suited for students of all proficiency levels two years after its implementation rather than widening existing achievement gaps as indicated by the shorter-term results one year after implementation.

## **5.2 Financial autonomy and financial behaviors**

In this section, we turn to examining short-term changes in financial autonomy and behaviors using the sample of 8<sup>th</sup> graders. Individual characteristics in this subsample are balanced at baseline across curriculum and control group (see Table A4 in Appendix A).

*Financial autonomy.* We study the dimensions of the financial autonomy index used in Bruhn et al. (2016) and developed in Micarello et al. (2012) measuring whether teenagers are capable to draft and follow a budget (Financial planning), exert influence on decisions of their

households (Emotional), make reasonable financial decisions (Functional) and whether they think carefully before making a purchase (Reflexive) (see Appendix C for the items).

< Table 4 about here >

As shown in Table 4 the new curriculum does not appear to affect three of four index dimensions. Regarding the reflexive dimension, however, students affected by the reform show a difference of 0.16 SD relative to the control group (statistically significant at the ten-percent level) indicating that the curriculum may have the potential to foster students' ability to make reasonable and thoughtful financial decisions.

*Financial behaviors.* We consider potential differences in savings and debt behavior by asking students whether they possess any savings or debt as well how much they saved or borrowed, with results shown in Table 5

< Table 5 about here >

In contrast to previous studies on financial education interventions, we don't observe any significant impact of the curriculum mandate on savings and debt. Note that the curriculum content covers a broad range of economic topics (see section 2), with relatively little coverage of consumer-related topics that are typically implemented in financial education interventions. Regarding heterogeneity among subgroups, our data indicate that boys, students with a migration background, and students with parents of lower socio-economic status are less likely to save and possess smaller amounts of savings (see Appendix B for effects across observables). Yet, our data provides no evidence of heterogeneous treatment effects of the new curriculum for those students.

### **5.3 Individual and social preferences**

Next, we investigate four students' key economic preferences - risk and time preferences, trust, and altruism using non-experimental and self-reported measures.

< Table 6 about here >

*Risk preferences.* We ask respondents to evaluate on a scale from 1 to 10 whether they are risk-seekers or generally try to avoid risks (Dohmen et al. 2010). Dohmen et al. (2010) identified these self-reported risk preferences as a meaningful predictor for risky behaviors as well as for risk-taking in incentivized experiments. The results in Table 6 of columns 1 and 2 show risk-seeking to be significantly lower among the treatment group by 0.2 SD.

*Time preferences.* Columns 3 and 4 report determinants of students' intertemporal preferences by asking to what extent they are capable of delaying gratification to gain utility at a later point of time on a scale from 1 to 4. As with measuring risk preferences, survey instruments for measuring discounting have been shown to be a reliable predictor of preferences elicited in incentivized decision experiments (e.g., Falk et al. 2016). In line with previous literature (e.g., Alan and Ertac 2018; Frisanchi 2018, 2020), we observe a meaningful treatment effect on the self-reported patience measure suggesting that education may change students' perspective on future utility.

*Trust.* To measure trust, we implement a simple binary item used in the German Socio-Economic Panel (SOEP) and validated in Falk et al. (2016). The item asks students whether they generally trust in others or whether they must be careful. We observe no treatment effect on this measure of trust (Columns 5 and 6).

*Altruism.* To investigate social preferences, i.e., altruism, we also follow the simple question used in the SOEP. In essence, students receive hypothetical 1000 Euros and are subsequently asked whether they would share a larger amount on a scale from 1 (not at all) to 4 (definitely). Our data reveal no significant impact of the curriculum mandate on trust and on altruistic behavior. With respect to heterogeneity across observables, we discover girls to be significantly more altruistic than men, but, again, find no evidence for heterogeneous treatment effects (see Appendix A).

## 5.4 Normative attitudes

Next, we study potential effects on normative attitudes related to questions of profit maximization and the market mechanism. For this purpose, we administer five well-established items from previous studies in this field (Cipriani et al. 2009, Frey et al. 1993, Rubinstein 2006, Haucap and Just 2010) with all items shown in Appendix C (Figure C1). Specifically, we asked students for a judgment about the fairness of a price raise following an exogenous shock leading to excess demand. The item (henceforth coded as SHOVEL) reads as follows:

*“A hardware store has been selling snow shovels for 15 Euro. The morning after a large snowstorm, the store raises the price to 20 Euro. Please rate this action as: (a) Very unfair, (b) Unfair, (c) Acceptable, (d) Very fair”.*

In the second item students were asked to judge on the fairness of different allocation mechanisms (auction, lottery, queue). Cipriani et al. (2009) classify this question as the most neutral one as the item SHOVEL requires a trade-off of between the market equilibrium and fairness considerations. The item (henceforth coded as AUCTION) reads as follows:

*“A football team normally sells some tickets on the day of their games. Recently, interest in the next game has increased greatly, and tickets are in great demand. The team owners can distribute the tickets in one of three ways. (1) By auction: the tickets are sold to the highest bidders. (2) By lottery: the tickets are sold to the people whose name are drawn. (3) By queue: the tickets are sold on a first-come first-served basis. Rank these three in terms of which you feel is the most fair and which is the least fair – the auction, the lottery, and the queue.”*

The next item (Rubinstein 2006; henceforth coded as PROFMAX) confronts participants with a dilemma between profit maximization and employee layoffs and reads as follows:

*Assume that you are a vice president of ILJK company. The company provides extermination services and employs a certain number of permanent administrative workers and 196 nonpermanent workers who are sent out on extermination jobs. The company was founded five years ago and is owned by three families. The work requires only a low level of skill, with each worker requiring only one week of training. All the company's employees have been with the company for three to five years. The company pays its workers more than minimum wage. A worker's salary includes payment for overtime, which varies from 1200 to 1500€ per month (suppose that the minimum wage is about 1000€ per month). The company makes sure to provide its employees with all the benefits required by law. Until recently, the company was making large profits. As a result of the continuing recession, there has been a significant drop in profits, although the company is still in the black. You will be attending a meeting of the management in which a decision will be made regarding the layoff of some of the workers. ILJK's finance department has prepared scenarios of annual profits shown in the following.*

<i>Number of workers who will continue to be employed</i>	<i>Expected annual profit (millions of Euro)</i>
<i>0 (all the workers to be laid off)</i>	<i>Loss of 8</i>
<i>50 (146 workers to be laid off)</i>	<i>Profit of 1</i>
<i>65 (131 workers to be laid off)</i>	<i>Profit of 1.5</i>
<i>100 (96 workers to be laid off)</i>	<i>Profit of 2</i>

144 (52 workers to be laid off)	Profit of 1.6
170 (26 workers to be laid off)	Profit of 1
196 (0 workers to be laid off)	Profit of 0.4

---

*Complete the following: I recommend continuing to employ . . . of the 196 workers presently employed by the company.*

In contrast to the first item (AUCTION), this item investigates whether students prioritize profit maximization, i.e., choose a profit of two million Euro and simultaneously dismiss 100 workers, or take social considerations into account. The fourth question (henceforth coded as PROFMAX-VP) investigates students' opinion on the behavior of a real vice president in this hypothetical situation:

*"What do you think would be the choice of a real vice president in Question 3? I think that he would recommend continuing to employ. . . of the 196 workers in the company."*

< Table 7 about here >

Probit regression results on these four questions are reported in Table 7. We code responses on the item SHOVEL as 1 if students assess the price raise at least as "fair" or "completely fair", 0 otherwise. In the second item (AUCTION) we code responses as 0 if students prefer the auction over the other allocation mechanisms. In the third (PROFMAX) and fourth (PROFMAX-VP) question, we code responses as 1 if students choose the solution that maximizes profit, i.e., 2 million Euro of profit and 100 layoffs, and 0 otherwise. Our data reveal no significant impact of the curriculum reform on the decisions in all four settings indicating that the mandate does not alter students' normative attitudes. Regarding heterogeneity along observables (see Appendix A for details), we observe a significant gender gap in the second setting (AUCTION) with male participants choosing auction as the most favorable allocation mechanism.

Finally, we further investigate potential indoctrination effects using the survey item administered to adults in Haucap and Just (2010) and originally developed in Frey et al. (1993). The item asks respondents to evaluate the fairness of various allocation mechanisms for a scarce recourse from 1 (not fair at all) to 4 (totally fair) and reads as follows:



*At a sight-seeing point, reachable only by foot, a well has been tapped. The bottled water is sold to thirsty hikers for two Euro per bottle. The maximum daily production are 100 bottles. On a particularly hot day, 200 thirsty hikers are expected. Please judge the following measures for allocating the water among the thirsty hikers: (a) The price is increased to four Euro per bottle., (b) Selling the water for two Euro per bottle to the first 100 hikers according to ‘first come, first served’. (c) Selling the water for two Euro per bottle to the 100 hikers whose last name by chance happens to start with the letters A to K. (d) The local community buys all bottles for two Euro per bottle and distributes them as it sees fit. (e) Selling half-sized bottles for one Euro per bottle to all hikers (one bottle per hiker only).*

We run probit regressions for all five allocation mechanisms by coding responses as 1 that are at least “fair” or “totally fair”, 0 otherwise, with results shown in Table 8.

< Table 8 about here >

Regression estimates indicate that there is no indoctrination effect as there are no significant differences in fairness perceptions between the curriculum cohort and the control group across all allocation mechanisms. Furthermore, we find that boys are more likely to prefer the price raise by six percentage points as well as more likely to prefer the greyhound and the random procedure by nine percentage points. Migrants and students with lower socio-economic status are both less likely to consider the rationing procedure as fair by six percentage points (see Appendix A).

## 6 Discussion

Our results suggest the curriculum reform is highly effective in fostering economic competence among youth. Instead of widening existing achievement gaps, we find positive treatment effects along the entire competence distribution, thereby potentially enabling students of all ability levels to make more thoughtful economic decisions and exercise agency in settings and situations requiring cognitive components of economic competence. While the treatment effects on non-cognitive aspects of economic competence (e.g., interest in economic matters and financial autonomy) appear to be more muted and potentially short-lived, the effects on (self-reported) risk and time preferences are in line with previous literature on financial education interventions (e.g., Bover et al. 2018; Frisancho 2018, 2020, Horn et al. 2020).

However, the small treatment effects on financial behaviors suggest that more targeted financial education can complement general economic education in fostering financial decision-making capabilities of youth.

In a second step, we have shown that the economic education mandate did not affect social preferences or normative attitudes. These results are contributions to the literature on potential indoctrination effects of economic education (e.g., Bauman and Rose 2011, Cipriani et al. 2009, Frey et al. 1993, Haucap and Just 2010, Ifcher and Zarghamee 2018 Rubinstein 2006). Our setting rules out self-selection of certain students into the economics coursework, thereby enabling a direct test of the indoctrination hypothesis in a secondary school setting. We do not find treatment effects on any of our considered outcomes, suggesting that the increase in test scores did not come at the expense of indoctrinating students towards the neoclassical paradigm. While the contents covered include exposure to ideas and models usually taught in mainstream economics (i.e., neoclassical assumptions about individual decision-making, Pareto efficiency and their implications for government intervention) the curriculum covers a broad range of perspectives including social and systemic considerations which may differ from contents covered in university economics teaching. Yet, our results correspond to findings by Chen et al. (2021) who also do not find evidence for treatment effects on social preferences in higher education economics courses in China. Thus, it appears that the large gains in economic competence did not come at the expense of a one-sided indoctrination of students. An important area of future research may be to follow up with the cohorts over longer time periods and to couple the hypothetical choices with incentivized experiments in school settings.

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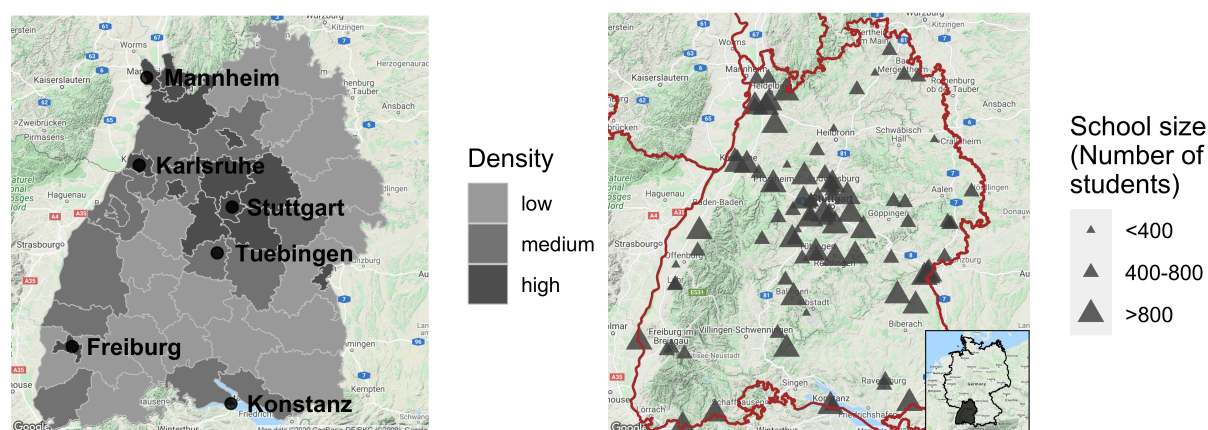
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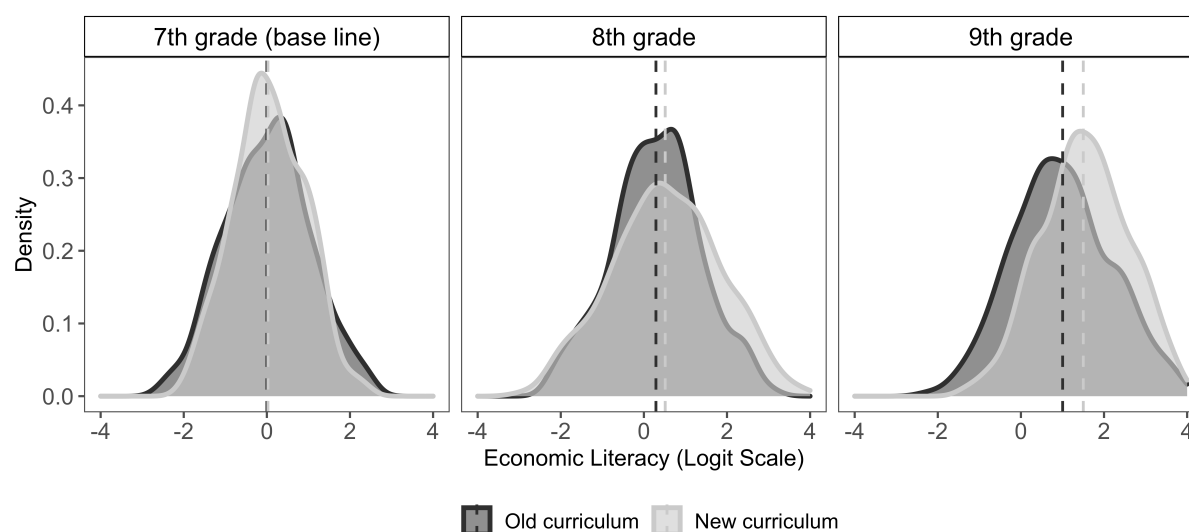
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**Figure 1: Degrees of urbanization and spatial distribution of sampled schools**



Population density map (left) and spatial distribution of sampled schools (right). A population density of below 220 residents per square kilometer is defined as a “low degree of urbanization”, between 220 and 485 residents per square kilometer defines a “medium degree of urbanization”, and more than 485 residents per square kilometer is defined as a “high degree of urbanization”.

**Figure 2: Distribution of economic competence**



This figure shows density plots for student economic competences for students affected by new curriculum and the control group. Plots for the baseline (7<sup>th</sup> grade) as well as one year (8<sup>th</sup> grade) and two years (9<sup>th</sup> grade) follow-up are presented separately. Vertical lines display weighted means for each group.

**Table 1: Overview of main differences between the curricula**

	New curriculum	Old curriculum
Implementation	Discrete subject course entitled “Economics and Vocational/Academic Orientation” with about 27 lessons per school year. Usually implemented as two lessons per week during one term (i.e., half a school year) of a given school year.	Combined subject course entitled “Geography, Economics, and Social Studies”. Economic education is integrated into the Social Studies curriculum in grade 8. Social Studies is mandated with about 54 lessons per school year. Given the extent of the curriculum, assuming a maximum of 50 percent of instruction being devoted to economic education is reasonable. Details of implementation under this curriculum may vary with some Social Studies courses possibly covering much less of the economic content.
Teachers	Teachers with a teaching degree in economics or social studies teachers (usually with teacher training in economics).	Social studies teachers
Class-level		
7	No	No
8	Yes	Yes
9	Yes	No
Total intensity	About 40 hours	Maximum of 20 hours
Contents	Curriculum based on competence model structured into three competence areas, (i) Decision-making and rationality (individual perspective); (ii) Relationships and interaction (social perspective), and (iii) System and order (systemic perspective). While the official syllabus provided by the ministry of education does not encompass a breakdown of contents and competence goals by class-level, blueprint curricula (used in teacher training) suggest fostering competences in all three areas at any class-level from different perspectives, i.e., (a) individuals as consumers of goods and services (8 <sup>th</sup> grade), (b) individuals as wage-laborer or self-entrepreneurs (9 <sup>th</sup> grade), and (c) individuals as tax-paying and voting citizens (10 <sup>th</sup> grade).	Curriculum based on content related learning goals in the following areas: (i) Consumption: - students can draft a budget plan - students are aware of the legal landscape for adolescent consumers - students can reflect on their role as consumers with emphasis on sustainable consumption (ii) Money: - students can explicate different purposes of money (income, savings, credit, investment) (iii) Businesses: - students can distinguish basic operations of firms (procurement, production, sales) preferably in a regional context - students can illustrate examples of sustainable production - students can obtain information about firms based in the region - students can acquire information about vocations and different working environments (including vocations in the social sector, i.e., vocational orientation in higher-track secondary schools)

The curriculum reform affects the cohort of students who attend 7<sup>th</sup> grade in the school year of 2017/2018. Students attending 7<sup>th</sup> grade in the school year of 2016/2017 are exposed to the old curriculum (and will continue their entire schooling career in the old curriculum framework). Both curricula cover economic aspects in grade 10, which is not covered in this study due to the Covid-19 pandemic affecting data collection.



**Table 2: Sample characteristics at baseline**

	New curriculum cohort (N=1,594)	Old curriculum cohort (N=1,503)	p-value for difference in means
<i>Individual characteristics</i>			
Male (1= yes)	48.6%	48.2%	0.871
Age (years)	14.534 (1.186)	14.574 (1.090)	0.957
Migrant (1=yes)	31.9%	32.5%	0.886
≤ 25 books at home (1=yes)	11.9%	12.7%	0.568
Reading abilities	4.073 (0.711)	4.037 (0.735)	0.395
Math abilities	3.658 (0.927)	3.571 (0.929)	0.140
Own salary	69.8%	69.7%	0.985
Effort score	0.968 (0.095)	0.975 (0.092)	0.262
<i>Class-level</i>			
7th grade	52.54 %	47.46 %	0.750
8th grade	49.39 %	50.61 %	0.623
9th grade	52.10 %	47.90 %	0.850
<i>School-level characteristics</i>			
Low degree of urbanization	32.5%	33.1%	0.816
School size (no. of students)	717.770 (272.954)	713.857 (204.199)	0.640

This table represents the mean and standard deviations in parentheses of individual and group-level characteristics for those who are affected by the reform (new curriculum) and those who are not affected (old curriculum). The third column reports the p-values testing that the coefficient in the new curriculum group is equal to zero in a linear regression with cluster robust standard errors.

**Table 3: Economic competence and interest in economic matters**

	Competence		Interest	
	(1)	(2)	(3)	(4)
New Curriculum	0.041 [0.089]	-0.002 [0.079]	0.098 [0.104]	0.058 [0.088]
8th grade	0.294*** [0.090]	0.385*** [0.055]	-0.157 [0.104]	-0.157* [0.086]
New Curriculum × 8th grade	0.206 [0.144]	0.175 [0.113]	0.369** [0.151]	0.395*** [0.128]
9th grade	1.014*** [0.095]	1.043*** [0.075]	-0.018 [0.085]	-0.006 [0.079]
New Curriculum × 9th grade	0.445*** [0.141]	0.411*** [0.119]	0.182 [0.124]	0.201* [0.111]
Constant	-0.003 [0.054]	0.152 [0.117]	-0.006 [0.073]	0.024 [0.111]
Controls	No	Yes	No	Yes
R-squared (Lev1, Lev2)	(0.29, 0.63)	(0.40, 0.75)	(0.03, 0.16)	(0.11, 0.31)
Observations	3,097	3,097	3,097	3,097
Number of groups	157	157	157	157

This table shows hierarchical regressions and robust standard errors clustered at school level with 20 imputations (see section 4 for details). The competence measure (column 1 and 2) is derived from the IRT model specified in section 4.1., with the control group in the baseline cohort (7<sup>th</sup> grade) being standardized to have a mean of 0 and a standard deviation of 1. We measure interest in economics (columns 3 and 4) by asking participants whether they are interested in economic topics on a scale from 1 (not at all) to 4 (very interested), with the same standardization as in columns 1 and 2. In 8<sup>th</sup> grade, the treatment group received one year of instruction, in the 9<sup>th</sup> grade two years. Control variables (individual and school-level) are defined as in Table 1. R-squared at school-level (Level 1) and individual level (Level 2) follows the method described in Snijders and Boskers (1999) \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 4: Financial Autonomy Index**

	Financial planning		Emotional		Functional		Reflexive	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Curriculum	0.106 [0.071]	0.058 [0.073]	0.036 [0.076]	0.094 [0.091]	0.057 [0.100]	0.061 [0.086]	0.156* [0.091]	0.081 [0.084]
Constant	-0.047 [0.053]	-0.789 [0.725]	-0.023 [0.051]	-0.476*** [0.142]	-0.034 [0.082]	-0.037 [0.173]	-0.081 [0.063]	-0.267 [0.173]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	899	851	899	851	899	851	899	851
R-squared (Lev1, Lev2)	(0.00, 0.03)	(0.10, 0.14)	(0.00, 0.02)	(0.05, 0.07)	(0.00, 0.03)	(0.08, 0.13)	(0.01, 0.05)	(0.07, 0.19)
Number of groups	48	47	48	47	48	47	48	47

Notes: This table presents hierarchical regressions on different attitude dimensions of the financial autonomy scale. Control variables (individual and school-level) are defined as in Table 1. R-squared at school-level (Level 1) and individual level (Level 2) follows the method described in Snijders and Boskers (1999). Cluster-robust standard errors in brackets, \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 5: Self-reported financial behaviors**

	(1) Savings (1/0)	(2) ln(Savings) <sup>#</sup>	(3) Any Debt (1/0)	(4) ln(Debt) <sup>#</sup>
New Curriculum	-0.022 [0.027]	-0.301 [0.484]	0.006 [0.023]	0.154 [0.215]
Constant		4.743*** [0.333]		-6.410*** [0.535]
Observations	786	638	772	764
R-squared		0.001		0.030

Notes: Column (1) reports estimated marginal effects of a probit model on the likelihood that a student saves. Column (2) displays OLS regression results on the natural logarithm of savings. Column (3) reports estimated marginal effects of a probit model on the likelihood that a student has debt. Column (4) shows OLS regression results on the natural logarithm of debt. # indicates that the outcome (in Euro) is winsorized at the 95<sup>th</sup> percentile. Cluster-robust standard errors in brackets, \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 6: Self-reported preferences**

	Risk seeking		Time preferences		Trust		Altruism	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Curriculum	-0.193**	-0.225***	0.181**	0.183**	-0.022	-0.029	0.008	0.048
	[0.080]	[0.071]	[0.080]	[0.079]	[0.029]	[0.026]	[0.081]	[0.089]
Constant	0.100*	-0.183	-0.088	0.009	0.259***	0.259***	0.000	-0.164
	[0.059]	[0.146]	[0.066]	[0.112]	[0.020]	[0.075]	[0.063]	[0.143]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Number of students	808	803	800	795	797	791	813	806
Number of schools	48	48	48	48	48	48	48	48
R-squared (Lev1, Lev2)	(0.01, 0.08)	(0.06, 0.20)	(0.01, 0.07)	(0.09, 0.13)	(0.00, 0.01)	(0.03, 0.03)	(0.00, 0.01)	(0.01, 0.03)

Notes: This table reports hierarchical regressions on various self-reported and non-experimental preferences with robust standard errors clustered at school-level. Control variables (individual and school-level) are defined as in Table 1. R-squared at school-level (Level 1) and individual level (Level 2) follows the method described in Snijders and Boskers (1999) \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 7: Attitudes towards profit maximization**

	(1) SHOVEL	(2) AUCTION	(3) PROFMAX	(4) PROFMAX-VP
New Curriculum	-0.011	0.007	-0.011	-0.010
	[0.041]	[0.036]	[0.049]	[0.051]
Controls	Yes	Yes	Yes	Yes
Observations	825	648	797	780

Notes: This table reports marginal effects from probit regressions on various allocation settings. SHOVEL takes the value 1 if students assess the price raise either as fair or totally fair. AUCTION takes value 1 if participants choose the auction over other allocation mechanisms for the football cards. PROFMAX and PROFMAX-VP take the value 1 if students decide to maximize profit, i.e., to layoff 100 workers and get € 2 Million profit. Regressions with control variables shown are listed in Table A6 in Appendix A. Cluster-robust standard errors (at the classroom-level) in brackets, \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 8: Preferences for alternative allocation mechanisms**

	(1) Price raise	(2) Greyhound	(3) Random	(4) Government	(5) Rationing
New Curriculum	0.024	-0.002	0.006	-0.072*	-0.002
	[0.031]	[0.044]	[0.026]	[0.042]	[0.035]
Controls	Yes	Yes	Yes	Yes	Yes
Observations	793	791	791	798	794

Notes: Marginal effects from probit regressions with cluster-robust standard errors (at school levels) in brackets. Each dependent variable takes the value 1 if the respondent evaluates the allocation mechanisms either as “fair” or “totally fair”. Regressions with control variables shown are listed in Table A6 in Appendix A. Cluster-robust standard errors (at the classroom-level) in brackets \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

# **Appendix**

**(Online appendix not intended for print publication)**

**to accompany**

**“Economic education at the expense of indoctrination? Evidence from Germany”**

**Table A1: Regression estimates (HLM) with control variables shown**

	Competence		Interest	
	(1)	(2)	(3)	(4)
New Curriculum	0.041 [0.089]	-0.002 [0.079]	0.098 [0.104]	0.058 [0.088]
8th grade	0.294*** [0.090]	0.385*** [0.055]	-0.157 [0.104]	-0.157* [0.086]
New Curriculum × 8th grade	0.206 [0.144]	0.175 [0.113]	0.369** [0.151]	0.395*** [0.128]
9th grade	1.014*** [0.095]	1.043*** [0.075]	-0.018 [0.085]	-0.006 [0.079]
New Curriculum × 9th grade	0.445*** [0.141]	0.411*** [0.119]	0.182 [0.124]	0.201* [0.111]
Male		0.222*** [0.041]		0.231*** [0.039]
Migrant		-0.351*** [0.043]		-0.088** [0.036]
≤ 25 books at home		-0.309*** [0.058]		-0.114* [0.060]
Own salary		-0.035 [0.041]		0.060 [0.036]
Math abilities		0.293*** [0.022]		0.121*** [0.020]
Reading abilities		0.237*** [0.024]		0.188*** [0.024]
Effort		0.027*** [0.003]		0.007** [0.003]
Low urbanization		-0.026 [0.058]		-0.169*** [0.052]
School size		-0.000 [0.000]		-0.000 [0.000]
Constant	-0.003 [0.054]	0.152 [0.117]	-0.006 [0.073]	0.024 [0.111]
R-squared (Lev1, Lev2)	(0.29, 0.63)	(0.40, 0.75)	(0.03, 0.16)	(0.11, 0.31)
Observations	3,097	3,097	3,097	3,097
Number of groups	157	157	157	157

Notes: This table shows hierarchical regressions and robust standard errors clustered at school level with 20 imputations (see section 4 for details) as in Table 2. Control variables (individual and school-level) are defined as in Table 1. R-squared at school-level (Level 1) and individual level (Level 2) follows the method described in Snijders and Boskers (1999) \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table A2: Effects of the curriculum mandate on economic competence and interest in economic matters (OLS)**

	Competence		Interest	
	(1)	(2)	(3)	(4)
New Curriculum	0.026 [0.088]	-0.007 [0.078]	0.082 [0.105]	0.043 [0.089]
8th grade	0.290*** [0.089]	0.380*** [0.054]	-0.172* [0.103]	-0.174** [0.087]
New Curriculum × 8th grade	0.197 [0.145]	0.160 [0.112]	0.389** [0.152]	0.416*** [0.129]
9th grade	1.004*** [0.091]	1.031*** [0.073]	-0.032 [0.087]	-0.017 [0.080]
New Curriculum × 9th grade	0.471*** [0.139]	0.433*** [0.119]	0.210* [0.125]	0.223** [0.112]
Constant	0.000 [0.052]	0.184 [0.119]	0.002 [0.075]	0.024 [0.111]
Controls	No	Yes	No	Yes
Adj. R <sup>2</sup>	0.21	0.40	0.03	0.10
Number of schools	157	157	157	157
Number of students	3,097	3,097	3,097	3,097

Notes: Standard errors (clustered at the school-level) in brackets, \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table A3: Quantile regression for treatment effects on economic competence**

	(1) 20th	(2) 40th	(3) 50th	(4) 60th	(5) 80th
New Curriculum	0.186 [0.116]	0.040 [0.095]	0.038 [0.088]	0.016 [0.086]	-0.082 [0.108]
8th grade	0.356*** [0.109]	0.350*** [0.085]	0.348*** [0.071]	0.399*** [0.067]	0.281*** [0.101]
New Curriculum × 8th grade	-0.223 [0.179]	0.148 [0.146]	0.199 [0.142]	0.270** [0.136]	0.492*** [0.161]
9th grade	0.841*** [0.123]	0.941*** [0.100]	0.966*** [0.106]	1.078*** [0.111]	1.172*** [0.122]
New Curriculum × 9th grade	0.418** [0.163]	0.520*** [0.137]	0.464*** [0.148]	0.467*** [0.162]	0.445*** [0.155]
Constant	-0.385*** [0.131]	0.088 [0.110]	0.295** [0.124]	0.532*** [0.130]	1.081*** [0.160]
Controls	Yes	Yes	Yes	Yes	Yes
Observations	2,896	2,896	2,896	2,896	2,896
R <sup>2</sup>	0.356	0.368	0.369	0.367	0.361

*Notes:* This table reports simultaneous quantile regressions for the impact of the curriculum mandate on economic competences as described in section 3. The output displays treatment effects for each quartile and the median. The number of observations is slightly reduced due to missing values (item non-response) in covariates. Standard errors (clustered at the school-level) in brackets. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

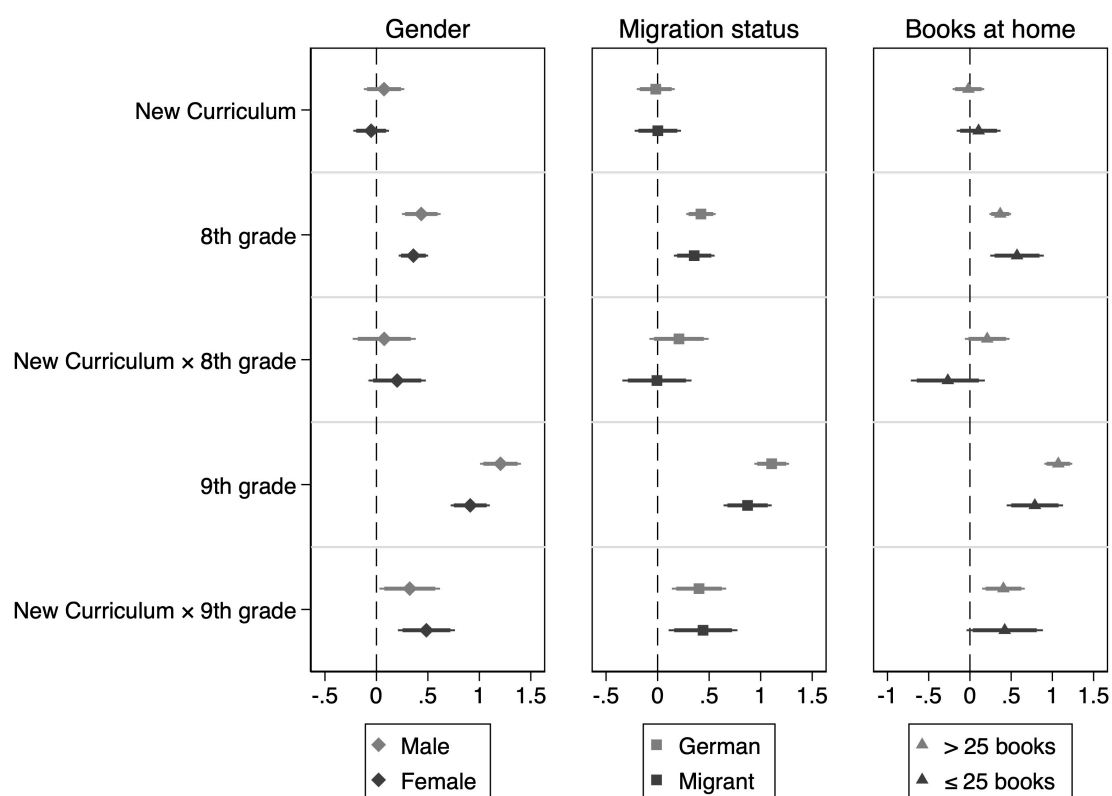


**Table A4: Balance on observables for the subsample of 8<sup>th</sup> graders**

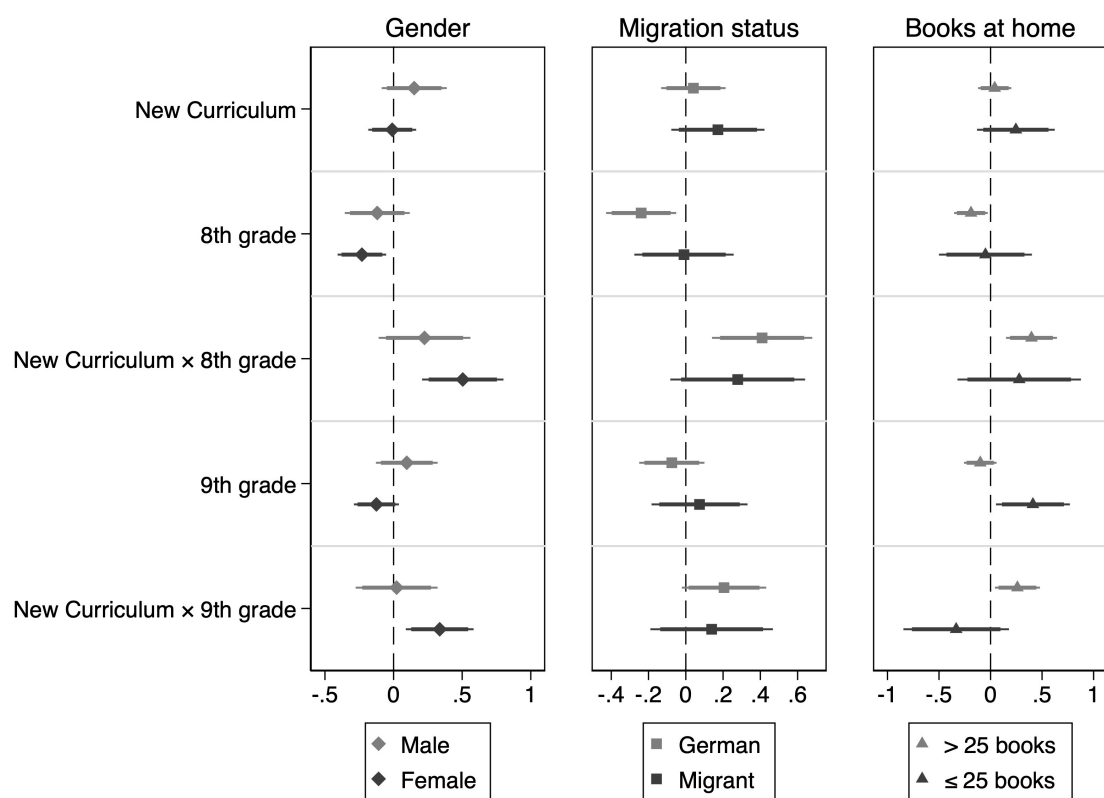
	New Curriculum (N=444)	Old curriculum (N=455)	t-Test (p-value)
<i>Individual characteristics</i>			
Male	49.8%	50.8%	0.844
Age	14.502 (0.977)	14.477 (0.795)	0.797
Migrant	32.9%	31.0%	0.651
≤ 25 books at home	12.4%	9.7%	0.229
Reading abilities	4.027 (0.798)	3.976 (0.764)	0.432
Math abilities	3.523 (1.012)	3.553 (0.998)	0.745
Own salary	70.7%	70.9%	0.969
<i>School-level characteristics</i>			
Low urbanization	19.4%	31.4%	0.365
School size	768.919 (150.973)	568.294 (273.358)	0.004

Note: This table represents the mean and standard deviations in parentheses of individual and group-level characteristics for those who are affected by the reform (new curriculum) and those who are not affected (old curriculum) for the subsample of 8<sup>th</sup> graders. The third column reports the p-values of a t-test that the coefficient in the new curriculum group is equal to zero in a linear regression with cluster robust standard errors.

**Figure A1: Heterogeneous effects on economic competence**



**Figure A2: Heterogeneous effects on economic interest**



**Table A5: Effect on non-cognitive outcomes (with control variables shown)**

	Financial autonomy scale				Preferences			Financial behaviors				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Financial planning	Emotional	Functional	Reflective	Risk preferences	Time preferences	Trust	Altruism	Savings (1/0)	ln(Savings)	Debt (1/0)	ln(Debt)
New Curriculum	0.056 [0.072]	0.094 [0.091]	0.061 [0.086]	0.081 [0.084]	-0.225*** [0.071]	0.183** [0.079]	-0.029 [0.026]	0.048 [0.089]	-0.017 [0.027]	-0.137 [0.494]	0.006 [0.023]	0.154 [0.215]
Controls												
Male	-0.115* [0.068]	0.203** [0.085]	-0.132* [0.070]	0.428*** [0.071]	0.053 [0.067]	-0.083 [0.083]	0.016 [0.030]	-0.129** [0.063]	-0.050* [0.027]	-0.781 [0.486]	0.013 [0.028]	-0.018 [0.246]
Migrant	-0.088 [0.071]	0.146* [0.079]	-0.093 [0.063]	0.318*** [0.056]	0.170** [0.079]	-0.016 [0.084]	-0.086*** [0.030]	-0.055 [0.079]	-0.071** [0.028]	-1.420*** [0.520]	0.006 [0.030]	0.084 [0.287]
<25 books at home	0.066 [0.115]	-0.330** [0.129]	-0.268** [0.121]	-0.149 [0.115]	0.241* [0.136]	0.184 [0.144]	-0.053 [0.045]	-0.041 [0.128]	-0.033 [0.033]	-1.275 [0.767]	0.005 [0.034]	0.153 [0.391]
Reading abilities	0.132*** [0.047]	0.097 [0.062]	0.229*** [0.066]	-0.012 [0.047]	0.036 [0.058]	0.283*** [0.063]	-0.006 [0.020]	-0.050 [0.076]	0.043*** [0.016]	0.757** [0.308]	-0.006 [0.018]	-0.056 [0.198]
Math abilities	0.267*** [0.040]	0.057 [0.043]	0.086** [0.035]	0.048* [0.029]	-0.098*** [0.044]	0.152*** [0.031]	0.055*** [0.017]	0.063* [0.033]	0.001 [0.013]	0.313 [0.213]	-0.046*** [0.016]	-0.403*** [0.153]
Own salary	-0.020 [0.080]	0.245*** [0.092]	0.021 [0.074]	0.129* [0.070]	0.218*** [0.081]	0.009 [0.059]	-0.001 [0.039]	0.058 [0.074]	0.066*** [0.022]	1.191** [0.445]	-0.003 [0.023]	0.149 [0.181]
Low urbanization	0.059 [0.082]	-0.075 [0.082]	0.099 [0.104]	0.105 [0.066]	0.214*** [0.074]	0.008 [0.094]	0.072** [0.032]	0.156** [0.076]	0.029 [0.027]	0.167 [0.453]	-0.054 [0.035]	-0.434 [0.289]
School size	-0.000 [0.000]	0.000* [0.000]	0.000 [0.000]	0.006* [0.000]	-0.000 [0.000]	-0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.001 [0.001]	0.000 [0.000]	0.001 [0.001]
Constant	0.126 [0.116]	-0.476*** [0.142]	-0.037 [0.173]	-0.267 [0.173]	-0.183 [0.146]	0.009 [0.112]	0.259*** [0.075]	-0.164 [0.143]	[0.000]	4.295*** [0.766]	[0.000]	-6.410*** [0.535]
Observations	851	851	851	851	793	785	779	796	769	623	772	764
Adj. R-squ.	(0.10, 0.14)	(0.05, 0.07)	(0.08, 0.13)	(0.07, 0.19)	(0.06, 0.20)	(0.09, 0.13)	(0.03, 0.03)	(0.01, 0.03)		0.080		0.030
Robust standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1												

**Table A6: Effect on normative attitudes (with control variables)**

	Indoctrination				Attitudes towards allocation mechanisms (Mountain)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Shovel	Auction	Profmax	ProfmaxVP	Price raise	Greyhound	Random	Government	Rationing
New Curriculum	-0.011 [0.041]	0.007 [0.036]	-0.011 [0.049]	-0.010 [0.051]	0.024 [0.031]	-0.002 [0.044]	0.006 [0.026]	-0.072* [0.042]	-0.002 [0.035]
<i>Controls</i>									
Male	0.060 [0.040]	0.074*** [0.029]	0.032 [0.039]	-0.012 [0.043]	0.059** [0.028]	0.096*** [0.028]	0.088*** [0.020]	-0.002 [0.034]	-0.018 [0.028]
Migrant	0.063 [0.048]	0.091*** [0.033]	-0.033 [0.046]	-0.064* [0.037]	-0.004 [0.036]	-0.046 [0.040]	0.033* [0.018]	0.087* [0.047]	-0.036 [0.024]
<25 books at home	0.015 [0.054]	0.022 [0.058]	-0.048 [0.068]	-0.036 [0.065]	0.015 [0.042]	0.047 [0.053]	-0.003 [0.022]	0.040 [0.066]	-0.065** [0.031]
Reading abilities	0.098*** [0.025]	0.000 [0.019]	0.030 [0.029]	0.029 [0.025]	0.017 [0.020]	0.028 [0.028]	-0.025*** [0.009]	-0.037 [0.024]	0.028** [0.013]
Math abilities	0.009 [0.017]	-0.022 [0.016]	0.051*** [0.019]	0.087*** [0.022]	-0.021 [0.015]	0.040** [0.018]	-0.024** [0.011]	-0.008 [0.015]	0.055*** [0.013]
Own salary	-0.026 [0.038]	0.004 [0.038]	-0.031 [0.041]	-0.001 [0.035]	0.008 [0.031]	-0.016 [0.043]	-0.025 [0.026]	-0.061** [0.029]	0.010 [0.019]
Low urbanization	0.042 [0.030]	0.039 [0.032]	-0.023 [0.052]	0.042 [0.052]	-0.016 [0.034]	0.022 [0.040]	0.020 [0.028]	0.045 [0.033]	0.022 [0.032]
School size	0.000 [0.000]	0.000 [0.000]	-0.000 [0.000]	0.000 [0.000]	0.000** [0.000]	0.000 [0.000]	0.000** [0.000]	-0.000 [0.000]	-0.000 [0.000]
Observations	825	648	797	780	793	791	791	789	794

Robust standard errors in brackets; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table A7: Effect on normative attitudes  
(for respondents above the median of the competence distribution)**

	(1)	(2)	(3)	(4)
	SHOVEL	AUCTION	PROFMAX	PROFMAX-VP
New Curriculum	-0.004 [0.052]	0.013 [0.041]	0.004 [0.073]	-0.003 [0.071]
Observations	424	353	433	433

Note: This table reports marginal effects from probit regressions on various allocation settings for respondents who are above the mean in the competence distribution. SHOVEL takes the value 1 if students assess the price raise either as fair or totally fair. AUCTION takes value 1 if participants choose the auction over other allocation mechanisms for the football cards. PROFMAX and PROFMAX-VP take the value 1 if students decide to maximize profit, i.e., to layoff 100 workers and get 2 Million profit. Cluster-robust standard errors in brackets, \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

## Appendix B: Test of economic competence (psychometric properties)

**Table B1: Item statistics for the measure of economic competence**

Item	<i>n</i>	CTT		IRT			
		<i>RelFreq</i>	<i>r<sub>it</sub></i>	$\alpha$	[SE]	$\sigma$	[SE]
1*	3,092	0.821	0.199	0.637	[0.020]	-2.587	[0.076]
2	1,905	0.807	0.222	0.731	[0.030]	-2.332	[0.082]
3	1,871	0.758	0.337	1.201	[0.054]	-1.352	[0.049]
4	1,885	0.661	0.199	0.547	[0.034]	-1.472	[0.091]
5*	2,988	0.737	0.341	1.076	[0.040]	-1.158	[0.042]
6	1,948	0.726	0.301	0.950	[0.045]	-1.379	[0.057]
7	1,932	0.664	0.137	0.369	[0.024]	-2.073	[0.132]
8*	2,968	0.747	0.345	1.143	[0.042]	-1.170	[0.041]
9	1,884	0.714	0.278	0.835	[0.042]	-1.413	[0.065]
10*	2,979	0.176	0.288	0.913	[0.030]	1.967	[0.056]
11*	2,971	0.447	0.361	0.998	[0.047]	0.269	[0.041]
12*	2,882	0.506	0.323	0.851	[0.046]	-0.023	[0.047]
13*	2,928	0.369	0.259	0.643	[0.033]	0.925	[0.062]
14*	2,882	0.519	0.354	0.950	[0.048]	-0.080	[0.043]
15*	2,742	0.405	0.386	1.054	[0.048]	0.472	[0.041]
16*	2,851	0.411	0.325	0.848	[0.043]	0.506	[0.048]
17*	2,921	0.540	0.268	0.682	[0.042]	-0.242	[0.057]
18*	2,999	0.337	0.327	0.893	[0.038]	0.892	[0.047]
19*	2,982	0.726	0.264	0.774	[0.030]	-1.410	[0.056]
20*	2,916	0.780	0.232	0.715	[0.025]	-1.928	[0.065]
21	1,880	0.727	0.370	1.323	[0.062]	-1.129	[0.044]
22*	2,931	0.782	0.298	0.971	[0.034]	-1.537	[0.049]
23*	2,914	0.805	0.348	1.297	[0.045]	-1.402	[0.040]
24*	2,912	0.789	0.306	1.061	[0.037]	-1.476	[0.046]
25*	2,914	0.815	0.289	1.026	[0.034]	-1.693	[0.050]
26*	2,932	0.836	0.267	0.974	[0.032]	-1.940	[0.054]
27	1,747	0.639	0.303	0.873	[0.052]	-0.896	[0.061]
28*	2,940	0.664	0.270	0.735	[0.034]	-1.016	[0.056]
29	1,599	0.281	0.314	0.966	[0.050]	1.008	[0.062]
30	1,788	0.442	0.279	0.753	[0.055]	0.197	[0.067]
31	1,036	0.855	0.364	1.674	[0.097]	-1.136	[0.060]
32	1,052	0.679	0.331	0.975	[0.068]	-0.553	[0.073]
33	1,026	0.373	0.234	0.568	[0.053]	1.349	[0.118]
34	1,049	0.251	0.166	0.429	[0.027]	3.011	[0.169]
35	1,048	0.371	0.358	0.994	[0.073]	0.993	[0.070]
36	929	0.547	0.385	1.170	[0.093]	0.225	[0.063]
37	936	0.308	0.245	0.643	[0.050]	1.790	[0.115]
38	1,026	0.479	0.363	1.009	[0.083]	0.463	[0.068]
39	968	0.433	0.258	0.681	[0.070]	0.850	[0.100]
40	1,022	0.673	0.460	1.645	[0.105]	-0.266	[0.048]
41	1,039	0.664	0.234	0.618	[0.050]	-0.833	[0.110]

Notes: This table shows parameter estimates and standard errors for the *IRT* model specified in equation (1) as well as indicators from Classical Test Theory (*CTT*). *RelFreq* denotes the relative frequency of correct responses to the item. *r<sub>it</sub>* reports corrected item correlations, i.e., the point-biserial correlation between item response and the sum of all correct responses.  $\alpha$  denotes the discrimination parameter as specified in equation (1) and  $\sigma$  denotes the difficulty parameter. Asterisks indicate items which were administered to students in all waves.

## Appendix C: Survey items (financial autonomy scale and normative attitudes)

### Figure C1: Normative attitudes

#### 1. SHOVEL (Cipriani et al. 2009)

A hardware store has been selling snow shovels for 15 Euro. The morning after a large snowstorm, the store raises the price to 20 Euro. Please rate this action as:

- a) Very unfair
- b) Unfair
- c) Acceptable
- d) Very unfair

#### 2. AUCTION (Cipriani et al. 2009)

A football team normally sells some tickets on the day of their games. Recently, interest in the next game has increased greatly, and tickets are in great demand. The team owners can distribute the tickets in one of three ways. (1) By auction: the tickets are sold to the highest bidders. (2) By lottery: the tickets are sold to the people whose name are drawn. (3) By queue: the tickets are sold on a first-come first-served basis. Rank these three in terms of which you feel is the most fair and which is the least fair – the auction, the lottery, and the queue.

#### 3. PROFMAX (Rubinstein 2006)

Assume that you are a vice president of ILJK company. The company provides extermination services and employs a certain number of permanent administrative workers and 196 nonpermanent workers who are sent out on extermination jobs. The company was founded five years ago and is owned by three families. The work requires only a low level of skill, with each worker requiring only one week of training. All the company's employees have been with the company for three to five years. The company pays its workers more than minimum wage. A worker's salary includes payment for overtime, which varies from 1200 to 1500€ per month (suppose that the minimum wage is about 1000€ per month). The company makes sure to provide its employees with all the benefits required by law. Until recently, the company was making large profits. As a result of the continuing recession, there has been a significant drop in profits, although the company is still in the black. You will be attending a meeting of the management in which a decision will be made regarding the layoff of some of the workers. ILJK's finance department has prepared scenarios of annual profits shown in the following.

Number of workers who will continue to be employed	Expected annual profit (millions of euro)
0 (all the workers to be laid off)	Loss of 8
50 (146 workers to be laid off)	Profit of 1
65 (131 workers to be laid off)	Profit of 1.5
100 (96 workers to be laid off)	Profit of 2
144 (52 workers to be laid off)	Profit of 1.6
170 (26 workers to be laid off)	Profit of 1
196 (0 workers to be laid off)	Profit of 0.4

Complete the following: I recommend continuing to employ . . . of the 196 workers presently employed by the company.

#### 4. PROFMAX-VP (Rubinstein 2006)

*What do you think would be the choice of a real vice president in Question 3? I think that he would recommend continuing to employ. . . of the 196 workers in the company.*

#### 5. FAIRNESS ALLOCATION METHODS (Frey et al. 1993)

*At a sight-seeing point, reachable only by foot, a well has been tapped. The bottled water is sold to thirsty hikers for two Euro per bottle. The maximum daily production are 100 bottles. On a particularly hot day, 200 thirsty hikers are expected. Please judge the following measures for allocating the water among the thirsty hikers:*

1. a) *The price is increased to four Euro per bottle.*
2. b) *Selling the water for two Euro per bottle to the first 100 hikers according to “first come, first served”.*
3. c) *Selling the water for two Euro per bottle to the 100 hikers whose last name by chance happens to start with the letters A to K.*
4. d) *The local community buys all bottles for two Euro per bottle and distributes them as it sees fit.*
5. e) *Selling half-sized bottles for one Euro per bottle to all hikers (one bottle per hiker only).*



**Figure C2: Financial planning and financial autonomy scale (Micarello et al. 2012)**

Financial planning:

- I budget my money very well.
- I am very careful with my money.
- I am proud of my ability to save money.
- I keep track of my money.
- I regularly put money aside for the future.
- I often spend money even though I didn't plan to. (-)
- I sometimes have to borrow money from others to make ends meet. (-)
- I keep regular records of my income and expenses.

Emotional:

- I would like to have a vote when expensive new purchases are made in my family.
- I usually take a critical view of how my friends handle money.
- At home, I plan along with the expenses.
- I try to advise my parents on financial matters.
- I am capable of having conversations with my parents about money.

Functional:

- I try to save money for the things I really like.
- I like to negotiate prices when buying something.
- At home, I recommend setting aside money for emergencies.
- I keep an eye out for deals and perks.
- I am willing to do without something today in order to be able to buy something important later.

Attitudinal:

- I like to think carefully before I decide to buy something.
- I like to compare prices before I buy anything.
- I make sure I am well informed about warranty conditions.
- I always try to get information about the quality of a product.
- I follow news about the economy because it could possibly affect my family.