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Financial Education in Schools: A Meta-Analysis of Experimental Studies

Abstract

We study the literature on school financial education programs for children and youth via a quantitative meta-analysis of 37 (quasi-) experiments. We find that financial education treatment has, on average, a significant and sizeable impact on financial knowledge (+0.25 SD), similar to educational interventions in other domains. Additionally, we document small but still significant effects on financial behaviors (+0.05 SD). These results are robust to restricting the sample to 18 randomized experiments and they hold irrespective of the meta-analytic method used. Meta-regressions show the beneficial effect of more intensive treatments and smaller class size, albeit with decreasing marginal returns.

JEL-Codes: I210, A210, D140.

Keywords: financial education, financial literacy, financial behavior, meta-analysis.

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Financial education in schools: A meta-analysis of experimental studies

1 Introduction

Financial education is high on the agenda of policymakers worldwide. An abundance of rigorous empirical research shows the importance of financial literacy for individual welfare (cf. van Roij et al., 2011, 2012; Lusardi and Mitchell, 2014). Financial education policies and programs are being installed in the vast majority of OECD countries and in many of the largest emerging economies, such as China and India (see OECD, 2015). While these programs vary in size, design and coverage, many of these programs are designed to be implemented in schools. School-based financial education may be seen as a promising avenue since it allows an almost universal coverage of a cohort, mitigating previously documented low-demand of voluntary financial education later in the life-cycle (e.g. Bruhn et al., 2014). Moreover, providing financial education during formative years could be effective and sustainable with respect to long-term outcomes (e.g., Lusardi et al., 2010; Frisancho, 2018; Lührmann et al., 2018).

We contribute to the literature – to the best of our knowledge – with the first meta-analysis focusing exclusively on the impact of financial education in schools. The empirical basis of our meta-analysis is the complete set of those empirical studies that (i) report about impacts of financial education programs in schools, (ii) provide a quantitative assessment of treatment effects and (iii) rely on a control group. In summary there are 37 independent (quasi-) experimental studies fulfilling the above three criteria, 18 of them are randomized experiments (RCTs). As studies mostly report impacts on a set of several outcomes, our meta-analysis relies on 177 effect size estimates, of these 70 refer to financial knowledge and 107 refer to financial behavior.

Based on this sample of studies we find, on average, the intended treatment effects, i.e. improved financial knowledge test scores and changes in financial behaviors that are typically assumed to be enhancing individual welfare (e.g., increasing personal savings). We show that these effects are statistically significant from zero, that they hold for the outcomes of financial knowledge and behavior, and that they exist also when restricting the sample to RCTs. Reassuringly, these results are robust to employing various estimation methods: as to be expected, the effect of financial education on knowledge is higher than on financial behavior, and the effect documented in RCTs is estimated to be smaller than in quasi-experimental studies. However, even the smallest effect size we find in our study, i.e. from financial education treatment on financial behavior in RCTs estimated by weighted least squares with a correction for publication selection bias, still has a positive and highly significant coefficient. All this evidence unambiguously supports the case of financial education in schools.

Comparing the effectiveness of financial education in schools to the larger universe of empirical studies on financial education programs (including adults) as examined in Kaiser and Menkhoff (2017), our results are consistent with the interpretation that the impact on *knowledge* is, on average, even higher than in the larger sample of studies while the impact on *behavior* is smaller. This is a plausible result as younger people may generally have a higher capacity to learn than adults which could explain the higher average treatment effects of financial education for children and youth on financial knowledge. At the same time, we know that the motivation to incorporate financial knowledge into financial behavior is higher when financial decisions are more immediate and relevant (e.g., Miller et al., 2015; Kaiser and Menkhoff, 2017), and this may not apply to younger students in schools. Additionally, changes in financial behavior among children and youth are inherently more difficult to observe (measure), since children do not engage in a lot of financial decision at this stage of the life-cycle. Thus, average effect sizes on actual financial behaviors are estimated to be smaller than in the general literature but are still statistically significant.

Our main result is of high relevance for policy makers because the evidence clearly suggests that investing into the implementation of school financial education curricula does indeed impact financial knowledge, and to a smaller extent financial behavior. This result is particularly important because there is a public debate questioning the effectiveness of financial education in quite fundamental ways (e.g. Willis, 2011; Fernandes et al., 2014). While we address some of the qualitative arguments and alternatives suggested towards the end of this paper, we take care to probe the robustness of our conclusions by empirically investigating the possibility of publication selection bias. There is indeed such a bias in this literature, favoring the publication of statistically significant results, but this bias is too small to qualitatively arrive at different conclusions about the average impact of financial education programs.

Against the backdrop of scarce public resources, we finally examine potential determinants of effective financial education programs in schools. Unfortunately, the number and comparability of studies at hand is not large enough to generate truly granular and empirically significant results in this respect. For example, potentially crucial determinants of effective programs cannot be directly assessed in this quantitative meta-analysis, such as differences in implementation quality (Urban et al., 2018), in teacher training and experience (Rockoff, 2004; Harris and Sass, 2011; Chetty et al., 2014; Urban et al., 2018), the quality of curricula (Drexler et al., 2014), material and media (Heinberg et al., 2014; Lusardi et al., 2017), and the teaching methods employed (Kaiser and Menkhoff, 2018).

Still, we get five results being relevant in designing school financial education programs: research design and measurement matters in impact evaluation of financial education interventions as is known from earlier meta-analyses (Fernandes et al., 2014; Kaiser and Menkhoff, 2017), i.e. (i) the effect size measured in RCTs is lower than in other study types, and (ii) a longer delay between training and measurement is associated with a smaller degree of estimated effectiveness. Moreover, we provide evidence of three more specific results which also fit into the broader literature on educational interventions, i.e. (iii) younger students learn more than older ones with higher effect sizes reported in interventions in primary schools compared to high schools, (iv) higher intensity of teaching increases effectiveness with declining marginal returns, and (v) lower student to teacher ratios (class size) may be associated with a higher degree of effectiveness. These last three results provide the basis to derive some policy thoughts about potentially "optimal" strategies and timing of financial education in schools.

This meta-analysis contributes to two lines of literature, i.e. meta-analyses on the effectiveness of financial education in general, and studies examining the effectiveness of particular financial education programs in schools. Regarding the more general meta-analyses, the first one by Fernandes et al. (2014) uses broader selection criteria (including observational studies) than we do and covers studies until 2013. That study fundamentally questions the success of financial education (for adults) by the argument that only observational studies show a positive relationship between financial literacy and behavior, on average, but that a restriction to (quasi-) experiments and, in particular to 12 RCTs, reveals the elusiveness of financial education (with average zero-effects in the early literature). The next meta-analysis by Miller et al. (2015) limits itself to just 18 studies studying homogenous outcomes and shows that some financial behaviors, such as savings behavior, seem easier to be impacted than others, such as credit defaults. Kaiser and Menkhoff (2017) conduct a meta-analysis aiming for full coverage of financial education interventions; this ambition is comparable to the "manipulated-literacy sample" assembled by Fernandes et al. (2014) but data are more recent. It is found that financial education is effective, and this finding holds also for the sub-sample of rigorous RCTs. We are closest to Kaiser and Menkhoff (2017) but focus exclusively on financial education in schools and update the data by considering additional recent studies. This procedure results in a more homogenous sample of experimental studies and allows the investigation of design features specific to school financial education programs.

Another strand of the literature uses (plausibly) exogenous variation in U.S. high school financial education mandates across federal states over time to investigate effects of exposure to financial education on financial knowledge (Tennyson and Nguyen, 2001) and financial behaviors, such as the handling of debt (i.e. reduction in defaults) and savings outcomes (see Bernheim et al., 2001; Brown et al., 2016; Cole et al., 2016; Urban et al., 2018). While this literature documents a positive effect of financial education on financial knowledge (Tennyson and Nguyen, 2001) and on savings (e.g. Bernheim et al., 2001), it provides partially conflicting results on the (long-term) effects of financial education mandates on credit-related behavior (e.g. Brown et al., 2016; Cole et al., 2016). Brown et al. (2016) reveal long-term effects of financial education on reduced debt levels and loan defaults, but Cole et al. (2016) do not find such an effect. Recently, Urban et al. (2018) show that accounting for heterogeneity in the timing and quality of policy implementation at the state-level leads to the assessment of positive effects of financial education mandates on credit outcomes among young adults. Thus, while parts of this literature document important and long-run effects of financial education on financial outcomes with high external validity, the high degree of variation in the employed research designs in these papers (e.g. the definition of policy changes varies across studies) makes the systematic integration of this literature into a meta-analysis hardly possible.[1] Thus, we only include controlled (quasi-)experiments where the treatment is closely observed by the researchers.

This paper is structured into four further sections. Section 2 introduces into the meta-study method and into selection criteria for considered studies. Section 3 describes the data and Section 4 the regression results. Finally, Section 5 concludes by discussing results from a policy perspective.

[1] There are several other studies on the effects of financial education courses in school on financial knowledge and financial behavior (e.g., Peng et al., 2007; Grimes et al., 2010). These studies are observational and have varying degrees of internal validity, so that we do not include them in our meta-analysis.

2 Method

Meta-analysis is a quantitative method to integrate results from multiple empirical studies on the same empirical phenomenon (see Stanley, 2001 for an introduction). In a meta-analysis, the dependent variable is comprised of summary statistics reported in the primary research, while the explanatory variables may include, for example, characteristics of the research design, the target group, or the particular education program. Meta-analyses are helpful to address three types of general research questions about a given empirical literature: First, what is the direction and size of the (weighted) average effect of a treatment? Second, are results consistent across studies or is there a high degree of heterogeneity in reported findings (beyond measurement error)? Third, are there observable study or program characteristics that may explain part of this heterogeneity?

To be able to draw conclusions about an entire empirical literature, one has to assemble a complete representation of the literature of interest, meeting certain quality and inclusion criteria. Thus, we build on our existing database and update it using the same search strategy as described in Kaiser and Menkhoff (2017). We augment the earlier dataset with published studies on financial education in schools between October 2016 (end of collection period in Kaiser and Menkhoff, 2017) and September 2018 (end of collection period for this paper). Additionally, as our review of the larger literature on financial education included a screening of references from previous meta-analyses (Fernandes et al., 2014; Miller et al., 2015) as well as narrative reviews (Fox et al., 2005; Collins and O'Rourke, 2010; Willis, 2011; Xu and Zia, 2012; Hastings et al., 2013; Blue et al., 2014; Lusardi and Mitchell, 2014) we also screen the references of more recent or more focused narrative reviews of financial education for children and youth in schools (Collins and Odders-White, 2015; Walstad et al., 2017; Amagir et al., 2018). We screen all of the abstracts for relevance and apply our inclusion criteria to the remaining full texts: We include papers (i) reporting on impacts of an educational intervention on financial literacy and/or financial behavior for youth in schools, (ii) providing a quantitative assessment of intervention impact that allows coding an effect size statistic (g) and its standard error, and (iii) relying on a control group in the estimation of intervention impacts. Consequently, we only include (quasi-) experimental studies with sufficient information on intervention outcomes in our analysis while neglecting single-group pre-/post comparisons, since these have a lower degree of internal validity. Where necessary information is only partially missing, we consult additional online resources related to the article or contact the authors of the primary studies directly.

In order to be able to aggregate estimated treatment effects reported across multiple studies, one must standardize these statistics into a common metric. In an ideal world, all of these studies would measure the outcomes of financial education identically, i.e. in the same unit. If this was the case, a meta-analysis could be performed directly on the outcomes and standardization would not be necessary (this is sometimes referred to as economic effect sizes, such as percentage change or elasticities). In the heterogeneous body of literature on school financial education, however, standardization becomes necessary, because studies typically measure increases in financial knowledge in different ways (use different test items) or employ multiple methods or data sources to measure changes in financial behaviors. Thus, we conduct our meta-analysis using scale-free statistical effect sizes. Specifically, we compute the so-called "bias corrected standardized mean difference" (*Hedges' g*) as our effect size measure for each reported estimate within studies. This measure reports treatment effects in the form of scale-free standard deviation units.[2]

Regarding the meta-analytic method, there is a variety of models available, each making different assumptions: In the meta-analysis literature it is common to distinguish between a "fixed-effect" approach and a "random-effect" approach (see Lipsey and Wilson, 2001). Choosing a model from the "fixed-effect"-family implies that the researcher assumes the source of variance to be exclusively due to measurement error within each study. Put in other words: if each study had indefinitely large sample sizes one would be able to observe and calculate an estimate of a common true effect that every study shares. In contrast, a "random-effect" approach assumes that – in addition to within-study measurement error – there exists actual heterogeneity in the true effects between each study. Even if studies had no measurement error, it would still be possible that two studies would not share a common true effect. While most of canonical meta-analysis models (e.g., DerSimonian and Laird, 1986) from other disciplines use a random-effect approach, the use of "fixed-effects" models is much more frequent in meta-analysis of economic research (e.g. Staney and Doucouliagos, 2012).

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[2] Formally, Hedges' g is defined as: $g = \frac{M_T - M_C}{SD_p}$ with $SD_p = \sqrt{\frac{(n_T-1)\, SD_T^2 + (n_C-1)\, SD_C^2}{n_T^2 + n_C^2 - 2}}$. n_T and SD_T are the sample size and standard deviation of the treatment group, and n_C and SD_C are for the control group. Additionally, the standard error of each standardized mean difference (g), is defined as: $SE_g = \sqrt{\frac{n_T + n_C}{n_T n_C} + \frac{g^2}{2(n_T + n_C)}}$.

We use multiple approaches from both families of models to investigate the sensitivity of our results to the assumptions implied by each meta-analytic method and to arrive at estimates for an average (weighted) effect of financial education treatment on financial knowledge and financial behavior.

First, we estimate an unweighted average effect of financial education by relying on an ordinary least squares (OLS) model where each study contributes multiple effect sizes (see Card et al., 2017 for such an approach). We account for the statistical dependency of estimates in this data-structure by clustering the standard errors at the study level.

Second, we estimate the same model but weight each effect size estimate by its inverse standard error or the inverse variance, respectively. This unrestricted weighted least squares (WLS) estimation is advocated by Stanley and Doucouliagos (2012, 2015) and is capable of accounting for heteroskedasticity and excess heterogeneity.

Third, we account for potential publication selection bias in the financial education literature by testing for funnel asymmetry (FAT) and estimating both “precision-effect test” and “precision-effect estimate with standard error” (PET and PEESE) models as suggested by Stanley and Doucouliagos (2012).

Finally, we estimate a type of random-effects meta-regression where each effect size is weighted by its inverse variance. Since the canonical random-effects meta-regression (DerSimonian and Laird, 1986) assumes statistical independence among empirical estimates, we use a more recent method allowing for dependent effect sizes within the data-structure (i.e. multiple (correlated) effect sizes within studies). This approach is called „robust variance meta-regression with dependent effect sizes (Tanner-Smith and Tipton, 2014).

We are agnostic about which model may be the most “accurate” for this type of empirical literature so that we would like to provide the reader with a multitude of possible estimations. In some sense, the different models can be seen as possible upper and lower-bound estimates of the true empirical effect of financial education. The OLS-model places equal weights on each estimate and does not make any strong assumptions about the source of potential biases. It represents a description about the literature to date, without necessarily speaking to an estimate of a possible “true effect” of financial education in the broader set of possible studies. The unrestricted weighted least squares (WLS) models place extreme weight on larger studies, since these minimize the standard errors and variance of the estimate while assuming that each estimate relates to a single true effect. Thus, estimates from this family of models (and especially those accounting for potential publication selection bias, PET-PEESE) may server as a conservative lower-bound of the true effect of financial education. Finally, robust-variance estimation with dependent effect sizes explicitly models between-study heterogeneity in addition to within-study measurement error. As a consequence, smaller studies are not as strongly discounted as in the WLS-approach, since within-study measurement error is only one source of variance. This approach may be seen as an estimation of upper-bound effects in the universe of potential financial education impact evaluation studies in the presence of excess heterogeneity between studies.

In addition to estimating the average effect of financial education treatment, we are interested in exploring the determinants of effectiveness of programs reported across studies. Thus, we include observable characteristics in our meta-regression models and investigate whether these covariates may explain some of the documented variation in the literature. For these analyses, we rely on what may be considered the most conservative approach (WLS / PEESE).

3 Data

The application of the reported selection criteria (see Section 2) leads to a sample of 37 independent (quasi-) experimental studies in schools reported in 35 papers published between 1978 and 2018 (these studies are listed in Appendix A). The majority of papers has been published in recent years, 20 out of 37 since 2015. Out of these 37 studies, 18 are randomized experiments (RCTs) and 19 are quasi-experimental studies that employ a non-randomly selected control group. A description about the publication year of these two study types, i.e. either RCT or quasi-experimental studies, is provided in Figure 1. It becomes immediately obvious that RCTs are conducted more recently and dominate this literature since 2015 (with a 75% share of studies).

< Figure 1 about here >

From these studies, we code a total of 177 effect size estimates, because individual studies typically look at multiple outcomes, measure outcomes at multiple time points, or include separate effect size estimates for different school grades[3]. In our sample, RCTs appear to report more estimates per paper, since the 18 RCTs account for 135 estimates in our sample. The 19 quasi-experiments, in contrast, contribute 42 effect size estimates to our analysis.

[3] Note that, while this may give rise to concerns regarding deflated p-values due to multiple-hypothesis testing, there is not enough studies applying a correction to the estimated standard errors (e.g. the family wise error rate, FWER) to systematically study this in our meta-regression models.

With regard to outcome types, we consider two main families of outcomes: (i) treatment effects on *financial knowledge* (i.e. performance on a standardized financial knowledge test), and (ii) treatment effects on *financial behaviors* and their antecedents (for example an increase in savings or an observed financial decision in an incentivized experimental task). Not all of the included studies report treatment effect on both outcome families: The dataset includes information from 31 studies (70 effect size estimates) on *financial knowledge*. Out of these 14 are RCTs which report 41 effect size estimates. Information on impact on *financial behaviors* comes from 22 studies (107 effect size estimates). Out of these, 16 are RCTs and account for 94 effect size estimates. Thus, 16 studies report on both types of outcomes.

For each effect size estimate we code a number of characteristics in order to analyze later, in Section 4, potential determinants of effectiveness. These characteristics fall into four groups, i.e. (i) outcomes, (ii) research design, (iii) characteristics of the target group, and (iv) design elements of the education program. Regarding (i) outcomes, we either take them directly from the study or calculate ourselves Hedges' g values for each intervention, i.e. we extract 177 effect sizes. Their mean is 0.162 with a large standard deviation of 0.251 and thus not unexpectedly extreme values between -0.236 and 1.321 (see Table 1). Among all effects we distinguish between the outcome of financial knowledge and financial behavior. Within both categories we would also like to analyze sub-categories, but there is either not enough information on either the performance test being used to measure financial knowledge or there are too few observations (studies) to run models differentiating between different types of financial behaviors.

<Table 1 about here>

Regarding the (ii) research design we note, as mentioned already, whether the study is a RCT or a quasi-experiment and the standard errors of the effect sizes. Moreover, for 166 of 177 effect sizes, we know that average delay between treatment and measurement of potential effects is 17.6 weeks. Unfortunately, there is not enough information within these studies to differentiate between intention to treat effects (ITT) and the treatment-effect on the treated (TOT).

Coming to (iii) characteristics of the target group, studies provide information about school grades, so that we can group into elementary, middle and high school students, covering 21%, 49% and 27% of cases, respectively. However, many studies omit continuous measures of age, so that we can only include grades as a proxy of age. Also, information about gender-composition of the sample or the social status of parents (such as their income) is not always available. Finally, we code the country where the study takes place, but refrain from including this information into regressions because of the limited number of observations.

The last group of characteristics we cover are (iv) the design elements of the educational program. While we have information about the intensity of education, which is 21 hours on average, and, for a sub-sample of 138 effect size estimates, about the average class size of 26 students per class, there is a lack of systematic information regarding the content of curricula, the quality of materials and media such as textbooks, details about previous teacher training, and the teaching method employed (i.e. lecture or active learning). Thus, unfortunately, these latter characteristics cannot be considered in a quantitative meta-analysis as long as studies do not document enough detailed information to code these differences.

4 Results

We present results in three steps: first, main results are shown (Section 4.1), then the concern of publication selection bias is discussed (Section 4.2) and finally meta-regressions allow for investigating determinants of the effectiveness of financial education in schools (Section 4.3).

4.1 Summary effects

The summary effects of financial education in schools are estimated separately for the outcome types of financial knowledge and financial behavior. It is known from the literature (e.g. Kaiser and Menkhoff, 2017) and seems to be intuitive that educational effects on knowledge are higher than on behavior. This is indeed the finding from this meta-analysis as shown in Figures 2 and 3: the average effect (across all of the meta-analytic models estimated) on financial knowledge is about 0.25 standard deviation units (SDs), based on 70 effect size estimates from 30 studies, while the average effect on behavior is about 0.05 SDs, based on 107 effect size estimates from 22 studies.

<Figure 2 about here>

<Figure 3 about here>

Figures 2 and 3 provides further information. First, they demonstrate the same exercises for the subsample of RCTs only and shows tentatively smaller but still statistically significant effects. Second, it is reassuring that all the sixteen estimated average effect sizes, i.e. for two groups of outcomes times two samples (all studies and the sub-sample of RCTs) times four models each, are statistically highly significant and of similar magnitude irrespective of the meta-analytic model used. Thus, we conclude that the positive impact of financial education in schools is very robust to changes in method, sample and the outcome measure.

Next, we compare these effect sizes to earlier findings in the literature and to effect sizes realized in interventions from other educational domains. In the financial education literature, Fernandes et al. (2014) are the first to apply a quantitative meta-analysis. They arrive at small (weighted) average effect sizes on financial behavior for interventions (about g=0.02 for 15 estimates from RCTs and about g=0.07 including 75 estimates from quasi-experiments).[4] Thus, the effect on behavior among students is still higher or at least near identical to the findings from the limited number of early experiments on adults.

While not explicitly investigating the effect of financial knowledge, Fernandes et al. (2014) state that 12 papers in their sample report an average effect of about 0.13 SD units.[5] This result, however, is an obvious contrast to the results of our meta-analysis on students where the estimated average effect is about twice as large. Thus, the assertion that "[…] financial education yields surprisingly weak changes in financial knowledge […]" (Fernandes et al., 2014, p.1867) does not hold in this sample of studies on youth and may be seen as a particular result of the sample studied by Fernandes et al. (2014).

The second meta-analysis in the (adult) financial education literature uses a slightly different approach comparing only studies that measure effects on identical outcomes (Miller et al., 2015). This study does not quantify effects on financial knowledge but provides estimates on various financial behaviors reported in studies on adult financial education programs.

Third, the most-recent literature covering the largest number of interventions provides evidence of an average effect of about 0.2 SD units on financial knowledge, and about 0.09 on financial behaviors in a sample including many studies on adult financial education programs (Kaiser and Menkhoff, 2017). Thus, effect sizes on financial knowledge appear to be larger for programs that focus exclusively on children and youth than for adults. In

[4] Note, that Fernandes et al. (2014) use partial correlations (r) as their effect sizes measure. We transform these to standardized mean differences $d = \frac{2r}{\sqrt{1-r^2}}$ and apply the bias correction factor to arrive at (g) ex post.

[5] See Fernandes et al. (2014), p. 1867: " In 12 papers reporting effects of interventions on both measured literacy (knowledge) and some downstream financial behavior, the interventions explained only 0.44% of the variance in financial knowledge.", i.e. $\sqrt{r^2} = 0.066$.

contrast, effect sizes that measure changes in financial behavior appear to be slightly smaller for children and youth than for adults.

How do these effect sizes compare to learning that takes place in other domains? Comparing effect sizes across disciplines and research questions is always difficult, however, there exist some normative and empirical benchmarks with regard to learning outcomes in school: Hill et al. (2008) provide examples of effect sizes on reading and mathematics achievement. They document typical knowledge gains from year to year in school (in the absence of a particular intervention), achievement gaps with regard to specific subgroups, as well as a summary of effect sizes realized by interventions in these domains. If one compares their descriptive evidence to the result of our synthesis, financial education has near identical effect sizes, on average, as reported in 76 meta-analyses of various educational interventions (cf. Hill et al., 2008, p.176). Thus, again, the conclusion drawn by Fernandes et al. (2014, p.1867) that "[b]y comparison, meta-analyses in other domains of education show interventions explain 5 to 13 times as much variance in acquired knowledge […]" does not hold for our estimate of the average effect of financial education compared to data from meta-meta-analyses on educational interventions provided by Hill et al. (2008).

To make another empirical comparison: The average effect size realized by financial education appears to be of similar magnitude as the estimated increase in learning in mathematics in the transition from grade 9 to 10 or of similar size as the increase in reading achievement occurring in the transition from grade 7 to 8 (Hill et al., 2008, p.173). Thus, one could argue that these knowledge gains are indeed of high practical significance.

4.2 Publication selection bias

Publication selection bias refers to the potential behavior of researchers and journal editors to favor statistically significant results and not reporting estimates which do not pass tests for significance. Given a single true empirical effect (which may be questioned due to the heterogeneity of treatments), the standard error of this estimate should be orthogonal to the reported effect sizes in a given literature. If this is not the case, we observe so-called funnel asymmetry. This tendency to underreport "undesired" estimates with large standard errors (especially in small studies) can lead to a biased assessment of the (weighted) average effect of a given literature. In the following, we test whether such a mechanism can be observed in the literature on school financial education.

In the presence of “publication selection”, researchers and editors may favor the publication of empirical estimates that pass tests for conventional levels of statistical significance. When such a mechanism is present, the reported effect is (ceteris paribus) correlated with its standard error (Stanley and Doucouliagos, 2012, p.61). The intuition of this method is to “correct” the estimate of the average empirical effect (the intercept of a given meta-regression model). In order to arrive at an estimate of a genuine empirical effect Stanley and Doucouliagos (2012) suggest including the standard error (PET) or the variance (PEESE) as a predictor of effect sizes and estimate the model by employing an unrestricted weighted least squares procedure using inverse variance weights.

Table 2 shows results from these tests for publication selection bias and its correction. Column 1 of Table 2 shows the unadjusted (weighted average) effect on financial knowledge. In the next step, column 2 introduces the standard error of each estimate as a regressor (funnel asymmetry test (FAT) and precision effect testing (PET)) and indicates funnel asymmetry regarding the reported effects on financial knowledge. Thus, column 3 applies the correction proposed by Stanley and Doucouliagos (2012) and includes the variance of each estimate and weighting each effect size estimate with its inverse variance. Applying this correction still leads to a statistically highly significant estimated (weighted) average effect of financial education on financial knowledge (0.147 SDs). Turning to effect sizes on financial behavior, column (4) repeats the WLS result from Table 2 for comparison. The PET estimate (column 5) suggests that there may be no empirical effect (just selection) while the PEESE estimate arrives at a significant effect of still about 0.036 SD units.

<Table 2 about here>

We conclude from these examinations, which follow a standard approach in the literature, that even when correcting for potential publication selection bias, the positive effects on financial knowledge remain statistically and economically significant. The small average positive effect on financial behavior, however, is less certain – as already suggested by the small effect size estimated in the other meta-analysis models.

4.3 Explaining heterogeneity in treatment effects

In the following we show results of meta-regressions considering additional covariates. These regressions “explain” the average effect size of defined interventions realized in a specific group of studies by a set of observable study characteristics included as RHS variables. In the case of assessing the effectiveness of financial education, these variables represent three different kinds of influences on effectiveness, i.e. (i) research design, (ii) characteristics of the target group, such as age of pupils, and (iii) elements of the educational intervention. As discussed in Section 3 above, available studies either do not report all variables we are interested in or do not provide enough variation in our limited sample of studies, so that the group of variables to be considered is to a large extent driven by data availability and limitations in the degrees of freedom for the estimation of these models.

Meta-regressions models. In the following we first report results on explaining the effect sizes of financial knowledge before we turn to explaining financial behavior. In the first regression, i.e. column (1) in Table 3, we model effect sizes on financial knowledge as a function of intensity while controlling for the research design (RCT) and applying the correction for potential publication selection bias (SE^2).

<Table 3 about here>

We find that more intensive interventions lead to higher effect sizes on financial knowledge (+0.4% of a standard deviation per additional hour of instruction), albeit with decreasing marginal returns to increased intensity. In line with our earlier results, we find that RCTs report smaller estimated effect sizes, on average. Next, we consider the delay between treatment and measurement of outcomes, since effects of the intervention may fade out over time. Column (2) of Table 3 shows this relationship: Effect sizes on financial knowledge seem to decay at a rate of approximately 0.07 percent of a standard deviation per week. Next, we consider whether treatment effects may depend on the size of the class (i.e. the student to teacher ratio). Column (3) of Table 3 shows an estimation result including class size as an additional predictor. Note, that the sample size is reduced substantially, since few studies report this key detail of the financial education program. While all standard errors increase, it is noteworthy, that the point estimate is negative, i.e. possibly yielding support to the hypothesis that an increase of class size is associated with a decrease in impact on financial knowledge. Finally, we turn to average differences between interventions targeted at different age groups. In column (4) of Table 3 we compare interventions in middle- and high school to interventions in primary school (omitted category) while controlling for research design, intensity of the treatment and delay of measurement. We exclude class-size in this estimation, since this would result in a substantially reduced sample. We find that studies reporting on interventions in primary school appear to show substantially larger effect sizes than those in middle school, while the difference to high schools is estimated to be smaller (and statistically insignificant). Overall our models explain between 20 and 42 percent of the variance in the reported effects in the literature. Thus, while we cover many of the key differences that may explain the heterogeneity in findings, there is still unexplained heterogeneity that may be attributed to other variables.

Next, we study effects on financial behavior. Columns (5) to (8) of Table 3 mirror the models introduced above for the outcome of financial knowledge. Qualitatively, the results are similar to the models estimated in columns (1) to (4), however, at a lower magnitude of coefficients as the average effect on financial behaviors in schools is much smaller. Again, intensity may increase effectiveness with decreasing marginal returns (column 1) and, again, interventions in primary school are estimated to show larger effect sizes than those in middle school, while the difference to high schools is insignificant. In detail, however, some differences arise: First, the models cannot estimate with certainty whether effects on financial behavior decay over time (see column 6). Second, class size may be a stronger predictor of effectiveness of interventions (column 7), thus reflecting the intuition that personalization - which is potentially realized in smaller classrooms with lower student to teacher ratios - may be important to achieve behavior change (e.g. Carpena et al., 2017). Note, that the heterogeneity in the estimated effects on financial behaviors is explained by our models to a lesser extent than the effects on financial knowledge, since these account for about 6 to 12 percent of the variance in effect sizes.

Analyzing marginal effects of design-relevant variables. On the basis of the models described above, we look at marginal effects from policy-relevant variables that may inform the design of financial education programs. First, we show how the effect size changes depending on an increase of intensity of the treatment, while keeping all other variables at their mean.

<Figure 4 about here >

It has been hypothesized by Miller et al. (2015) and has been shown for our sample above that the effect increases with intensity and that this increase declines with intensity, reflecting declining marginal returns of education. Figure 4 shows the plot for our data, where the left panel shows effects on financial knowledge and the right panel shows effects on financial behaviors. The predicted values indicate, that on average one needs about 5 hours of training to create an effect of about 0.15 standard deviation units and that 80 hours may indicate an order of magnitude where additional teaching on given subjects does not create measurable additional value. Put differently, a significant effect size of 0.35 is realized after only about 30 hours of teaching while this minimum threshold does not really increase thereafter. This signals cautiously that short trainings gain strongly from increasing intensity up to about 30 hours (with a range of 20-40) but that more input does not create respective value anymore. Turning to effects on financial behavior, we find that even brief one-hour interventions result in effects statistically different from zero, and that increasing effectiveness by means of more intensity is more difficult than for the case of improving financial knowledge.

Next, we study another marginal effect, i.e. the effect of delayed measurement on the estimated size of the treatment effect. It may be expected that typically learning effects decay over time because people forget what they had learnt. This has been shown in the context of financial education by Fernandes et al. (2014) and we demonstrate this effect for our sample of studies covering only financial education in schools. Figure 5 shows this effect graphically (while keeping all other variables constant at their mean). With a delay of 30-40 weeks the average effect of financial education on financial knowledge and behavior is estimated to be statistically insignificant (the 95% confidence interval is estimated to include zero). Thus, we can say with confidence, that effects of the average program with approximately 20 hours of teaching intensity, may be observable at least up to 7 months after the intervention took place. Note, that the long-term effect of financial education is uncertain as very few studies report on long time horizons after the treatment. Thus, the 95% confidence intervals at long delays (i.e. over 30 to 40 weeks) can neither rule zero-effects nor an increase of effectiveness at longer time horizons, i.e. the "long-term" effect on financial behavior may be effectively zero or over 10 percent of a standard deviation (0.1 SDs) after 80 or more weeks. Unless the literature provides more long-term assessments of financial education programs, this relationship will remain to be uncertain.

<Figure 5 about here >

Finally, we study the relationship between the student to teacher ratio (class size) and estimated treatment effects. Figure 6 shows the predicted margins at average intensity and average delay. Effect sizes on financial knowledge do not appear to strongly depend on class size in our sample of school financial education evaluations. While effects on financial knowledge are estimated to be larger in smaller groups of students (e.g. 0.2 SD units in classes of five students) the effect is reduced only by about 20% when teaching students in classrooms that are six times as large (e.g. 0.16 SD at a class size of about 30 students). Additionally, the standard errors for the effects realized in small classes is relatively large.

<Figure 6 about here >

Turning to treatment effects on financial behaviors, however, shows that a smaller class size may be a strong predictor of effective financial education. While the effect on behavior is very small (about 0.03 SD units) at class sizes of 20 or more pupils, the effects point estimate increases to 0.12 SD units at class size of 10 and to even 0.20 SD units at class size of 5. This result is in line with the literature on the effects of smaller classes in general, that documents positive effects from reduced class sizes, especially for low-income and minority students (e.g. Finn et al., 1990; Krueger and Whitmore, 2001; Chetty et al., 2011). Thus, teaching within smaller classrooms could be a very effective measure when it comes to impacting financial behavior, however, two caveats are in place: first, our estimations are based on a relatively small sample of studies, thus, the confidence intervals for effects in smaller classes include the possibility of a zero effect of decreased class size on behavior (as can be seen from Figure 6), and second, reducing class size (i.e. lowering the student to teacher ratio) may be a costly endeavor. Unfortunately, not many studies provide information about the costs of the programs studied.

5 Policy conclusions

We present our conclusions in the following from a policy perspective: What can policy makers learn from the meta-analysis being presented so far, which elements could be integrated into an effective program, which elements may be added beyond the scope of this study, and what does this imply for the discussion of principal alternatives?

Meta-analysis lessons. The main lesson is that financial education seems to be quite successful in increasing financial knowledge among school students. This result is robust irrespective of the meta-analytic model and whether or not one accounts for potential publication selection bias in the financial education literature. In particular, and this directly addresses earlier concerns, financial education in schools has a statistically and economically significant effect also when the most rigorous type of impact evaluation design is conducted, i.e. in the sub-sample of RCTs. This also holds if the intended outcome is a change in financial behavior, however, the degree of effectiveness is much smaller. When compared to all kinds of financial education (whether in schools or not), the effects on knowledge are rather higher while those on behavior tend to be relatively smaller (Kaiser and Menkhoff, 2017).

Design elements of an effective school financial education program. There are some ex-ante expectations on the determinants of effectiveness which can only be partly tested here due to a lack of better data. The two determinants where enough information is available are the grade (elementary vs. middle school vs. high school) and the intensity of education. While we find that effectiveness is highest at elementary schools, this does not imply that financial education should necessarily be limited to these early ages. The implication of this result is in our view that younger pupils learn more than older ones because they know less, so that there is no specific implication for the case of financial education. Also, regarding the positive impact of increased intensity, the consequence is not as straight forward as it may look like, i.e. to make programs as comprehensive as possible. We rather suggest thinking about a format with limited content that is taught for about 20 to 40 hours which translates into roughly two teaching hours per a half year of schooling. Finally, it seems advisable to think about reducing class sizes when changes in financial behavior are focused, although we would need more research in this respect to be sure about a recommendation and the cost-effectiveness of such an approach. In addition, there are further insights from the general literature on financial education programs which may be applied to schools as well.

One important element which could not been tested here is the impact of a so-called 'teachable moment'. It has been shown for studies covering such an effect that the additional positive impact may be in the order of 0.05 to 0.07 standard deviations and thus quite sizable (Kaiser and Menkhoff, 2017). This suggests considering proper teachable moments during the process of life-long financial education. Moreover, there is evidence that education that is more entertaining or personalized has more impact on financial behavior (Berg and Zia, 2017; Carpena et al., 2017). Finally, it appears that those programs that employ design elements resembling 'active learning' (e.g. simulations and experimental learning) may yield higher effect sizes (see Amagir et al., 2018; Kaiser and Menkhoff, 2018). All these are elements which may contribute to increasing effectiveness of financial education.

Assessment relative to alternatives. The finding of successful financial education in schools is a necessary but not sufficient condition that respective programs should be implemented. Opposing positions either emphasize to regulate the financial sector in a way that financial education becomes less necessary or favor a more general education in mathematics or statistics over more narrow financial education (see Brown et al., 2016). Regarding the first opposing position it seems questionable whether all financial decisions can be or should be addressed by regulation. Regarding the second opposing position, the argument against financial education can be also reversed as recent research shows that financial education can have significantly positive externalities, such as positive effects on the financial knowledge of parents (Bruhn et al., 2016) and of teachers (Frisancho, 2018), and on the cumulative GPA of students (Frisancho, 2018). Additionally, recent experiments show that financial education has an impact on intertemporal decision making among children and youth, leading to more consistent and more patient intertemporal choices (see Migheli and Moscarola, 2017; Alan and Ertac, 2018; Bover et al., 2018; Frisancho, 2018; Lührmann et al., 2018). Thus, financial education provided early in the life-cycle may have beneficial outcomes with regard to debt taking or long-term savings and may reach even beyond the financial domain. Thus, financial education improves the understanding of financial affairs but seems to have broader welfare implications, similar to other forms of education.

Overall, academic research alone cannot answer the policy question whether financial education in schools should be introduced at all or the extent to which it should be developed. What can be said, however, given the current knowledge, is that financial education is as effective as education is regarding other school subjects and that basics of financial education can be taught in about 20 to 40 hours. Despite this encouraging situation, we want to emphasize that more could be done in order to increase effectiveness of financial education and that more thorough documentation of such efforts within empirical studies would be crucial to gain deeper insights in future surveys or meta-analyses.

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Figure 1. Number of included studies by research design per year

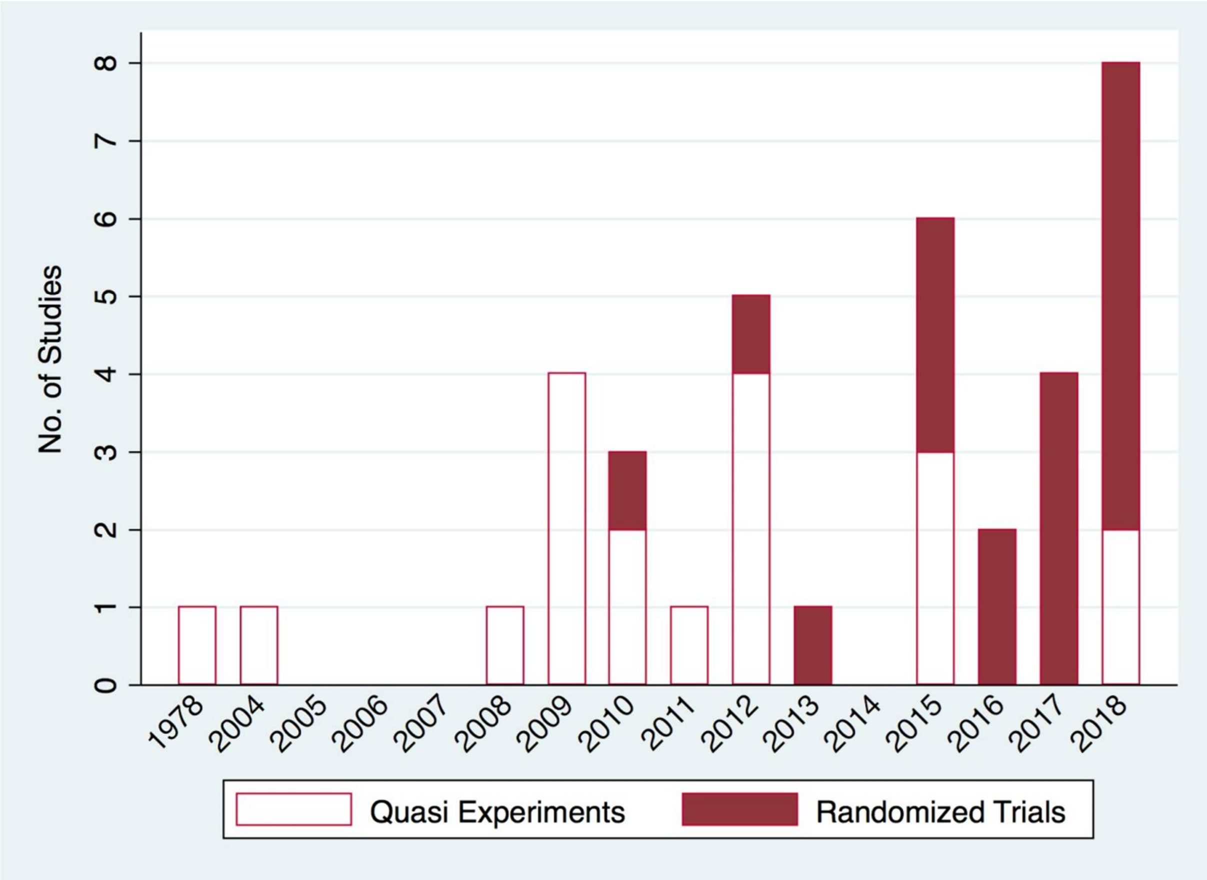
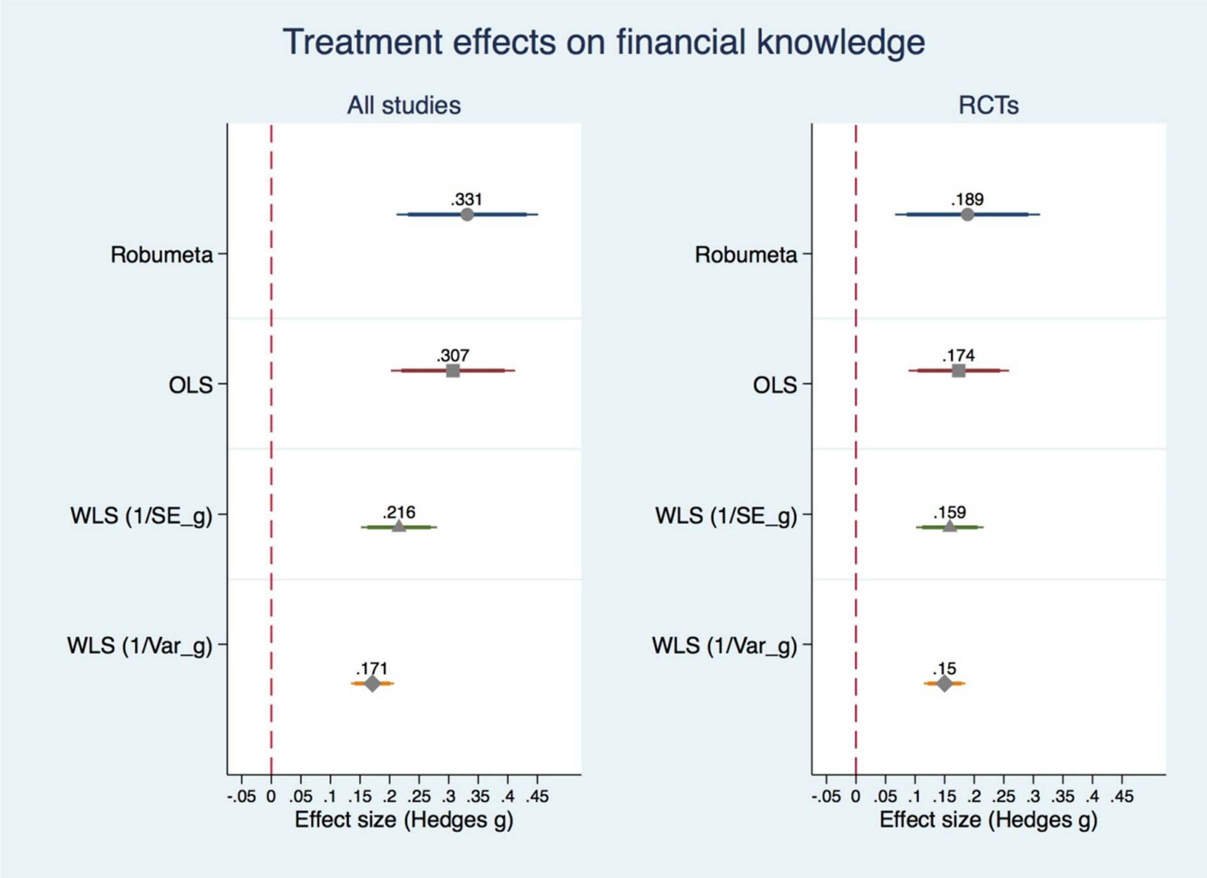
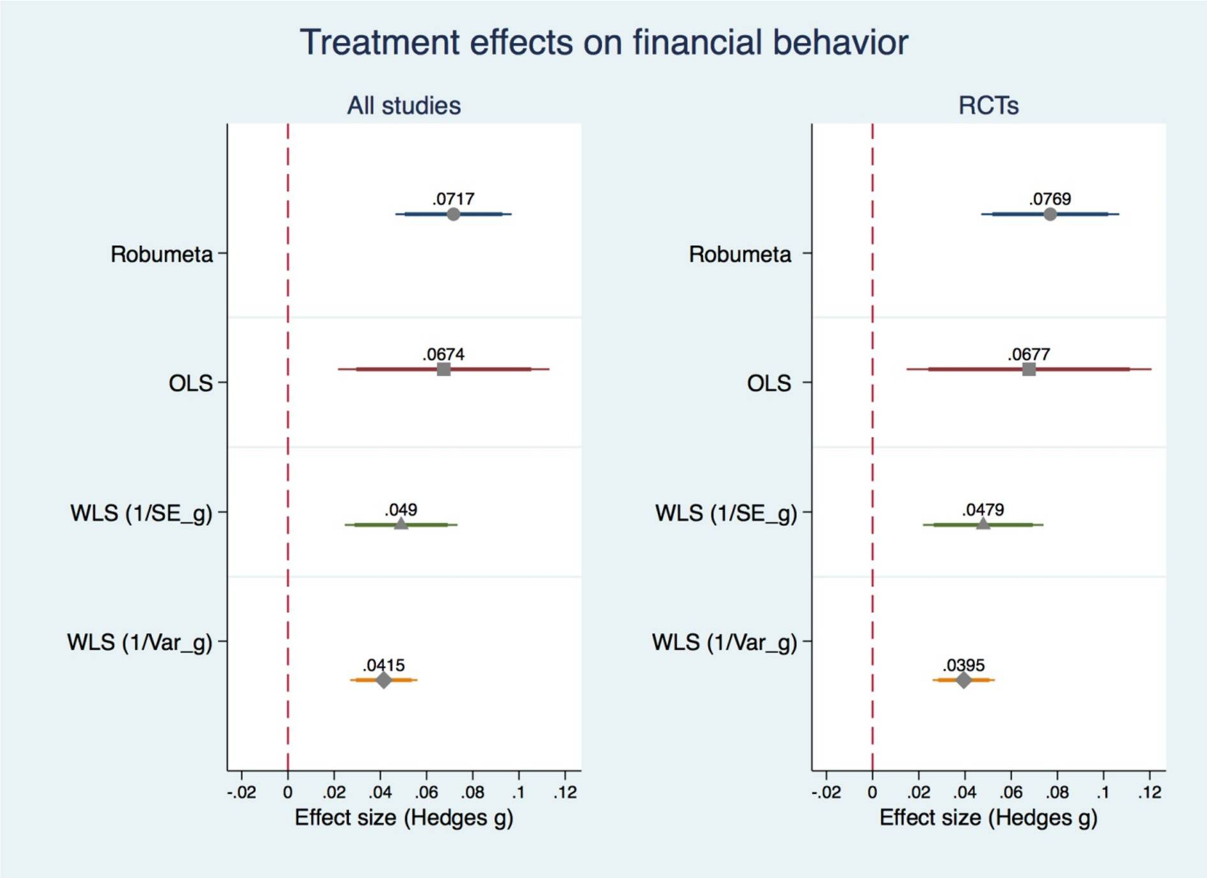


Figure 2: Summary of treatment effects on financial knowledge



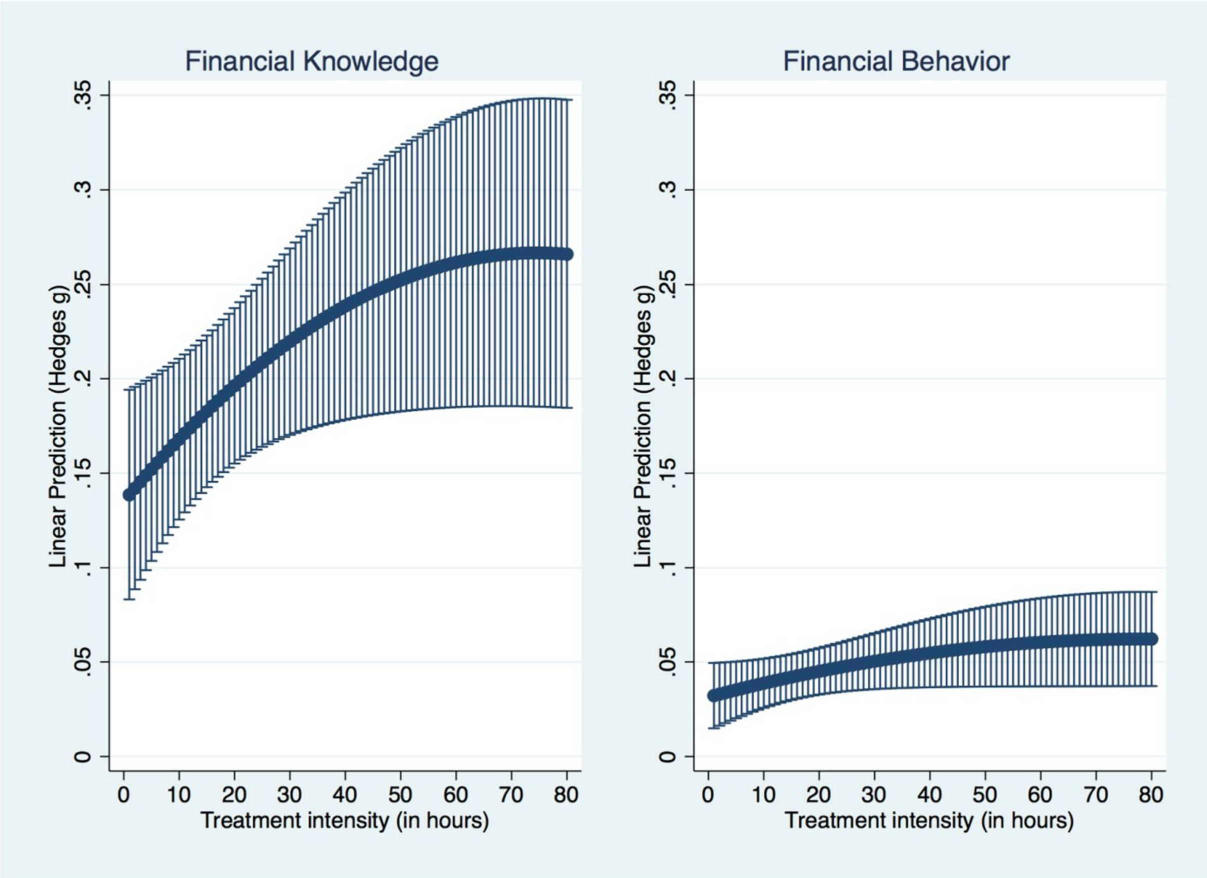
Notes: The figure shows the (weighted) average effects and 90% and 95% Cis estimated by the different meta-analysis models. Number of observations for all (31) studies is n=70 effect size estimates. Number of observations for the 14 RCTs is n=41 effect size estimates. The average effect (across the four models) is g=0.257 (all studies) and g=0.168 for RCTs.

Figure 3: Summary of treatment effects on financial behavior



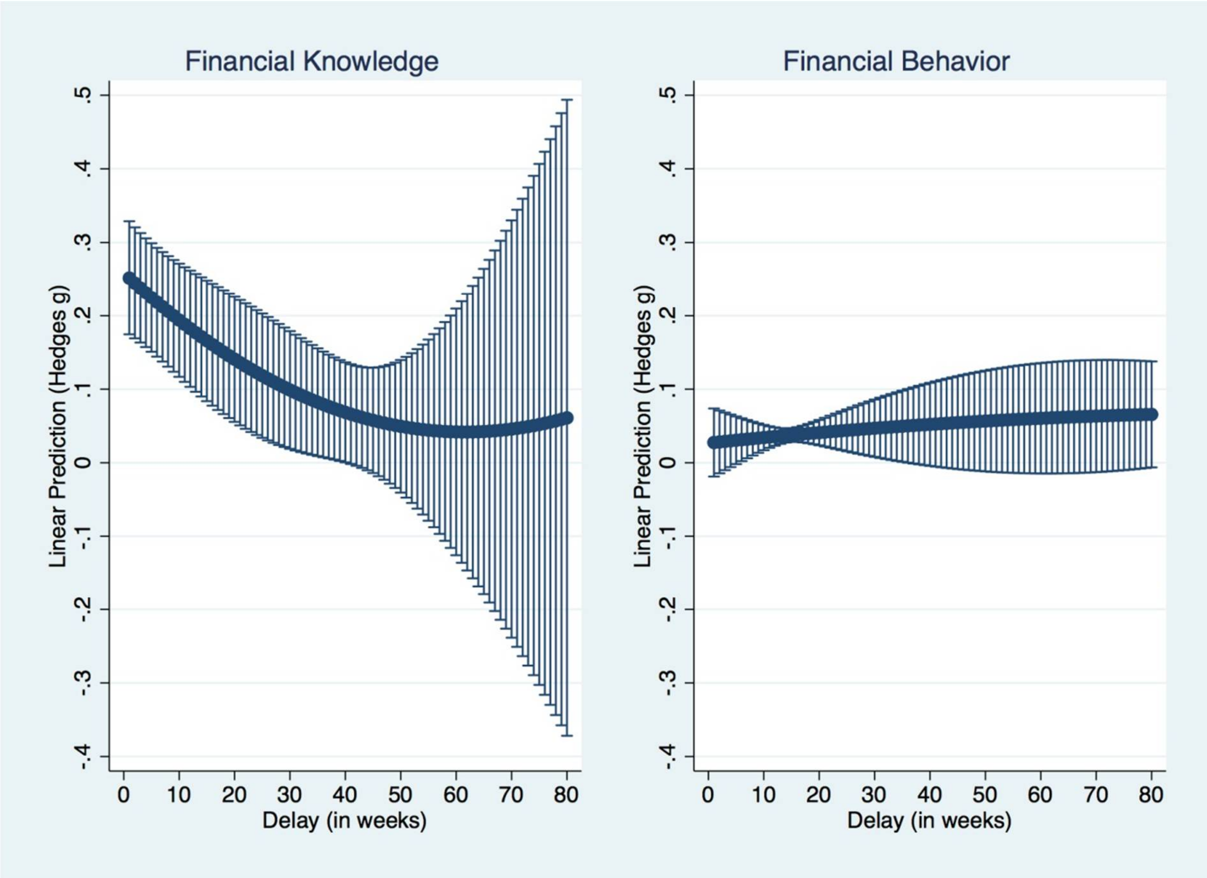
Notes: The figure shows the (weighted) average effects and 90% and 95% Cis estimated by the different meta-analysis models. Number of observations for all (22) studies is n=107 effect size estimates. Number of observations for the 16 RCTs is n=94 effect size estimates. The average effect (across the four models) is g=0.057 (all studies) and g=0.058 for RCTs.

Figure 4: Decreasing marginal returns to increased intensity



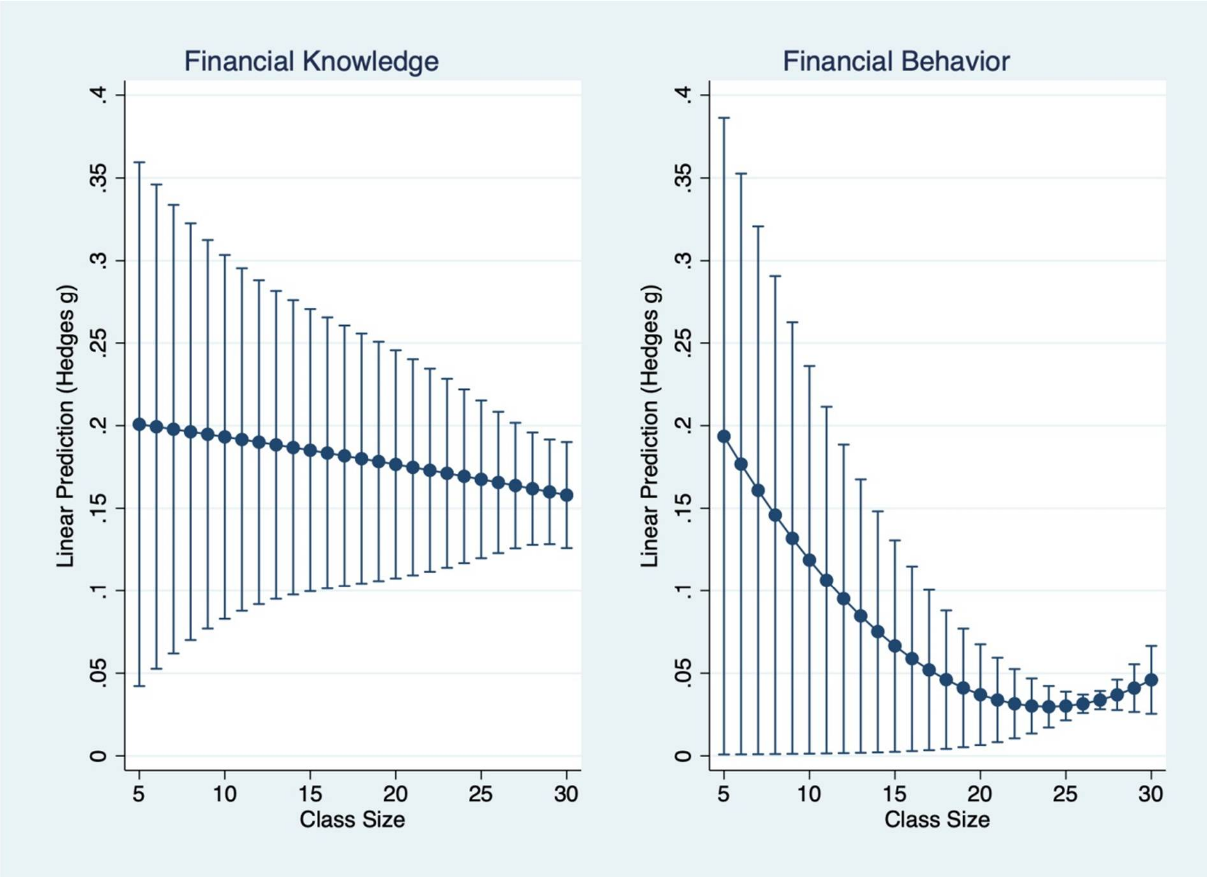
Notes: These figures show the effect size of financial education treatments as a function of treatment intensity (see Columns (1) and (5) of Table 3). Intensity is measured in hours. The shaded areas cover the 95% confidence upper- and lower bounds.

Figure 5: Decreasing effect size with increasing delay of measurement



Notes: These figures show the effect size of financial education treatments as a function of delay between treatment and measurement of outcomes (at average empirical intensity, see Columns (2) and (6) of Table 3). Delay is measured in weeks. The shaded areas cover the 95% confidence upper- and lower bounds.

Figure 6: Relationship between treatment effects and class size



Notes: These figures show the effect size of financial education treatments as a function of class size (at average intensity and delay, see Columns (3) and (7) of Table 3). Class size is defined as the number of students within treated classrooms. Bars show the 95% confidence upper- and lower bounds.

Table 1. Descriptive statistics at the estimate-level

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Hedges g	177	0.162	0.252	-0.236	1.321
SE	177	0.065	0.059	0.013	0.372
SE^2	177	0.008	0.016	0.000	0.139
RCT	177	0.763	---	0	1
Delay	166	17.63	30.620	0	132.675
Intensity	174	20.64	36.536	0	150
Elementary school	177	0.239	---	0	1
Middle school	177	0.490	---	0	1
High school	177	0.271	---	0	1
Class size	138	26.158	7.323	7	35

Table 2: Testing for publication selection bias

	(1) FL WLS	(2) FL FAT-PET	(3) FL PEESE	(4) FB Unadjusted	(5) FB FAT-PET	(6) FB PEESE
SE		2.493***			1.067	
		(0.588)			(0.709)	
SE^2			11.136***			8.963
			(2.916)			(5.291)
Constant	0.216***	0.075**	0.147***	0.049***	0.017	0.036***
	(0.032)	(0.032)	(0.015)	(0.012)	(0.018)	(0.006)
R^2		0.235	0.136		0.094	0.038
n (Studies)	31	31	31	22	22	22
n (Effect sizes)	70	70	70	107	107	107

Notes: Dependent variable is effect size (Hedges g). Robust standard errors clustered at the study-level in parentheses. ***, **, and * denote significance at the one percent, five percent, and ten percent level.

Table 3: Explaining heterogeneity in effect sizes

	Dep. Var: ES on Financial Knowledge				Dep. Var: ES on Financial Behavior			
	(1) FL	(2) FL	(3) FL	(4) FL	(5) FB	(6) FB	(7) FB	(8) FB
RCT	-0.133*	-0.167**	-0.155	-0.146**	-0.039	-0.036	0.109**	-0.033
	(0.075)	(0.077)	(0.175)	(0.062)	(0.031)	(0.032)	(0.038)	(0.025)
Intensity	0.004**	0.006**	0.008	0.005	0.001*	0.000	-0.001	
	(0.001)	(0.002)	(0.005)	(0.003)	(0.000)	(0.001)	(0.001)	
Intensity× Intensity	-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)	
Delay		-0.007*	-0.011	-0.006*		0.001	0.002***	
		(0.004)	(0.008)	(0.004)		(0.002)	(0.001)	
Delay × Delay		0.000	0.000	0.000		-0.000	-0.000**	
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	
Class size			-0.001				-0.022*	
			(0.010)				(0.010)	
Class size × Class size			-0.000				0.000**	
			(0.000)				(0.000)	
Middle school				-0.226**				-0.067*
				(0.101)				(0.037)
High school				-0.181				-0.057
				(0.107)				(0.037)
SE^2	9.265**	8.463**	10.287**	4.983	8.605	7.622	5.175	4.855
	(3.371)	(3.584)	(4.177)	(3.504)	(5.424)	(5.589)	(9.279)	(6.567)
Constant	0.232***	0.292***	0.318***	0.485***	0.063*	0.061*	0.177	0.128**
	(0.072)	(0.069)	(0.092)	(0.114)	(0.031)	(0.033)	(0.143)	(0.050)
R^2	0.204	0.308	0.332	0.425	0.061	0.066	0.125	0.09
n (Studies)	30	28	19	28	22	21	15	22
n (Effect sizes)	67	60	40	60	107	106	89	107

Notes: Dependent variable is effect size (Hedges g). Robust standard errors clustered at the study-level in parentheses. ***, **, and * denote significance at the one percent, five percent, and ten percent level.

Appendix: List of studies included in the meta-analysis

Alan, S. and Ertac, S. (2018). Fostering Patience in the Classroom: Results from Randomized Educational Intervention. *Journal of Political Economy*, 126(5), 1865–1911.

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