CESifo WORKING PAPERS

11010
2024

March 2024

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CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editor: Clemens Fuest
https://www.cesifo.org/en/wp
An electronic version of the paper may be downloaded
· from the SSRN website: www.SSRN.com
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CESifo *Working Paper No. 11010*

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Abstract

Working memory capacity is a key component of executive functioning and is thought to play an important role for a wide range of cognitive and noncognitive skills such as fluid intelligence, math, reading, the inhibition of pre-potent impulses or more general self-regulation abilities. Because these abilities substantially affect individuals' life trajectories in terms of health, education, and earnings, the question of whether working memory (WM) training can improve them is of considerable importance. However, whether WM training leads to spillover effects on these other skills is contested. Here, we examine the causal impact of WM training embedded in regular school teaching by a randomized educational intervention involving a sample of 6–7 years old first graders. We find substantial immediate and lasting gains in working memory capacity. In addition, we document positive spillover effects on geometry, Raven's fluid IQ measure, and the ability to inhibit pre-potent impulses. Moreover, these spillover effects emerge over time and only become fully visible after 12–13 months. Finally, we document that three years after the intervention the children who received training have a roughly 16 percentage points higher probability of entering the academic track in secondary school.

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January 9, 2024

I. Introduction

Cognitive and noncognitive skills affect important individual life outcomes such as health, education, and earnings (Cunha et al. 2006; Heckman, Stixrud and Urzua 2006; Moffitt et al. 2011; Duckworth et al. 2012; Almond, Currie and Duque 2018). Executive functions (Diamond 2013), which are malleable through interventions in childhood with long-lasting effects into adulthood (see, e.g., Walker et al. 2022; García, Heckman and Ronda 2023), are thought to play a key role in a wide range of abilities. Working memory (WM) capacity — the ability to mentally store and process information (Baddeley 1999) — is a key component of executive functions and has been shown to be positively associated with math and language skills (Gathercole et al. 2004; Alloway and Alloway 2010), general fluid IQ (Kyllonen and Christal 1990; Ackerman, Beier and Boyle 2005; Oberauer et al. 2005; de Abreu, Conway and Gathercole 2010), and self-regulation skills such as attention and inhibitory ability (Engle 2002; Hofmann et al. 2008; Schmeichel, Volokhov and Dernaree 2008; Diamond and Ling 2020). Conversely, individuals with learning problems, self-regulation and attention deficits often have low WM capacity (Westerberg et al. 2004; Martinussen et al. 2005; Van Snellenberg et al. 2016). In view of this relevance of EFs and WM capacity for many important skills, the question is whether one can *simultaneously* improve several of these skills through WM training and whether this can be achieved by introducing WM training into the school curriculum. These questions are of fundamental importance for human capital formation and its underlying mechanisms as well as for educational policy.

Previous evidence suggests that WM training can improve performance on untrained WM tasks (direct effects). However, the question of whether training-induced improvements in WM capacity lead to improvements in other important skills, such as academic and self-regulatory skills (spillover effects), lacks a conclusive answer as even meta-analyses and review studies are controversial on this point (Shipstead, Hicks and Engle 2012; Karbach and Verhaeghen 2014; Au et al. 2015; Melby-Lervag, Redick and Hulme 2016; Aksayli, Sala and Gobet 2019; Sala et al. 2019). This lack of a conclusive answer suggests that WM training studies face a number of considerable challenges (see, e.g., Gobet and Sala 2023 and Greene et al. 2019). For example, (i) spillover effects are likely to need time to evolve and identifying these effects requires follow-up evaluations that go beyond just a few weeks or 3–4 months after the training, (ii) unobservable background variation in school environments may swamp potential treatment effects, (iii) training may only lead to spillover effects in specific subject pools such as young children. Other difficulties involve (iv) choosing an appropriate control group, (v) using or developing appropriate age-adjusted outcome measures, and (vi) sample size issues.

We tackle these challenges with a randomized controlled field experiment—described in more detail below—in a sample of 572 typically developing school children in the first grade of primary school. We focus on the training of relatively young children at age 6–7 years because evidence from economics indicates that training programs for youths in their late adolescence or young adulthood may be less effective than for young children (Cunha et al. 2006; Heckman 2006). Young children have higher brain plasticity, which might increase the chances of generating positive spillover effects (Heckman 2006; Constantinidis and Klingberg 2016; Klingberg 2016; Almond, Currie and Duque 2018). In contrast to most other WM training studies in typically developing children, we track children's outcomes for longer than 3–5 months after the training.

Specifically, we measure outcomes also after 6 and after 12–13 months and we examine whether the training has an effect on children's school trajectory three years later.

In our study, 31 school classes were randomly assigned to a treatment group (15 classes) or a control group (16 classes). Since we randomized within schools, we are able to control for unobservable background variation in school environments via school fixed effects. The children in the treatment group participated in a daily (one lesson per school day) computer-based adaptive WM training over a period of five weeks. We find not only substantial direct effects on WM capacity that emerge right after the five-week training period and last throughout all evaluation waves; we also find *spillover effects* on several important skills such as geometry, Raven's fluid IQ measure and children's ability to inhibit pre-potent impulses. Interestingly, for all these abilities there is no significant treatment effect shortly after the training, i.e., the spillover effects do not emerge in the short term. Instead, they show an increasing pattern over the course of several evaluation waves and are typically highest in the last wave (after 12-13 months) with effect sizes between 0.24 and 0.38 standard deviations. These effects are sizeable in view of the intervention's intensity (25 school hours).

One important aspect of our field experiment is that the WM training was embedded into the normal school routine and was introduced like any other new lesson or sequence of exercises that children experience during a school year. Thus, the children in the treatment group did not know that they were part of an experiment. The five-week WM training took place during one of the first two morning lessons during which children typically have math or German classes. This means that the children in the treatment group missed 25 school lessons relative to the children in the control group who participated in their normal math and German lessons. Our treatment effects therefore already incorporate the opportunity cost of the lost school lessons. This means that the children in the treatment group seem to have experienced a net benefit from the WM training because the training did not reduce any outcome measure but significantly improved the children's skill level in several dimensions. This interpretation is further corroborated by the finding that three years after the training the treatment group had a 16 percentage points higher probability of entering the academic track (called *Gymnasium*) of secondary school. In Germany, the choice of the secondary school track after the 4th grade in primary school is one of the most decisive educational choices for a child. This decision typically has a large influence on the probability of earning a high school (i.e., Gymnasium) degree and thus on the later university enrollment and adult labor market outcomes.[1]

Our paper is related to the literature on the role of children's cognitive and noncognitive skills in human capital formation. Research in this area has established that not only cognitive but also noncognitive skills have an important influence on individuals' life outcomes in terms of education, income, and health (Cunha et al. 2006; Heckman, Stixrud and Urzua 2006; Conti and Heckman 2010; Moffitt et al. 2011; Duckworth et al. 2012; Duckworth and Carlson 2013). Furthermore, research discussed in (Borghans et al. (2008), Cunha and Heckman (2009), Almond, Currie and Duque (2018); Garcia, Heckman and Ronda (2023) and Garcia and Heckman (2023)) has focused on the determinants of children's cognitive and noncognitive skills, and has

[1] Dustmann (2004) finds that individuals with a degree of the academic track of secondary school (*Gymnasiumabschluss*) earn on average 54-73% higher wages at labor market entrance than those with a lower secondary school degree (*Hauptschulabschluss,* earned after 9th grade), and 22-34% higher wages than individuals with an intermediate degree (*Realschulabschluss,* earned after 10th grade).

identified the early family environment and associated parental investments, the school environment, and early health shocks as important determinants of adolescent and adult human capital. In addition, researchers designed interventions to boost cognitive and noncognitive skills and have conducted randomized controlled trials to measure the interventions' causal effects. This literature examined, among others, the general role and malleability of (i) children's "growth mindset", i.e., an optimistic belief about the role of effort in individuals' success (Dweck 2006; Yeager et al. 2014; Sisk et al. 2018; Yeager et al. 2019), (ii) children's perseverance and patience (Duckworth et al. 2007; Duckworth 2011; Alan and Ertac 2018), and (iii) children's trust and social preferences (Cappelen et al. 2020; Kosse et al. 2020).

Our paper differs from these studies by focusing on different outcome measures and by choosing an intervention that has rarely, if at all, been considered by economists as a potential mechanism for changing children's cognitive and noncognitive skills: working memory (WM) capacity. WM capacity is a key component of executive functioning (Diamond 2013) – with inhibitory control and cognitive flexibility being the other two components – and comprises not just the ability to store information in the short term. The use of working memory also requires the ability to process information in the presence of distracting impulses and competing information that is not conducive for the individual's goal. Research on executive functioning has therefore emphasized that "working memory and inhibitory control need one another and co-occur", and that "working memory supports inhibitory control" (Diamond 2013, p. 143) This is the reason why WM capacity may also generate spillover effects on important noncognitive skills by facilitating impulse control and self-regulation.

The literature on WM training in typically developing children has mostly measured the impact of WM training only immediately after the training or a few weeks or months after the training. There are, however, reasons to believe that detecting spillover effects to more complex skills might require follow-up evaluations that leave more time for spillover effects to develop. Cunha and Heckman (Cunha and Heckman 2007; Cunha, Heckman and Schennach 2010), for example, have pioneered and provided supporting evidence for the view that higher skill levels at earlier stages positively affect skill formation at later stages due to 'self-productivity' (skills attained at one stage augment the skills attained at later stages) and 'dynamic complementarity' (skills produced at one stage raise the productivity of investment into skills at subsequent stages).[2] This is the reason why we evaluated outcomes not only shortly after the training but also 6 and 12–13 months after the training. Our findings on the time path of treatment effects corroborate the view that spillover effects need time to develop: in all cases in which we eventually document a significant spillover effect, the effect is rising over time, but in none of these cases the spillover effect is significant already shortly after the training. However, after 6 months a spillover effect on geometry skills and Raven's fluid IQ measure emerges (also visible after 12–13 months), and after 12–13 months we observe, in addition, a spillover effect on inhibitory control, namely the ability to inhibit pre-potent impulses.

[2] Several authors in the psychology and education science literature (Holmes, Gathercole and Dunning 2009; St Clair-Thompson et al. 2010; Nutley and Soderqvist 2017) have also pointed out that, while direct effects of WM training to untrained WM tasks may happen in the short run, training-induced improvements in WM capacity need time to affect spillover outcomes.

Our paper is also related to the literature in psychology and education science that examines whether EF and WM training interventions (and other forms of cognitive training) lead to spillover effects in children (for an early contribution, see Klingberg et al. (2005); for reviews, see Diamond (2013), Diamond and Ling (2020), Sala and Gobet (2023)). A relevant share of this literature focusses on disadvantaged children, e.g., with disorders, very low WM capacity, or from low-educated family backgrounds (e.g., Klingberg et al. (2005); Roberts et al. (2016)). For interventions targeting these disadvantaged children, several studies show strong positive long-term effects on EFs which also spill over to several other domains, such as health, education, and (reduced) crime (Walker et al. 2022; García and Heckman 2023; García, Heckman and Ronda 2023). Our paper instead focusses on typically developing children. We contribute to this literature by demonstrating positive WM training effects, showing that improvements in one EF domain (working memory) can create spillovers in other domains (inhibitory control), which is consistent with a foundational role of WM capacity for the dynamic process of skill formation (Cunha and Heckman 2007). Finally, we show that improvements in these domains can have causal, long-term effects on educational trajectories.

We believe that our approach has the advantages that (i) the children in the control group are participating in their normal school lessons, i.e., we have a natural control group, (ii) the children in our study are not aware of being part of an experiment because the training was introduced like other new topics during normal school teaching, (iii) we can also examine a question of high policy relevance, namely whether WM training provides additional benefits or costs for the children relative to normal school lessons, and (iv) we have short- and longer-run outcome measures that enable us to study how the treatment effect evolves over time. To our knowledge, there are only two other studies (St Clair-Thompson et al. 2010; Rode et al. 2014) that implemented WM training into the normal school routine such that the effects of training relative to normal school lessons could have been assessed. Unfortunately, these two studies experienced large attrition already after a few months, and/or did not have long-term follow-up measurements. In the light of our finding that many treatment effects only become fully visible after many months, this may have severely limited their ability to discover spillover effects.[3]

The rest of the paper is organized as follows: Section II describes our study design, the data collection, and our outcome measures. In addition, we put forward conjectures about the effect of WM training on our outcome measures. In Section III, we describe the estimation method. In Section IV, we present and discuss our empirical results in detail. Section V summarizes the results and concludes the paper.

[3] There are also a number of studies that implement randomized WM training for children *outside* the school context (see review by Sala and Gobet (2020)), i.e., the children know that they are part of a study. Most of these papers measure outcomes between a few weeks and three months after the experiment.

II. Study Design and Data Description

The field experiment was conducted in primary schools in Mainz, Germany, in 2013/2014 after receiving ethical approval in September 2012.

A. Participants

With the aid of the school authorities, we recruited 31 first grade classes from numerous schools in the city of Mainz, Germany, for participation in the study. Each school participated with at least two classes. Out of 599 children in these classes in November 2012, we received the consent from 580 parents (consent rate of 96.8%) for four waves of data collection (W1, W2, W3, W4). We were able to collect test data for 572 of these 580 children at baseline (W1) and shortly (i.e., 4-5 weeks) after the training (W2).[4] Randomization was done between classes and within schools: 15 classes (279 children, i.e., 49%) were randomly assigned to the treatment group and 16 classes (293 children) to the control group. Randomization occurred within schools enabling us to control for school fixed effects. Summary statistics are reported in Table 1 below. About 49% of the children were male, mean age at the beginning of the year (i.e., on January 1, 2013) was 82 months (6.8 years, SD = 4.3 months). Attrition over the course of the four evaluation waves (from W1 to W4) was very low (only about 7%, with no difference between treatment and control group, see Online Appendix Section 1.1).

B. Treatment and Control Condition

The treatment consisted of a daily WM training session lasting approximately 30 minutes, taking place during the first or second lesson of a school day over a period of 25 consecutive school days. The WM training was embedded into the classes' normal school routine. Accordingly, parental consent on their children's participation in the training was not required, and thus all children in the treatment classes participated in the training. In each class, a single teacher covers almost all the topics that need to be taught according to the first-grade curriculum. Thus, the WM training was introduced to the children as a normal sequence of exercises by this teacher, similar to when the teacher introduces a new sequence of exercises for math, reading, or writing as required by the curriculum. Accordingly, the teacher was present during the lessons when the WM training took place, children remained in "their" classroom, and they conducted the training sessions at their usual desks. This minimizes Hawthorne or demand effects because it ensures that the children viewed the WM training simply as a usual topic of their curriculum, in which the sequential introduction of new learning content during the school year is part of normal school routine. In addition, we did not inform parents about the treatment assignment of their children, and we also did not provide information that would have enabled them to infer the treatment assignment.[5]

[4] Six children completed the W1 tests slightly after the actual start of the WM training (two of them in the control group) because they were sick or absent at the original test date. Since the delays were rather small, we kept these children in the sample. Dropping them from the sample does not change our results.

[5] For further details on the information received by the parents, see Section 1.2 in the Online Appendix.

We used a commercially available WM training software[6] providing training on different span tasks, using an age-specific user-interface, and adaptive levels of difficulty. Eight out of ten training tasks focus on visuo-spatial WM, while only two focus on verbal WM, i.e., a much larger variety of WM tasks and more training time was allocated to visuo-spatial WM training. The teachers supervised children in each training session, and logins for the training software were user-specific and only valid during the intervention period. Thus, the children only had access to the training software during their dedicated training sessions (see Online Appendix Section 1.2 for further details).

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Working memory training	0.488	0.5	0	1	572
Male	0.49	0.5	0	1	572
Children's age in months on Jan 1, 2013	82.129	4.324	72.222	101.578	572
Children's age on test day W1 (in months)	84.247	4.377	74.523	103.485	572
Children's age on test day W2 (in months)	87.288	4.355	77.745	106.706	572
Children's age on test day W3 (in months)	92.368	4.379	82.774	111.703	544
Children's age on test day W4 (in months)	99.582	4.381	90.467	118.836	531
Migration background	0.451	0.498	0	1	568
Language problems	0.247	0.431	0	1	572
Monthly HH-Net Income <750 Euros	0.023	0.149	0	1	441
Monthly HH-Net Income 750-1500 Euros	0.12	0.326	0	1	441
Monthly HH-Net Income 1500-2500 Euros	0.209	0.407	0	1	441
Monthly HH-Net Income 2500-5000 Euros	0.433	0.496	0	1	441
Monthly HH-Net Income >5000 Euros	0.215	0.412	0	1	441
Mother university degree	0.446	0.498	0	1	444
Mother vocational degree	0.423	0.495	0	1	444
Mother no professional degree	0.131	0.337	0	1	444
Academic track secondary school	0.692	0.462	0	1	393
Mixed-track secondary school	0.204	0.403	0	1	393
Non-academic track secondary school	0.104	0.306	0	1	393

The table provides socio-demographic information about our sample. The gender and age variables have been reported by the schools and are therefore available for all children. The variables 'Migration background' and 'Language problems' are taken from the teacher questionnaire in W1; for four children teachers reported not to know the migration background. Income and maternal education variables are taken from the parent questionnaire in W1. The information about secondary school track is taken from a survey administered to parents three years after treatment.

WM training typically took place in the first or the second lesson in the morning. During this time, the control group teachers taught their students the usual content covered in the first and the second lesson of the day for first graders in primary school (mostly major subjects such as math and German language). This means that students in the treatment group missed 25 such school lessons. Therefore, even if WM training improves some math or German skills, this improvement could, in principle, fall short of the improvement that the children in the control group experienced because they received more direct training in these subjects. This paper therefore analyzes the question of which activity improves skills more. This allows us to address a question of particular

[6] We used the WM training software Cogmed. Cogmed and Cogmed Working Memory Training are trademarks, in the U.S. and/or other countries, of Cogmed Inc. (www.cogmed.com).

importance for education policy, i.e., whether computer-based WM training during school hours is beneficial for the children. In other words, when we compare the treatment and the control group children on the various skill dimensions, we automatically take the foregone school lessons during WM training, i.e., the opportunity cost of the training, into account. This is important for an overall assessment of the desirability of WM training for a general school population of young children—the training is not without cost.[7]

Compliance with WM training was high in our sample. Only four out of 279 treated children finished less than 20 of the 25 daily training sessions. Since classes as a whole participated in the training, children missed a training session only when they did not attend school (e.g., for health reasons).

C. Data Collection

1. Computer-based Tests

Computer-based tests were completed by all children in four evaluation waves: at baseline (i.e., 3–4 weeks) before the training (W1), shortly (i.e., 4-5 weeks) after the training (W2), 6 months after training (W3), and 12–13 months after training (W4) (see Online Appendix Section 1.3 for further details). Parents of both treatment and control children gave their consent to participate in the data collection (consent rate of 96.8%). The tests were highly standardized and developed specifically for the purpose of the present study. The entire sequence of tests was computer-based, including auditory explanations (via headphones) and comprehension checks. The test items for each evaluation wave were adjusted to the relevant age and school curriculum at the different waves. A pretest prior to W1 with a different (smaller) sample of similar aged children served to adapt the initial level of difficulty. The input devices for the tests were large touchscreens instead of computer mice because we wanted to avoid any bias arising from the fact that children in the treatment group had been working with computer mice during the WM training. The testing procedure was run by a professional data collection service. The staff administering the tests was blind to treatment conditions. Teachers were not present during the tests and did not know their content. The teachers also did not receive any information or feedback about the performance of their students in the evaluation tasks. When the children had finished all evaluation tasks in a given wave, all children received a small toy for participating in the evaluation waves. These rewards were given to all children from the control and the treatment group to avoid any motivational differences between them.

In each evaluation wave, the children completed three (non-trained) WM tasks. WM capacity was measured with a verbal simple span task, a verbal complex span task, and a visuo-spatial complex span task (for details, see Online Appendix Section 1.4). Importantly, both the verbal complex span task and the visuo-spatial complex span task clearly differ from the tasks used in the WM training. We included a verbal simple span task (but not a visuo-spatial simple span task) in the set of our WM evaluation tasks because the WM

[7] Part of the literature on WM training emphasizes the importance of so-called active control groups. In our case, the control group is involved in the normal teaching lessons. It is sometimes also argued that an active control group might perform *non-adaptive* WM training, i.e., the children are *not* exposed to increasingly challenging tasks when they have solved the less challenging ones. However, one disadvantage of non-adaptive training is that the children may become bored and demotivated if they face tasks that constitute no real challenge and that, therefore, lead to no improvements. For this reason, and because we were interested in the policy question whether WM training enables improvements relative to normal teaching lessons, our control group is involved in normal teaching lessons that typically involve increasingly challenging material.

training places considerably less weight on verbal compared to visuo-spatial WM. Direct effects may therefore be weaker for verbal WM. The verbal simple span task might allow us to capture these presumably weaker effects. The three WM tasks mentioned above not only enable us to study direct effects, but they also serve the purpose of examining the extent to which WM capacity mediates training-induced improvements in other important skills.

In each evaluation wave, the children also completed a set of tasks that enabled us to measure such spillover effects: Educational achievement was measured in three areas: arithmetic, geometry, and reading. We included geometry as an outcome measure because—like arithmetic and reading—it plays an important role in everyday life (e.g., orientation, reading maps, driving, and parking) as well as in various professions (e.g., construction/architecture, fashion/art design, geography, physics, sports, etc.). In addition, we measured three other important skills that capture key aspects of executive functions (EFs), such as fluid IQ (higher-level EFs), the ability to inhibit pre-potent responses (inhibitory control), and the ability to sustain attention and display frustration tolerance (attentional stamina). We use Raven's Colored Progressive Matrices test (Bulheller and Häcker 2010) as a measure for fluid IQ. The ability to inhibit pre-potent responses (inhibitory control) was measured with the go/no-go task (Gawrilow and Gollwitzer 2008), and attentional stamina was measured using the bp task (Esser, Wyschkon and Ballaschk 2008). For a detailed description of all these tasks, see Online Appendix Section 1.4.

2. Teacher Ratings

In each data collection wave (W1–W4), teachers filled out a questionnaire containing items on children's and teachers' characteristics and behaviors, and (for treated teachers) expectations about the intervention. We achieved a 100% return rate for the teacher questionnaire in all four evaluation waves. A key part of the teacher questionnaire is a series of questions capturing teachers' assessment of each child's self-regulatory abilities (for details, see Online Appendix Section 1.4).

3. Secondary School Track Choice

In a follow-up survey in spring 2016, we asked parents to report their children's school track for secondary school in fall 2016. Secondary school starts at grade five, i.e., three years after the WM training when the children are 10–11 years old. Essentially, there are three different secondary school tracks available: (i) an academic track (*Gymnasium*), (ii) a mixed track (*Integrierte Gesamtschule*), and (iii) a non-academic track (*Realschule Plus*). In this particular federal state in Germany, 86% of the children in the academic track earn a degree that qualifies them for general university enrollment (*Abitur*), whereas only 25% percent of children in mixed-track schools achieve this (Rhineland-Palatinate 2018). Within the non-academic track, students cannot earn a degree that qualifies them for general university enrollment. For children in the non-academic track, the probability of switching track is small (< 5% per year) (Bellenberg 2012). Moreover, since the early school track choice at this age has a decisive influence on the whole educational career path, it also exerts a substantial influence on later wages (Dustmann 2004). Thus, the choice of the secondary school track constitutes a major educational decision that strongly affects a child's future outcomes and life-time earnings.

D. *Conjectures About the Treatment Effect on Outcome Measures*

In addition to direct effects on WM capacity, WM training may have positive spillover effects on our educational outcome measures, but in varying degrees. Performing arithmetic tasks, such as adding or subtracting several numbers, requires children to store and recall "intermediate results" while performing the computations, thus requiring WM capacity. Likewise, geometry tasks, such as estimating how many times a smaller geometrical object fits into a larger one, and reading comprehension require WM capacity. However, in our context it is important to take into account that teaching time in primary school is very unevenly allocated between arithmetic and geometry: during the first grade, the curriculum requires that about 70% of the math lessons be spent for teaching arithmetic. Because the treatment subjects miss a considerable number of math lessons and because our WM training was focusing on visuo-spatial WM (see above), it seems more likely that we find positive training effects on geometry than on arithmetic skills. With regard to reading performance, it is important to keep in mind that the children gradually learn the various letters of the alphabet during the first grade, allowing them to read and understand an increasing number of letters and words over time. We measured reading skills by a reading comprehension task that required children to understand and process all words in a sentence, and to assign meaning to the full sentence. This is obviously much more difficult when children still have problems reading single words. Moreover, correlational evidence suggests (Kibby, Lee and Dyer 2014; Nutley and Soderqvist 2017) that WM capacity does not predict word identification, but it seems to be an independent predictor of reading comprehension once word reading ability has been acquired. This suggests an additional, independent reason—apart from the possibility that spillover effects generally may need time to emerge—for why WM training effects in our reading task may only emerge over time.

Turning to more general cognitive skills, WM capacity has also been shown to be correlated with fluid intelligence as measured, for example, by the Raven's matrices task—a task that requires visuo-spatial WM but is nevertheless different from pure WM tasks because it requires (i) reasoning in novel situations without prior knowledge, (ii) the ability to generate high-level schemata in order to handle complexity, as well as (iii) the ability to absorb, recall, and reproduce information provided in the task (Carpenter, Just and Shell 1990; Oberauer et al. 2005; Wiley et al. 2011).[8] Therefore, WM training may improve performance in Raven's matrices task. However, the previous empirical literature is in sharp disagreement about whether WM training improves fluid IQ measured using Raven's Matrices tasks (Au et al. 2015; Melby-Lervag, Redick and Hulme 2016).

Working memory is one of three core components of executive functioning – with inhibitory control and cognitive flexibility being the other two (Diamond 2013). The literature on executive functioning hypothesizes that "working memory and inhibitory control need one another and co-occur", and that "working memory supports inhibitory control" (Diamonds 2013, p. 143). This is also consistent with the view that working memory capacity is crucial for the ability to actively maintain task-relevant and suppress/inhibit task-irrelevant information (Engle 2002). WM capacity might thus enhance the ability to avoid distraction, which is consistent with the evidence showing that individuals with low WM capacity are less able to suppress salient distractors

[8] Note that Raven's matrices task does not measure general IQ but is a non-verbal test that is regarded as a measure of fluid intelligence based on visuo-spatial capabilities.

(Gaspar et al. 2016). Based on this account, WM training may thus generate spillover effects on inhibitory control. In the context of the go/no-go task this means that children who undergo WM training should be better able to avoid commission errors because the children in this task almost always see symbols that require them to press a button within a very short time interval, placing them in the "go-mode". Occasionally, however, a "no-go" symbol is shown that requires them to *refrain* from pressing the button. In this view, the frequent display of "go" symbols distracts individuals and makes it difficult for those with low WM capacity to maintain the goal and provide the appropriate behavioral response associated with the "no-go" symbols. We also measure children's attentional stamina with a letter discrimination task, the so-called "bp task". To our knowledge, it is an open question whether WM training improves this aspect of EFs.[9]

Finally, in case we find that WM training has spillover effects on academic performance or other important skills, it might be possible that WM training also positively affects secondary school track choice because that choice is presumably influenced by children's academic skills, their fluid IQ, and their self-regulatory skills.

III. Empirical Results

To estimate the treatment effect of WM training, we regress outcome scores measured after the training (W2–W4) on a treatment indicator and a vector of control variables.[10] All outcome scores are standardized within each evaluation wave to mean 0 and standard deviation 1. We control for the pre-training baseline level of the respective outcome score in our regressions. Thus, instead of identifying how WM training changes individuals' outcome scores between pre- and posttreatment waves (i.e., using the difference-in-differences estimator), we estimate how the training changes outcome levels and control for the baseline level of the respective outcome. The advantage of this method is that the variance of the estimated effect is smaller, i.e., the treatment effect is measured with more precision (Frison and Pocock 1992; McKenzie 2012). Finally, in order to allow for interdependence of observations within school classes, standard errors are clustered at the classroom level. In our robustness analysis we also apply the Romano-Wolf stepdown procedure to control for multiple hypothesis testing (Romano and Wolf 2005; Romano and Wolf 2016)—a technique that is increasingly used for large-scale intervention studies (see, for example, Cunha et al. (2010), Campbell et al. (2014), Gertler et al. (2014))—and, simultaneously, we control for potential biases that may arise when the number of clusters is relatively small with the BRL (biased-reduced linearization) correction method (Bell and McCaffrey 2002).

A. Sample Balance

To examine whether randomization led to a balanced sample across treatment and control group in terms of socio-economic characteristics, we regress various socio-demographic characteristics (gender, age, migration background, as well as parental income and education) measured prior to the treatment (W1) on the treatment

[9] Our WM training may also be viewed from the perspective of prominent interventions that boosted executive functioning (see e.g., Walker et al. 2022; Garcia, Heckman and Ronda 2023) and led to long-lasting spillover effects on a wide range of skills.

[10] The vector of control variables consists of school fixed effects, gender, age, age at test days, baseline value of the outcome, and indicators for other treatments (unrelated to the WM-training) that were conducted in the same sample. For further details on estimation, see Online Appendix Section 1.5).

indicator (see Table S1 in the Online Appendix). The results show that the treatment coefficient in all regressions is close to zero and insignificant, indicating that there were no significant imbalances between treatment and control group with respect to these variables.

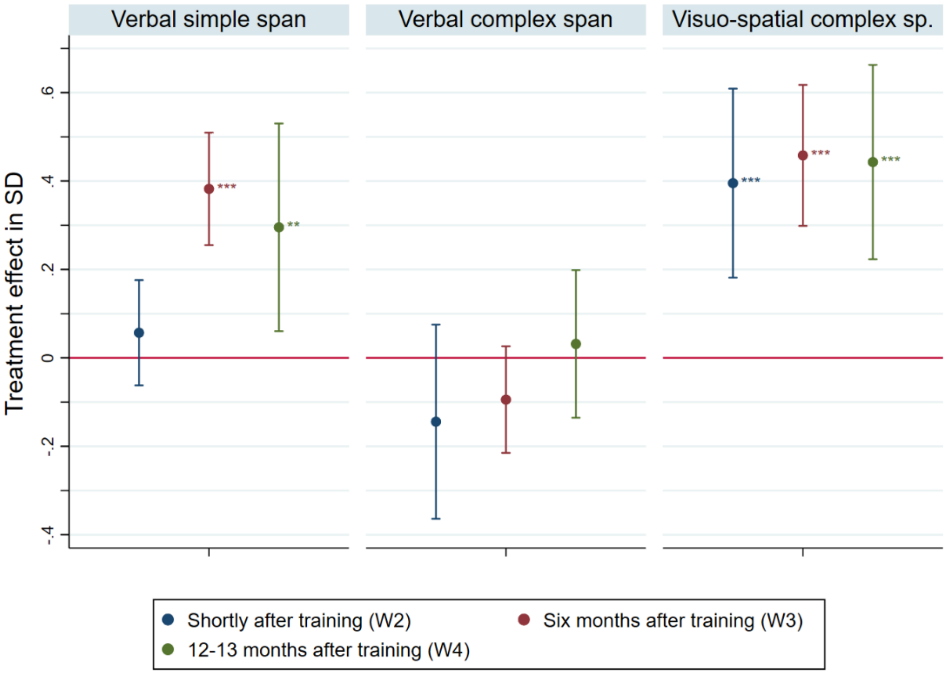
As a further sample balance check, we regressed standardized outcome test scores at baseline (i.e., test scores measured prior to the treatment in W1) on the treatment dummy, school fixed effects, and the same control variables that are included in the main estimations of the treatment effect. Table S2 in the Online Appendix shows that with the exception of the baseline score for the verbal complex span task, none of the coefficients related to the treatment dummy is significantly different from zero, indicating that for all other baseline test scores there is no evidence for significant imbalances between treatment and control group. With regard to the possible imbalance in the baseline score of verbal complex span, we have to take into account that we conducted a total of 15 imbalance test regressions. For this reason, we further examined the issue by adjusting p-values for multiple hypothesis testing and applying the biased-reduced-linearization clustering method (which accounts for small numbers of clusters). This then yields a p-value of 0.332 for the verbal complex span outcome, suggesting no significant difference between the treatment and control group once we account for the number of tests conducted. In addition, we would like to mention that we control for the baseline tests scores in W1 in all our regressions that measure the treatment effect of WM training on outcome scores in W2–W4.

B. Treatment Effect on Computer-based Test Outcomes

To estimate the effect of WM training, we regress outcome scores measured shortly after the training (W2), 6 months after the training (W3), and 12–13 months after the training (W4) on the treatment indicator. The estimated direct effects of WM training on WM capacity are presented in Figure 1 and Table S3 in the Online Appendix. We find significantly positive treatment effects for the visuo-spatial complex span task in all three post-treatment waves with an effect size (d) of 0.40–0.46 SD ($p = 0.00004$–0.006). We also find a significantly positive training effect on performance in the verbal simple span task of $d = 0.38$ SD ($p = 0.000008$) in W3 and $d = 0.30$ SD ($p = 0.015$) in W4. We do not find any significant treatment effect for performance in the verbal complex span task. The stronger effect of training on visuo-spatial WM compared to verbal WM is plausible, as the training focused primarily on visuo-spatial WM (see Section II.B).

Spillover effects of WM training on educational outcomes—arithmetic, geometry, and reading—and Raven's fluid IQ measure are reported in Figure 2 and Table S4 in the Online Appendix. While there is no treatment effect on arithmetic in all three post-training waves, we find an effect on geometry skills that is increasing over time. The effect size $d = 0.17$ in W2 is not yet significantly different from zero ($p = 0.108$), but the effect size increases in W3 and W4 to $d = 0.24$ and $d = 0.38$, respectively, with significance levels of $p = 0.021$ in W3 and $p = 0.001$ in W4. Thus, it seems that WM training had a positive and increasing spillover effect relative to the normal school curriculum on geometry skills but not on arithmetic skills. The significant and relatively strong impact on geometry skills is also consistent with the fact that training focused primarily on improving visuo-spatial WM capacity. The spillover effects on reading are generally lower than for geometry, but they are also rising over time and become significant in W4. There is no positive effect on reading shortly after the training, but we observe a larger, yet still insignificant effect in W3 and an effect size of d = 0.23 at p = 0.037 in W4. This rising spillover effect on reading is consistent with the view (Nutley and Soderqvist 2017) that WM capacity plays a smaller role for reading comprehension when children are still struggling to understand words, but eventually becomes relevant for reading comprehension when word identification has progressed sufficiently.

Figure 1: Direct Effect of Training on Working Memory Capacity

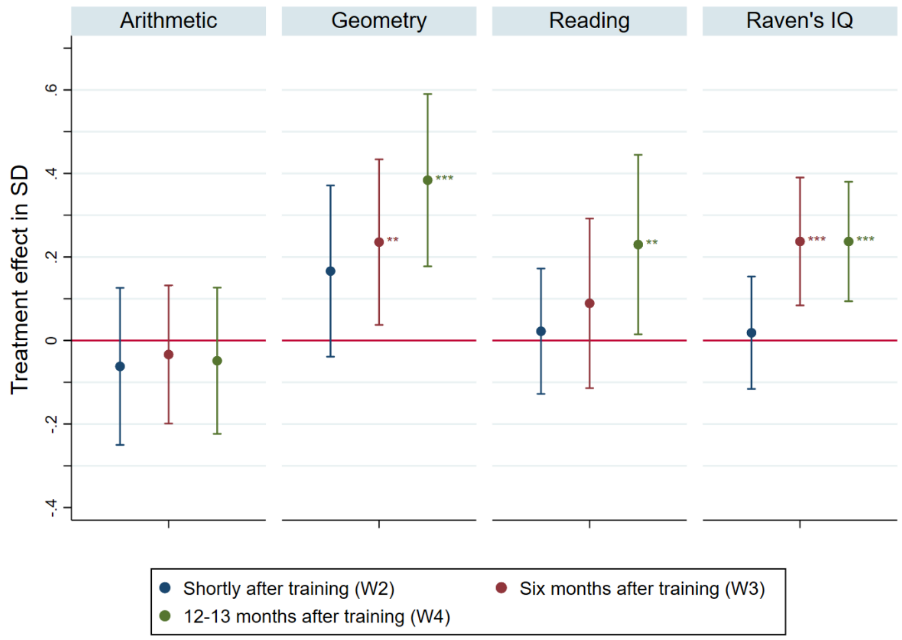


The dots show the point estimates (as fractions of a standard deviation) of how WM training changes the performance in the three working memory tasks (indicated in the subfigure title) relative to the control group. The bars indicate the 95% confidence intervals. All estimates are based on least squares models controlling for school fixed effects, pre-treatment outcome scores, and further controls (see Online Appendix Section 1.5 for details). The econometric estimates are shown in Table S3 of the Online Appendix. The confidence intervals and the associated significance statements are computed based on the clustering of standard errors at the classroom level. Stars refer to significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01.

We also find a significant spillover effect on Raven's Colored Matrices task six months (d = 0.24, p = 0.004) and 12–13 months after the training (d = 0.24, p = 0.002). We emphasize that this finding does not mean that WM training increased all dimensions of fluid intelligence, as some research indicates that only 64% of the variance in performance in a Raven's task is attributable to general fluid intelligence (Jensen 1998). However, the Raven task measures important dimensions of fluid intelligence which require WM capacity (Carpenter, Just and Shell 1990) and its deployment in novel situations (Wiley et al. 2011).

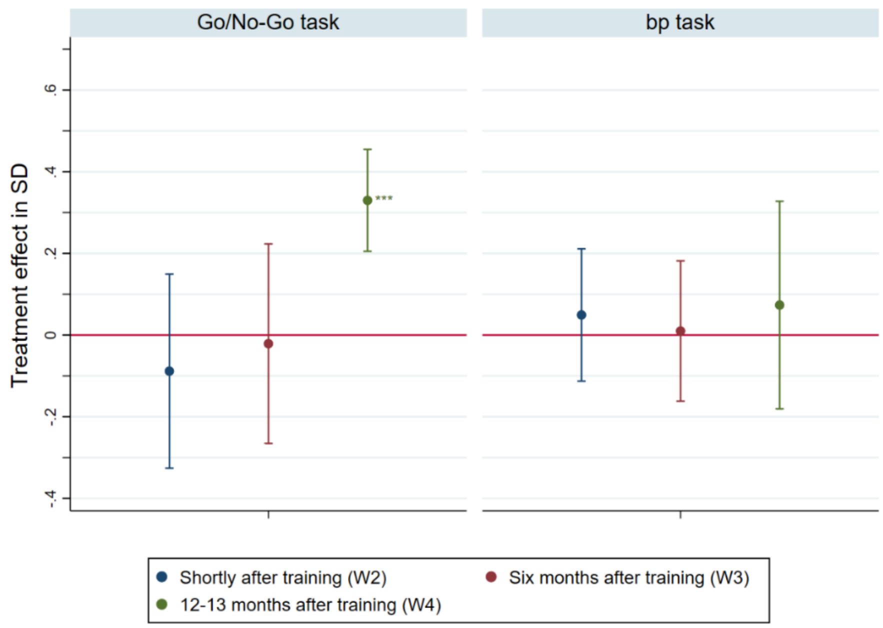
It is also important to mention that *none* of the treatment effects in geometry, reading, or Raven's fluid IQ measure are driven by a *decline* in the performance of the control group. Due to cognitive maturation over the course of one year, both the treatment and the control group increased their performance over time. As such, the treatment effects are due to a differentially larger increase in performance in the treatment group.

Figure 2: Spillover Effects on Arithmetic, Geometry, Reading, and Raven's IQ



The dots show the point estimates (as fractions of a standard deviation) of how WM training changes performance in arithmetic, geometry, reading, and Raven's fluid IQ measure, respectively, relative to the control group. The bars indicate the 95% confidence intervals. All estimates are based on least squares models controlling for school fixed effects, pre-treatment outcome scores, and further controls (see Online Appendix Section 1.5 for details). The econometric estimates are shown in Table S4 of the Online Appendix. The confidence intervals and the associated significance statements are based on the clustering of standard errors at the classroom level. Stars refer to significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3: Spillover Effects in the Go/No-Go Task and the bp Task



The dots show the point estimates (as fractions of a standard deviation) of how WM training changes the performance in the go/no-go task and the bp task relative to the control group. The bars indicate the 95% confidence intervals. All estimates are based on least squares models controlling for school fixed effects, pre-treatment outcome scores, and further controls (see Online Appendix Section 1.5 for details). The econometric estimates are shown in Table S5 of the Online Appendix. The confidence intervals and the associated significance statements are computed based on the clustering of standard errors at the classroom level. Stars refer to significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Finally, we turn to the effects of WM training in the go/no-go task and the bp task (Figure 3 and Table S5 in the Online Appendix). We find positive spillover effects of WM training on children's inhibitory control measured in the go/no-go task. We measure inhibitory control by multiplying children's standardized number of commission errors with -1, i.e., a reduction in commission errors shows up as a numerical increase in this performance measure. Figure 3 indicates a highly significant reduction in commission errors in the treatment relative to the control group in W4 ($d = 0.33$, $p < 0.0001$).[11] Interestingly, while we observe no treatment effect on commission errors in W2 and W3, we observe a weakly significant treatment effect on performance in terms of a reduction in response times in W2 ($d = 0.23$, $p = 0.053$) and W3 ($d = 0.37$, $p = 0.094$). Thus, although the children in the treatment group did not make fewer mistakes in W2 and W3, they were quicker in delivering their responses (without increasing their mistakes) in these evaluation waves.[12]

Overall, these data patterns suggest that, similar to the case of geometry, reading, and Raven's fluid IQ measure, spillover effects on inhibitory control emerge over time. This effect supports the theoretical conjecture that "working memory supports inhibitory control" (Diamonds 2013, p. 143). Note also, that this spillover effect is due to a differentially larger increase in the performance of the treatment group relative to the control group in terms of fewer errors. In contrast to the results in the go/no-go task, we cannot detect a training-related improvement in performance in the bp task. In fact, the time profile of the treatment effects is completely flat and close to zero, suggesting that WM training does not affect attentional stamina.

C. Treatment Effect on Choice of Secondary School Track

Our finding that WM training has positive spillover effects on several outcomes relevant for the school context suggests the possibility that it might affect children's further school career. As mentioned previously, one of the most consequential school track choices in the German education system is whether the children enter the advanced track (academic track, also called *Gymnasium*) of secondary school. This choice is typically taken around age 10, i.e., three years after the children received the WM training.

Controlling for the same set of variables as for the other treatment effects, we find that children in the treatment group are roughly 16 percentage points more likely to choose the advanced track of secondary school relative to children in the control group (Table 2, column 1). If we estimate the treatment effect with a probit model instead of a linear probability model (Table 2, column 2), the result is very similar—the children in the treatment group are again roughly 15 percentage points more likely to be enrolled in the advanced track of secondary school. If we take the full range of secondary school choices (advanced track, mixed track, non-

[11] We also analyzed the standardized (i.e., z-scored) d'-measure of performance in this task—which subtracts the standardized fraction of commission errors in the no-go trials from the standardized fraction of correct responses in the go trials—and find a significant performance effect in W4 (W2: $d = 0.118$, $p = 0.410$; W3: $d = 0.071$, $p = 0.619$; W4: $d = 0.475$, $p < 0.0001$). If we analyze omission errors (i.e., missing to push the button in go-trials, which is often interpreted as a measure for "attention") separately, we also find similar positive treatment effects as for inhibitory control, with the strongest and significant improvements in W4 (W2: $d = 0.282$, $p = 0.109$; W3: $d = 0.133$, $p = 0.357$; W4: $d = 0.416$, $p = 0.001$).

[12] Similarly, when analyzing *teacher-reported* overall self-regulation as a measure of everyday self-regulatory behavior in the classroom, we also find significant positive treatment effects (see Online Appendix, Sections 1.4 and 1.5, and Table S19).

academic track[13]) into account, we again find a sizeable positive treatment effect on enrollment in the advanced track (columns 3 and 4). Column 4 of Table 2 also indicates that the increase in advanced track enrollment by roughly 14 percentage points is due to a decrease in mixed track enrollment by roughly 7 percentage points and a similar decrease in non-academic track enrollment.

As we measure the secondary school track enrollment three years after the WM training, we naturally observe some attrition. This is due to reasons such as families moving away from the city of our study or when the parents do not answer the long-run follow-up questionnaire. Importantly, however, we do not observe a systematic difference in attrition between treatment and control group. In the treatment group, we still can collect data of 68.1% of the sample in W1 and in the control group we have data of 69.3% of the sample in W1 (see Online Appendix, Section 1.6 for further robustness checks on attrition).

Table 2: Treatment Effect on Secondary School Choice at Age 10

Treatment Effect of WM training on choice of	(1) OLS	(2) Probit	(3) OLS cat var	(4) Ordered Probit	(5) Inverse Prob Weighting
Academic track school	0.157*** (0.050)	0.148*** (0.045)	0.221*** (0.078)	0.136*** (0.046)	0.170*** (0.050)
Mixed track school				-0.067*** (0.025)	
Non-academic track school				-0.069*** (0.023)	
N	393	393	393	393	393

Column 1 reports the effect of the treatment on the probability of being enrolled in an academic track secondary school based on a least squares model. When we cluster the standard error using biased reduced linearization (BRL), the standard error in column 1 becomes 0.070 (which corresponds to a p-value of 0.026). Column 2 reports the marginal treatment effect of the probit estimate on the same dependent variable as in column 1. Column 3 reports the least squares effect on a categorical dependent variable. This variable takes on value 1 if the child is enrolled in a non-academic track school (*Realschule Plus*), value 2 if the child is enrolled in a mixed-track secondary school (*Integrierte Gesamtschule*), and value 3 if the child is enrolled in an advanced track school (*Gymnasium*). Column 4 reports the marginal treatment effects of the ordered probit estimates on the same dependent variable as in column 3. Column 5 reports a similar estimation as in column 1 but accounts for attrition by applying inverse probability weighting. The weights are calculated for groups defined based on migration background, high/low academic performance (math and reading performance), and high/low cognitive performance (WM capacity and Raven's fluid IQ measure). All models include school fixed effects and further controls (see Online Appendix, Section 1.5, for further details, including our calculation of the inverse probability weights). Standard errors in parentheses are clustered at the classroom level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

We also address systematic attrition by estimating inverse probability weighting models. To apply these models, we compared the sample characteristics in W1 with the sample characteristics at the time of secondary school choice. This comparison shows that at the time of secondary school choice there are (i) fewer children with a migration background, (ii) more children with higher academic performance (i.e., geometry, arithmetic, and reading), and (iii) more children with higher cognitive skills (i.e., working memory capacity and Raven's fluid IQ measure). Therefore, we calculated the inverse probability weights for groups defined on the basis of three binary variables: (i) migration background, (ii) high/low academic performance in geometry, arithmetic, and reading, and (iii) high/low cognitive skills as measured by WM capacity and Raven's fluid IQ measure.

[13] In Germany, the non-academic track is called "*Realschule Plus*", the mixed track is called "*Integrierte Gesamtschule*", and the academic track is called "*Gymnasium*".

The result of this model (shown in column 5) indicates that the WM training increases advanced track enrollment by roughly 17 percentage points.

To gauge the size of our effect on school track choice, consider the relationship between parental education and school track choice for the control group: for children whose mother has a university degree, 86% chose the advanced track, for those whose mother does not have a university degree, the number is 54%; i.e. a difference of 32 percentage points. This difference reduces to 27 percentage points when controlling for children's baseline measure of Raven's fluid IQ. Thus, the 14–17 percentage point increase in advanced track enrollment is substantial when compared with this socio-economic gap.

D. Heterogenous Treatment Effects?

Do disadvantaged children benefit particularly strongly from WM training? Existing work has raised this question and remains inconclusive (Katz and Shah 2016; Roberts et al. 2016). We examined the heterogeneity of treatment effects with regard to initial WM capacity by including a dummy variable for the children who are below the 25th percentile in the distribution of WM capacity at baseline (W1), and by interacting this dummy variable with the treatment dummy (see Tables S6–S8 in the Online Appendix). The results show that children with low baseline WM capacity perform substantially worse in all spillover outcome measures (and all data collection waves) with the exception of the bp task. However, the interaction between low WM capacity and the treatment dummy is almost never significant (with the exception of geometry in W2, where we observe a positive interaction, and the bp task in W2, where the interaction is negative). This suggests that the treatment effect is not systematically different for children with low WM capacity. Importantly, however, the treatment effect is robust to the inclusion of the low WM capacity dummy and its interaction with the treatment dummy for all outcome variables for which we previously found a significant treatment effect.

E. Robustness Checks

We perform a series of robustness checks, including checks for attrition, the potential role of computer use, Hawthorne or demand type effects, and multiple hypothesis testing corrections. For the multiple hypothesis testing, we grouped our outcomes into four families, following the above conjectures for treatment effects: 1) working memory outcomes (verbal simple span, verbal complex span, visuo-spatial complex span), 2) spillover effects on educational outcomes (arithmetic, geometry, reading), 3) spillover effects on general cognitive skills (Raven's IQ), and 4) spillover effects on general noncognitive skills (Go/No-go task, bp task). Note that each family includes three measurements for each outcome (at W2, W3, and W4). Overall, these robustness checks confirm our findings, except for the treatment effect on reading in W4, which turns insignificant if we correct for multiple testing (see Table S9 in the Online Appendix).[14] All details on robustness can be found in the Online Appendix, Section 1.6.

[14] We also provide further multiple testing analyses in Table S10, using an even more conservative grouping into only two families (direct effects and spillover effects). Again, three measurements are included for each outcome in a family (at W2, W3, and W4). While we believe that the grouping of families described above (in Section E) is the most reasonable, the choice of families of outcomes is always somewhat discretionary. With the very conservative grouping of outcomes into only two families, results remain similar to Table S9 but the treatment effects on Raven's IQ are no longer significant at conventional levels (W3: $p = 0.136$, W4: $p = 0.114$).

IV. Mechanisms

In our view, the documented treatment effects on WM capacity and on spillover outcomes have a plausible interpretation. For example, it is plausible that WM training has an immediate effect on visuo-spatial WM capacity (i.e., the aspect of working memory that received the most emphasis during the training), while spillover effects need more time to evolve—which is what we observe in our data. Likewise, the finding that WM training does not increase arithmetic but geometry skills may be due to the fact that the training emphasized visuo-spatial WM, which may well play a larger role in geometry compared to arithmetic. Similarly, visuo-spatial WM capacity is likely to be a basic prerequisite to deploy the problem-solving skill that is required to solve Raven's fluid IQ task.

To provide a quantitative assessment of the extent to which WM capacity might be a mediating mechanism for the observed spillover effects, we performed a mediation analysis by using the method applied in Heckman, Pinto and Savelyev (2013), and similarly in, e.g., Kosse et al. (2020) and Carlana, La Ferrara and Pinotti (2018). The formal details of this method are described in the Online Appendix, Section 1.5. Intuitively, the method provides us with the share of the total treatment effect of the training on each spillover outcome that can be explained by the training induced changes in WM capacity.

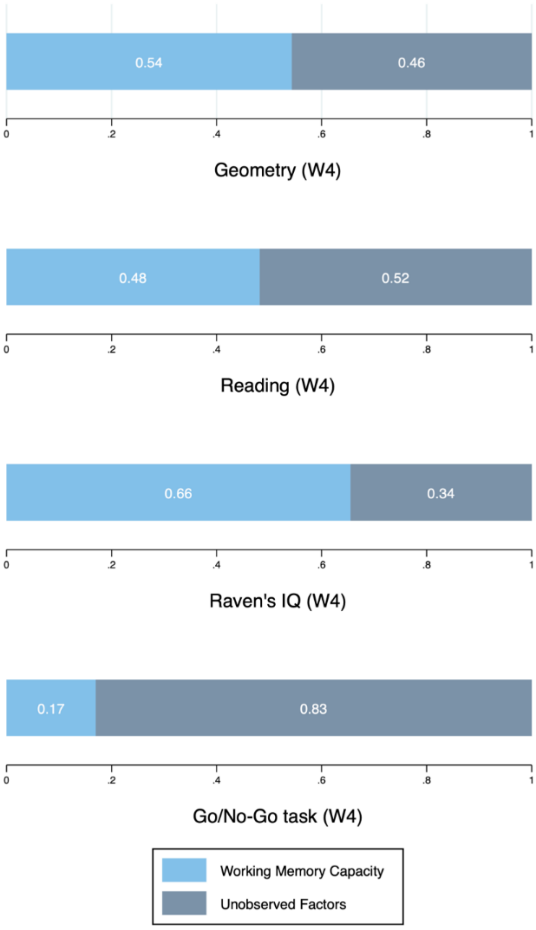
The results of our mediation analysis are presented in Figure 4. The figure shows that for geometry, reading, and Raven's fluid IQ measure a large part of the total treatment effect—roughly between 50% and 66%—is mediated by WM capacity. Interestingly, the mediation effect of WM capacity is much lower for our measure of inhibitory control (performance in the go/no-go task). Perhaps this lower mediation effect of WM capacity is one reason why the training effect on the ability to inhibit pre-potent impulses took more time to develop.

Overall, this analysis suggests that training-induced changes in WM capacity appear to explain substantial parts of the treatment effect on spillover outcomes. In view of the previous literature on WM training (e.g., Sala et al 2019, Sala and Gobet 2020), we were, however, surprised by the magnitude of the effects on spillover outcomes. Therefore, we point out specificities of our study that are likely to be relevant in this context: First, we delivered the WM training in a school context as part of the regular curriculum which ensures high external validity. Moreover, the integration of the treatment into regular classroom teaching may have facilitated the spillover effects to other school-related skills. The context of regular classroom teaching is also likely to minimize placebo or Hawthorne effects. Of course, we cannot completely rule out the possibility that placebo effects may have played a role, for example because the children in the treatment group received extra attention (e.g., because they used computers in class or due the presence of a research assistant during the training, see Online Appendix, Section 1.2). Moreover, by design minor differences between treatment and control group inevitably remain that could potentially affect abilities other than WM capacity (e.g., narrow task learning due to familiarity with WM tasks).[15] However, the facts that (i) we carefully developed outcome measures that are different from the training tasks (even with respect to input devices, i.e., touchscreens vs. external mice, see

[15] For example, in a computer-based WM training, treated children will automatically become more familiar with WM tasks. Thus, they may perform better in subsequent WM tasks merely because they are more familiar with the type of tasks and not because they have higher WM capacity. Similarly, they have more screen time than children in the control group, which could potentially improve skills such as perceptual speed.

Section II.C), and that (ii) we see treatment effects on very specific spillover outcomes that require visuo-spatial working memory (and no effect on other important educational outcomes) and that (iii) the pattern of effects increases over time, suggest that placebo effects or remaining minor group differences are unlikely to have played a substantial role.[16]

Figure 4: The Relative Importance of Working Memory Capacity for the Treatment Effects on Spillover Outcomes



Notes: This figure displays the estimated decomposition of the total treatment effect on those spillover outcomes that are significantly improved by the WM training in W4 (12–13 months after treatment). For each outcome, we estimate the effect of the treatment that is mediated by WM capacity (see Online Appendix 1.5 for details). The light blue bars show the percentage of the treatment effect that is mediated by training-induced increases in WM capacity.

Second, as mentioned previously, our study is better capable of detecting spillover effects because we measure the relevant outcomes also 6 and 12-13 months after the treatment while most other studies stop collecting spillover outcomes after a few months, and thus cannot identify effects that might take a longer time to evolve. Third, because we treated complete classes (class-wise randomization), in addition to effects on individual-level skills, the treatment possibly led to various sorts of positive peer group and classroom effects,

[16] Note also that our intervention was part of a larger educational study, involving other treatments. However, we control for the other treatments in all our estimations, and we conduct various robustness checks, including correction for multiple testing and small number of clusters, to minimize the likelihood of false positives or spurious findings (for details see Online Appendix, Sections 1.5 and 1.6).

that, in turn, could have affected teachers' behavior and attitudes. In our setting, such beneficial peer group effects seem plausible, given that the children usually stay together in the same class and with the same teacher for four years in primary school. Evidentially, these peer group effects constitute an important factor for the persistence of treatment effects of interventions at young ages (cf. Bailey et al. (2017)).

V. Summary

Based on a randomized controlled trial with 572 first graders in primary schools, we found that a five-week, one lesson per school day, adaptive WM training during class improves not only children's WM capacity but also has spillover effects on their geometry skills, Raven's fluid IQ measure, and their ability to inhibit pre-potent impulses. We observe an increasing pattern of treatment effects on these spillover outcomes over the three evaluation waves with effect sizes ranging between 0.24 and 0.38 SD. In addition, the general pattern of our results and our mediation analysis suggest that training-induced improvements in WM capacity mediate considerable parts of the spillover effects. When assessing the reported effect sizes for the spillover effects, it is interesting to compare them with effect sizes observed in other (more intensive) intervention studies such as Perry Preschool, the Jamaican supplementation and stimulation study, and others, producing improvements in executive functions even in the very long-run of 0.25 to well above 0.5 of a SD (Riggs et al. 2006; Raver et al. 2011; Heckman, Pinto and Savelyev 2013; Gertler et al. 2014; Walker et al. 2022; García, Heckman and Ronda 2023). Finally, we document that the WM training has a sizeable impact on one of the most consequential school career decisions in the German school system: whether to enroll the child in the advanced track of secondary school (*Gymnasium*). This fact has potentially far-reaching implications for the treated children's probability of entering university and their labor market outcomes, because children who complete the Gymnasium are much more likely to go to university and earn significantly higher salaries. The increasing pattern of effects on spillover outcomes combined with the effect on long-run educational choices is consistent with the idea of self-productivity in the process of skill formation (Cunha and Heckman 2007). Taken together, our findings thus provide novel evidence consistent with the dynamic process of skill formation and they suggest that our treatment generated substantial benefits for the children.

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Acknowledgments. We would like to thank all teachers, schools, and educational authorities as well as all parents and children for their participation in the project. We are also thankful to countless excellent research assistants who made this field study possible. Moreover, we would like to thank Michael Wolf for support and provision of code in conducting the multiple testing correction. We also thank the editor for pointing us to the links of our study to intervention research on executive functions and for making the paper more concise. We gratefully acknowledge financial support by the Jacobs Foundation (project 2013-1078-00), the University Research Priority Program of the University of Zurich on Equality of Opportunity (project U-302-01-01), the German Academic Scholarship Foundation, the German Research Foundation (DFG, BE 5436/1-1), the university research priority program “Interdisciplinary Public Policy” at Johannes Gutenberg University Mainz (project FI 2/2014-2016), and the Research Council of Norway (FAIR, project 262675). The funders had no role in study design, data collection and analysis, decision to publish, or preparation of the manuscript.

Author Contributions: E.F. and D.S. initiated and supervised the study throughout all stages. E.F., D.S., E.M.B., and K.W. conceptualized the study and all authors developed the field design. E.M.B., H.H., D.S., and K.W. developed outcome measures for the study. H.H. conducted the field experiment with input from E.M.B., E.F., D.S., and K.W.; E.M.B. and H.H. performed the data analysis with input from E.F., D.S., and K.W.; all authors were involved in the interpretation of the results and all authors wrote the paper.

Online Appendix

for

The Impact of Working Memory Training on Children's Cognitive and Noncognitive Skills

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Contents

1 Supplementary Text

The study was conducted in primary schools in Mainz, Germany, in 2013/2014. It comprised a five-week intervention, four data collection waves, and a long-term follow-up survey three years after the intervention. Our study received ethical approval from the Human Subjects Committee of the Faculty of Economics, Business Administration and Information Technology at the University of Zurich in September 2012. We confirm that we have complied with all relevant ethical regulations.

The study consisted of a pre-intervention data collection wave (W1), the five-week intervention period, a data collection wave shortly (4–5 weeks) after the intervention (W2), and two follow-up data collection waves 6 months and 12–13 months after the intervention (W3 and W4).

We provide supplementary details on participants (Section 1.1), the treatment condition (Section 1.2), the data collection waves (Section 1.3), outcome measures (Section 1.4), and the data analysis (Section 1.5). We discuss a range of robustness checks in Section 1.6. Supplementary figures are provided in Section 2, and all supplementary tables in Section 3.

1.1 Supplementary Details on Participants

Sampling of Participants

In February 2012, we received the approval from the Federal Ministry for Education in Rhineland-Palatinate to conduct the study with first graders in the city of Mainz. The authority responsible for elementary schools in Mainz (ADD) contacted schools and provided us with a list of elementary schools in May 2012. We selected 12 schools for participation in the study based on two criteria: being located in the city of Mainz and the possibility of including at least two school classes per school in the study. The participating schools agreed that (i) one school lesson per day would be replaced by a working memory (WM) training lesson for 25 school days and that (ii) the children would participate in all four planned data collection waves. Importantly, all 12 schools that we approached for participation agreed to meet the above-mentioned conditions, including the randomization of classes within schools to treatment and control conditions. In other words, none of the schools that we approached for participation refused to participate, and in all participating schools, the schools (or school principals) had no influence on the treatment assignment of the participating classes. The high willingness to participate in the study might have also been due to the fact that the schools received the IT infrastructure necessary to run the study (a notebook for each participating child; cases for transportation, charging and storage of the notebooks; accessories like mice, headphones, and wifi routers) for permanent use.

Final Sample and Attrition

As described above, we recruited 12 schools with 31 classes for the study. The sample consisted of three schools with four classes, one school with three classes, and eight schools with two classes. There were 599 children in these classes in November 2012. We received 580 parental consent forms that allowed us to collect data in evaluation waves W1–W4, resulting in a consent rate of 96.8%.[1] Our sample covers a substantial

1. Among the children for whom we did not receive parental consent, roughly 50% participated in the working memory training while the other roughly 50% were in the control classes. However, we

fraction of children in the study area (covering more than one-third of a cohort of first graders, and 46% of primary schools in Mainz). Thus, while our sample is not representative for the German population, it covers a broad range of typical school children in Germany and specifically the city in which our study was conducted. We were able to evaluate 572 children of the 580 for whom we received parental consent to collect data for our final data set.[2] The children we could not evaluate either switched to non-participating classes or schools, moved away, or were ill for a longer period of time during data collection; we did not exclude any available data. Among the sample of 572 children, 292 were girls (51%) and 280 were boys (49%). Mean age prior to the intervention (Jan 2013) was 6.84 years (SD = 0.36 years).

Our sample decreased from 572 children in W1 (pre-training) to 531 children in W4 due to attrition. This corresponds to an attrition rate of 7.2%. This attrition was due to children who switched to non-participating classes or schools, moved away, or were ill for a longer period of time during data collection; we did not exclude any available data. Attrition did not differ between the treatment and control groups, the sample in the treatment group shrank from 279 to 259 children (attrition rate of 7.2%), while the sample in the control group shrank from 293 to 272 children (attrition rate of 7.2% as well). Furthermore, we find that the estimated treatment effects remain stable when we restrict the sample to only those children who remain in the sample throughout all waves. Results for these estimations can be found in Tables S13–S15.

We also tried to conduct another randomized field study in Switzerland but failed to do so because the relevant school authorities were not able to ensure randomization of school classes into treatment and control classes: several schools/classes were only willing to participate under the condition of being assigned to the control group.

1.2 Supplementary Details on the Working Memory Training

Procedures

The treatment in our study consisted of a daily WM training session that primarily took place during the first or second lesson at school over a period of 25 school days. The training was embedded into the classes' normal school routine. In each class, the teacher who covered the entire curriculum for the first grade also oversaw the study. The children thus considered the WM training to be a normal exercise unit, similar to when the teacher introduces new exercise units in a subject such as math, reading, or writing in the classroom. The teacher was present during the lessons when the WM training took place. The children also remained in their regular classroom and conducted the training sessions at their desks. This minimizes Hawthorne type effects because it ensures that the children viewed the WM training simply as a usual exercise unit in the context of their daily lessons, in which the sequential introduction of new learning content during the school year is part of normal school routine.

The first training session had an introductory character during which procedures and software were explained. The subsequent 24 lessons served as actual WM training sessions. The time frame for each training session was one school lesson, i.e., 50

could not collect data in W1–W4 for these children. The participation of roughly half of these children in the working memory training without consent was possible because the school authorities viewed the training as part of regular teaching.

2. Among the 572 children, 6 children participated in the baseline data collection (W1) somewhat after the start of the working memory training (because they were not available — due to illness — when the other children participated in this data collection). All reported effects of working memory training remain stable if we exclude these children from the data analysis.

minutes. During that time, every child had to pick up his/her computer as well as an external mouse and a headphone from the case, start the software, log-in, try to solve the training exercises, log-out, and put the notebook back to its pre-specified location. The net time available for training thus amounted to about 30 minutes per lesson.

The class teacher and one trained research assistant per class who helped the teacher (e.g., in distributing the notebooks, supporting the children during log-in, solving technical issues, ensuring compliance with the training protocol, and preparing a documentation of the training, including special events during training sessions) supervised the children.[3] The assistants also helped in preparing a comprehensive documentation of the training.

Parents' Information

All parents, regardless of whether their children were in the treatment or the control group, obtained written information that the study consisted of several building blocks that involved computer-based and non-computer-based components and that — for scientific reasons — children undergo different combinations of these building blocks. The parents did not receive information regarding the children's assignment to the specific building blocks, i.e., they were not told whether their child was in the control or the treatment group. In fact, we did not even speak of treatment or control group in our information material for the parents, i.e., the parents did not have the notion of discrete and distinct treatment and control groups in their mind. In other words, we did not inform parents about the treatment assignment of their children, and we also did not provide information that would have enabled them to infer the treatment assignment. In addition, because we introduced the working memory training smoothly into the school curriculum such that it appeared as a natural part of the curriculum, the *children* were also unaware of whether they were part of a treatment or control group.

Hardware

Schools were equipped with one notebook for each child in the treatment *and* the control groups as well as large wheeled cases for storage, charging, and transportation of the notebooks. The cases also contained external mice and headphones for each child. For the treatment classes, each notebook was labeled with the child's name and his/her user account for the WM training software. The control group had no access to the WM training software.

Children only worked with the external mouse to ensure that the training group could not gain experience of any kind with an input device similar to the touchscreens used for the outcome measure tests in the data collection phases (see Section 1.3).

Software

The WM training software used for the treatment was "Cogmed RM"[4] in an offline version with German instructions. It provides an age-specific user-interface, adaptive levels of difficulty, and a built-in incentive game (see below). The software requires

3. The assistants were university students who were familiar with the working memory training software.

4. Cogmed and Cogmed Working Memory Training are trademarks, in the U.S. and/or other countries, of Cogmed Inc. (www.cogmed.com).

the user to fulfill a certain set of tasks that consist of remembering sequences of information (e.g., numbers, locations) under various conditions. We excluded three of the thirteen different tasks available in the software because they contain letters or syllables that require reading abilities and knowledge about alphabetic characters that had not yet been introduced in all classes at the time of the WM training. Apart from this change (and the small reduction in trials, see below), we complied with the software provider's required protocol.

Of the ten tasks implemented, two consisted of remembering spoken digits and, hence, focus on *verbal* WM capacity. These two tasks were very similar backward digit span tasks. The remaining eight tasks were based on remembering sequences of locations and visual information, and, thus, focused on *visuo-spatial* WM capacity. Due to the stronger emphasis on visuo-spatial relative to verbal WM training, we thus would expect larger improvements in visuo-spatial WM capacity.

Five of the ten training tasks were *simple span* tasks, as they only required storing and recalling information sequences of varying length. The remaining five tasks were *complex span* tasks because they contained at least one element of processing of stored content prior to recalling (e.g., numbers must be recalled in backward order or locations are moved before they have to be recalled).

The level of task difficulty was adapted based on the child's previous performance. After a few correctly (incorrectly) solved trials, the level of difficulty increased (decreased). A daily training session consisted of six (varying) modules of 12 trials each (resulting in 72 trials per day).[5] When the children had finished the six modules of a training session, they played a few trials of a fun game called "RoboRacing". This is a feature built into the software and helps motivate children to participate in the WM training tasks.

Note that the training software was only available for the children during the five weeks of the intervention period. After this time, the login credentials for the software became invalid and no further training was thus possible. The software is, in principle, commercially available but was not so for the German market at the time of our intervention. Therefore, a further use of the training software after the time of our intervention was practically impossible (although the notebooks remained at the participating schools).

Cost Estimate

We estimate the costs of our intervention in a back-of-an-envelope calculation to be around US$ 300 per child. Our estimated costs include the cost for a software license (US$ 20), the cost for notebooks or tablets of around US$ 250 per child, and a budget for teacher training of US$ 30 per child (assuming an intensive training session for teachers lasting for four hours costing US$ 600 per teacher and 20 children per teacher as an average class-size).

1.3 Supplementary Details on the Data Collection

The main data was collected at four points in time: wave 1 took place immediately before the intervention (W1), wave 2 took place shortly (4–5 weeks) after the intervention (W2), wave 3 took place 6 months after the intervention (W3), and wave 4

5. The usual training protocol of Cogmed recommends 15 trials per module; we decreased the number of trials to 12 in order to fit the training in one school lesson (taking the time needed for picking up and bringing back the notebooks into account).

took place 12–13 months after the intervention (W4). In each wave, we collected several computer-based outcomes that served the purpose of measuring the consequences of WM training on skills. We describe these outcome measures in detail below. In addition, we administered questionnaires to teachers and parents. In W4, we also asked the children a few questions after the computer-based tests.

The data collection was run by a professional data collection service provider experienced with conducting research projects in these settings. The tests were conducted outside the classroom; both the children from the control and from the treatment groups participated in the tests. The data collection was conducted by interviewers experienced in standardized testing procedures and in working with children of that age. They were trained in an eight-hour training session run by the data collection service provider together with the authors of this study. Importantly, the interviewers involved in administering the tests to the children (i.e., the employees of the data collection service provider) were blind to the children's assignment to the treatment conditions. The teachers were not involved in the design and the conduct of the tests, and they did not even know the content of the tests, i.e., it was impossible for the teachers to prepare the children for the tests. Finally, three years after the treatment, we also conducted a survey on school track choice, details see below. This study reports all measures in this project up to and including the survey on school track choice.

Testing Procedures

The tests were administered using computers with 22" touchscreens and headphones. The instructions were auditive via headphones and supported by visual demonstrations shown on the screens. The children entered their responses using touchscreens that were easy to handle.

The tests were run in two blocks of about 30 minutes, scheduled on two consecutive days, primarily during the first or second lesson of the school day. Tests were done in groups of five children supervised by one "interviewer". Each child sat in front of a touchscreen positioned in a standardized way on the desk and had headphones to listen to the instructions. All children started at the same time but could complete the test at their own pace. The whole testing procedure for a class lasted for about three to four school days.

Note that (a) our testing procedure guaranteed a high degree of standardization, especially through the instructions via headphones, and (b) by using large touchscreens as the method of data input, we ensured that there was no advantage for the treatment group as the computer-based WM training was run not with touchscreens but with a smaller notebook and external mice.

All tests were pretested in a primary school that did not participate in the study. All children received a small toy for participating in the evaluation wave. Over the four data collection waves, the tasks became generally more difficult to account for the increase in children's abilities over time.

Parent Questionnaires

Parent questionnaires were only distributed in the data collection waves W1 and W3, i.e., before the intervention and 6 months after the intervention. Parent questionnaires included questions on socio-demographic characteristics of the family, parental behavior (also towards the child as well as educational goals) and parental characteristics as well as the child's personality, attitude towards school, general health, and everyday behavior (including SDQ). Parents filled out 467 out of 572 parental questionnaires in W1 (82%) and 419 out of 544 in W3 (77%).

Teacher Questionnaires

In each data collection wave, teachers filled out a questionnaire. These questionnaires contained questions on children's characteristics and behaviors and teacher characteristics and behaviors, as well as experience with and expectations about the intervention (if they were in the treatment group). In particular, we asked the teachers in every data collection wave to assess each child's self-regulation abilities using several questions (see Section 1.4). We achieved a 100% return rate for the teacher questionnaire in all four evaluation waves.

Survey on Secondary School Track Choice

In addition to the main data collection, we administered a short survey to parents and teachers when children were in the final grade of primary school (grade 4). This survey was conducted in April 2016, (i.e., three years after the treatment) and asked parents about the secondary school track the child was enrolled for grade 5. The questionnaire was sent to participating schools and teachers distributed and collected questionnaires. Parents submitted their answers in a sealed envelope, so that the teacher could not see their response. Teachers also provided a recommendation which school track the child should attend. However, in our study context the school track decision is taken by the parents, and teachers' recommendation is not binding for the children.

We received a total of 393 questionnaires (69% of the sample in W1 or 74% of the sample in W4). This attrition was due to reasons such as children moving away from the city of our study or parents not answering our follow-up questionnaire. Importantly, there was no difference in attrition between treatment and control group: If we regress participation in the school track choice survey on the treatment condition, gender of the child, age of the child, and school fixed effects we do not find any significant treatment effect regardless of whether we use a linear probability regression or a probit regression. Thus, we conclude that there was no significant difference in attrition between treatment and control group. We nevertheless control for any residual nonsignificant differences in attrition by applying inverse probability weighting when we analyze the impact of working memory training on secondary school track choice (see Table 4, column (5), as well as additional analyses in Tables S21–S23).

Data Availability Statement

The data for this publication have been collected in a project that has compiled a large set (and combination) of children's abilities, preferences, and family (socio-demographic) characteristics (see Sections 1.3 and 1.4), and thus represents highly sensitive data. This dataset cannot be made available for data protection reasons. In addition, parental consent for data usage only covers strictly scientific purposes. The restriction to scientific purposes was also necessary to comply with data protection requirements and use of the data for strictly scientific purposes cannot be guaranteed if the dataset is made (publicly) available. Not all the data collected in this project are analyzed for this publication, see Section 1.4 for details. Researchers interested in replicating our findings can get access to the data set after filling out a research agreement with us.

We confirm that in the paper and the Supplementary Information, we have reported all measures, conditions, data exclusions, and how we determined our sample sizes.

1.4 Supplementary Details on Outcome Measures

This section describes the tests that we used to measure the skill-consequences of WM training. WM capacity was assessed by one simple and two complex span tasks. For assessing educational achievement, we tested arithmetic skills, geometry skills, and reading comprehension. To measure important components of children's IQ, Raven's Coloured Progressive Matrices test (Raven 1995) was administered. For the assessment of self-regulation related abilities, we used a go/no-go task (adapted from Gawrilow and Gollwitzer 2008) and a letter discrimination task ("bp task", Esser, Wyschkon, and Ballaschk 2008). We also measured children's reading habits, and time and risk preferences using computer-based and non-computer-based tasks, but these measures are not part of the present study. For the ease of interpretation and comparison, we standardize all test scores to mean = 0 and SD = 1, separately by test and wave. Histograms of the distribution of all raw test scores (i.e., before standardization) for the evaluation waves W1–W4 are displayed in Figures S12–S15.

Working Memory Tests

We adopted three different tasks for measuring the different facets of children's WM capacity. To avoid task-learning effects, we chose tasks distinct from the WM training tasks. The children's WM capacity was measured by a *verbal simple span* task, a *verbal complex span* task, and a *visuo-spatial complex span* task. Especially the verbal complex span task and the visuo-spatial complex span task clearly differ from the WM training tasks.

The test scores in a given wave were constructed as follows. We summed up the number of correctly solved item series weighted by each series' difficulty, which is defined by the series' length (i.e., number of items in the series).[6] We standardized this score to mean = 0 and SD = 1. Because we expected the children to naturally improve their WM capacity when growing older, we increased the difficulty of the WM tasks across the four waves W1–W4 in order to avoid ceiling effects.

The *verbal simple span* task was a simple forward span short-term memory test. In this test, the child first had to listen to a sequence of one-digit numbers in the range of 1 to 9. After each sequence, a three by three grid with the digits 1 to 9 appeared on the screen and the child was asked to indicate the digits heard in the correct order (see Figure S1). The difficulty level in this task can be increased by increasing the number of items in the sequence of one-digit numbers that need to be recalled in the correct order.

In the *verbal complex span* task, the child first listened to a sequence of words, each of which described an object. After each object mentioned, the child had to decide whether the object is an animal or not by pushing a button "Animal" or "No animal". Due to these "interruptions", the task becomes a complex span WM task. After the sequence was finished, a three by three grid with pictures appeared on the screen. The pictures show the objects mentioned in the sequence as well as other, irrelevant objects. The child had to click on the pictures of the objects corresponding to the order in which the objects were previously mentioned (see Figure S2). The difficulty of this task was varied by varying the number of objects mentioned in a series.

The *visuo-spatial complex span* task was a complex span task measuring visuo-spatial WM capacity. First, the child was presented a sequence of "stimulus screens".

6. We get similar results if we use the non-weighted sum of the correctly solved items series as a measure of working memory capacity.

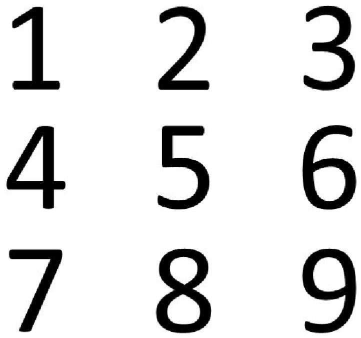


Figure S1: The Screen to Enter Answers for the Verbal Simple Span Task

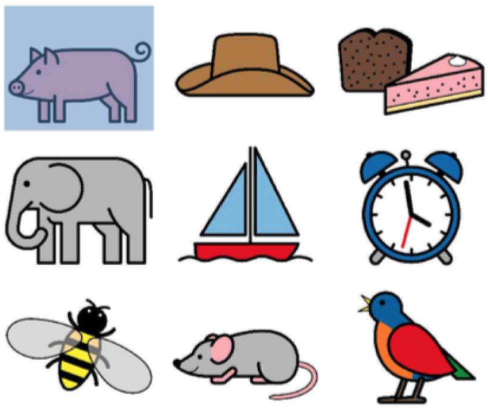


Figure S2: The Screen to Enter Answers for the Verbal Complex Span Task

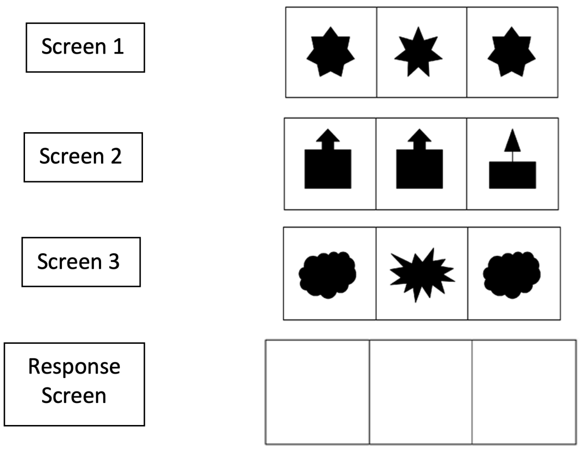


Figure S3: Stimulus and Response Screens in the Visuo-spatial Complex Span Task

A stimulus screen contained three items; the child had to detect the item shaped differently and click on it (see Figure S3). Then, a new stimulus screen appeared and the child again had to click on the deviant shape, etc. Figure S3 shows an example with three different stimulus screens after which the response screen appears which contains an empty grid. The child had to enter the position of the deviant items on the previous three stimulus screens in the correct order on the response screen. In Figure S3, for example, the correct response is to click “center”, “right”, “center” on the response screen. The difficulty level in this task is varied by varying the number of stimulus screens before the response screen appears.

Educational Achievement Tests

Educational achievement was assessed by testing for arithmetic skills, geometry skills, and reading skills. We increased the difficulty of the educational tasks across the four evaluation waves W1–W4 to avoid ceiling effects due to children's development in scholastic skills with age.

Arithmetic skills: Arithmetic skills were assessed using three different subtasks: a number sense task, an auditory arithmetic task, and a written arithmetic task. The children had to infer/compute a correct number from the presented stimuli in all three arithmetic tasks. Children had to enter the number in an input device on the computer screen that looked like a pocket calculator (see Figure S4). For example, if the child thought that the correct number is '23' she had to tap first a '2' so that this number appeared in the empty top left rectangle of the device; then she had to tap on the number '3' on the input device so that the number '23' appeared in the top left rectangle of the device. If the child was satisfied with her answer, she had to confirm it by tapping on the green arrow on the top right corner. If the child wanted to correct her answer, she could do so by tapping on the red "X" on the bottom left corner of the input device. Note that the children also had to identify a correct number in the geometry task described below, again using the same input screen in that task.

Figure S4: The Input Device for the Arithmetic and Geometry Tasks

Number sense task: In this subtask, the children were presented a number of balls on a two by ten grid that was only shown for 1.7 seconds (see Figure S5 showing several different examples with various levels of difficulty). In general, the display time was too short to count all balls before they disappeared. After the grid had disappeared, the children had to type the correct number of balls in the grid.

A two by ten grid with the subdivision at 5 is used in the first grade in the participating primary schools to teach numbers and calculations. To solve the number sense task, children need to be familiar with the number range up to 20, and a good understanding of the logic of the grid is useful. Because the children could not count the balls due to the short display time, they had to capture the pattern of the balls. This involves the assessment of structures as well as the detection of possible subgroups and the number of balls per subgroup. Children had to sum up the number of balls from different subgroups or use subtraction in cases where only a few balls were missing in the grid. For example, consider the first grid below (see Figure S5) with 18 balls: Depending on the child's mathematical experience, different strategies are

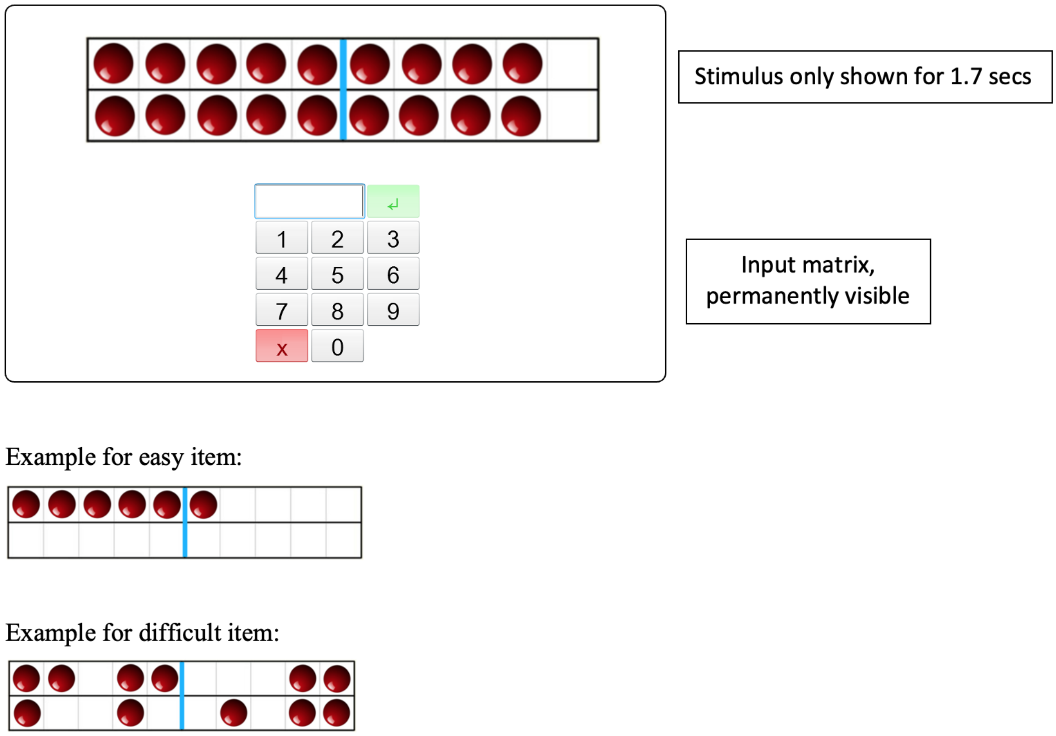


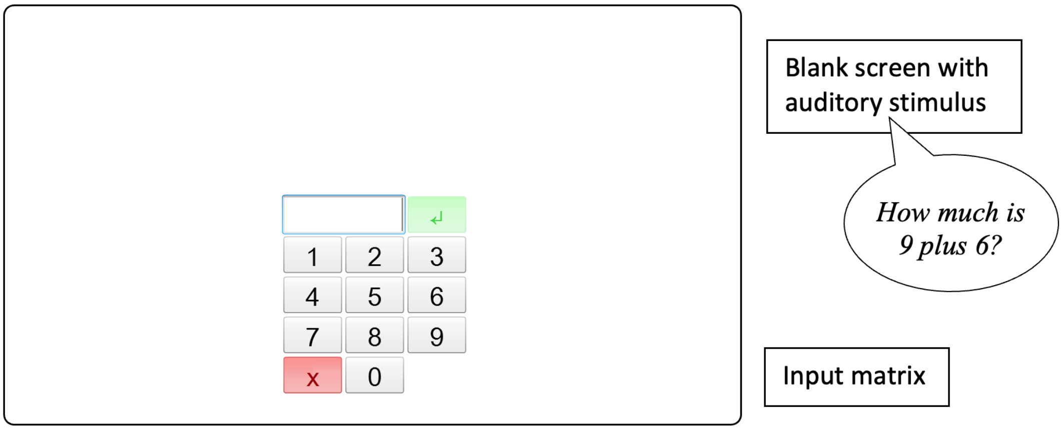
Figure S5: Number Sense Task, Screenshot Plus Two Further Examples

possible in this grid. A child knowing that 20 balls would fit in the grid and noticing that 2 balls are missing at the right end of the grid could compute $20 - 2 = 18$ to arrive at the correct solution. Another child might recognize 10 balls (2 rows with 5 balls each) in the left half and 8 balls (2 rows with 4 balls each) in the right half of the grid. This child will reach the correct solution by mentally computing $10 + 8$ after the balls have disappeared. The third grid below (see Figure S5) gives an example of a rather difficult item. Children had to quickly recognize and structure four groups of balls containing different numbers of balls each. The children had to capture the number of balls in each subgroup simultaneously and to correctly sum up $3 + 3 + 1 + 4$. As one of the fundamental steps in mathematical development at this age is to replace counting strategies by computing strategies, it is important that the display time was too short to be able to count the balls.

The number of balls and their distribution within the grid varied across the items and evaluation waves and was adjusted to the development of children's mathematical skills. The size of the grid, however, remained constant over time.

Auditory arithmetic task: This subtask measures arithmetic skills for addition and subtraction of two numbers (see Figure S6). Computational tasks were presented over the headphone (e.g., "How much is 9 plus 6?"). Children had to enter their answer into the input matrix. Each item in this task contained two numbers to be added or subtracted. Each evaluation wave contained 10 of these auditory arithmetic items.

The difficulty level was adapted to the school curriculum, e.g., with regard to the number range: In W1 and W2 the number range was up to 20, while in W3 and W4 it expanded to 100. Other major changes across waves are the increase in complexity of the mental operations and the need for numerical comprehension. Moreover, for the more difficult items, such as "92 minus 17", children needed to compute intermediate steps: First, many children would compute 92 minus 10 and keep the intermediate result 82 in mind. Then, they would subtract the remaining 7 from 82, leading to the final result.



Example for easy item:
"How much is 2 plus 5?"

Example for difficult item:
"How much is 92 minus 17?"

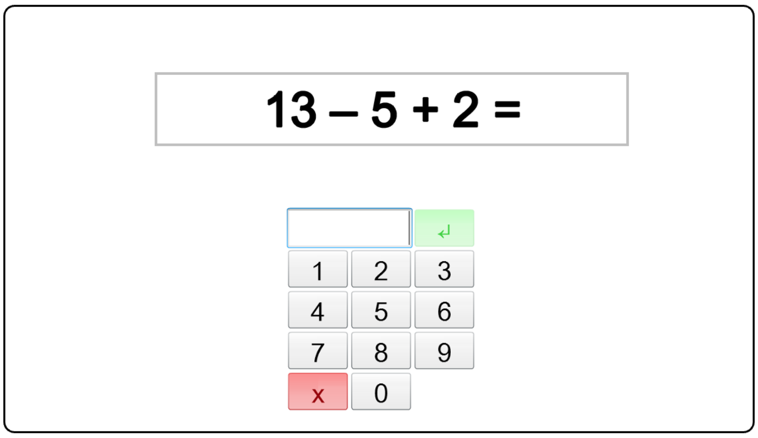
Figure S6: Auditory Arithmetic Task, Screenshot Plus Two Further Examples

Written arithmetic task: In contrast to the auditory task, the arithmetic problems in this subtask were not presented over the headphones but displayed on the screen. Most problems contained *more* than two numbers that needed to be added or subtracted; the reason for this is that we tried to avoid having children draw a result from their longer-term memory without computing. Each arithmetic problem was visible on the screen during the whole trial (see Figure S7). Because of this (i.e., because the subjects did not need to recall the numbers from memory), the difficulty level of the required mathematical operations was generally set to be higher than in the auditory task. Children were, for example, required to add and/or subtract three or four numbers. The difficulty level was also adapted to the curriculum, analogously to the way it was done in the auditory arithmetic task.

Computation of final arithmetic test score: For each of the three subtasks (number sense, auditory and written arithmetic tasks), we added up the number of correctly solved items and standardized each subtask score to mean $= 0$ and SD $= 1$ within each wave. We then added up the three standardized subscores and standardized this composite score to mean $= 0$ and SD $= 1$ to achieve comparability to the other test scores used in our analysis.

Geometry skills: Geometry skills were assessed by a test that required the children to assess how many simple-shaped objects—such as triangles, squares, or rectangles—fit into a larger geometric object (see Figure S8). Depending on the size and the shape of the larger geometric object, this task can be made harder or easier.

The task contained 10 items in each evaluation wave. The difficulty level varied across items and evaluation waves. Difficulty varied along various dimensions. Consider the easy item shown in Figure S8 (the red square): Children could solve the problem without any mental rotation of the small square. Furthermore, the larger object is subdivided into two components, making the task even easier. In contrast, for the first item shown in Figure S8 (the pink rectangle), children had to mentally rotate the small object to solve the question. For the difficult item in Figure S8 (the green



During the trial, the written stimuli were permanently visible

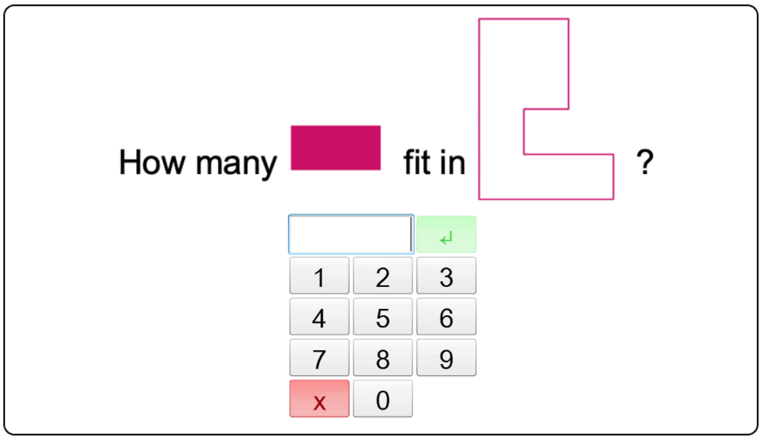
Example for easy item:

1 + 5 + 4 =

Example for difficult item:

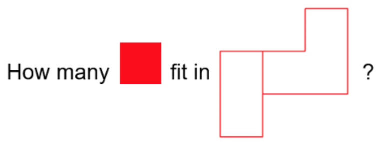
100 – 43 – 20 + 43 =

Figure S7: Written Arithmetic Task, Screenshot Plus Two Further Examples



Stimulus and Input matrix permanently visible

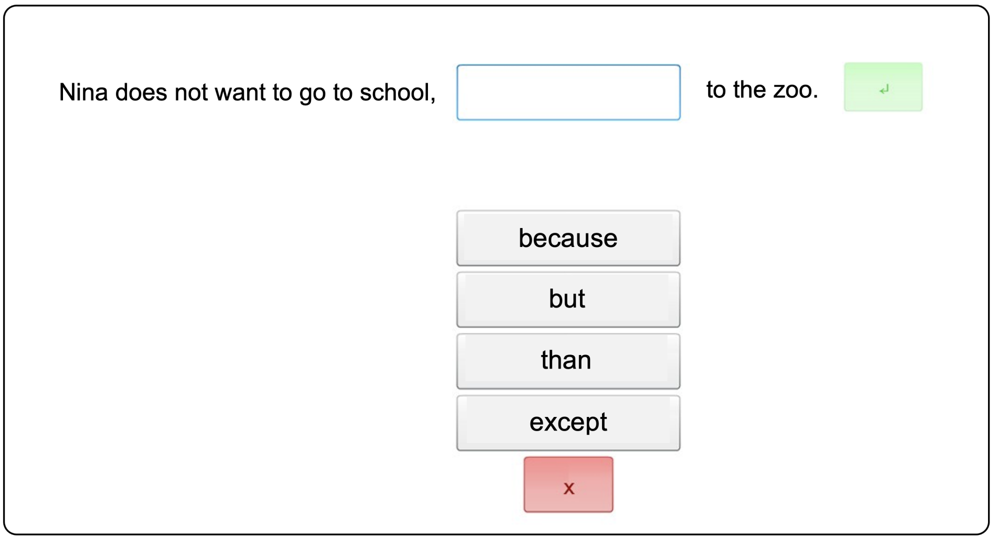
Example for easy item:



Example for difficult item:

Figure S8: Geometry Task, Screenshot Plus Two Further Examples triangle), children hat to mentally rotate the triangle, store the number for subparts and keep track of which parts were already counted when filling the larger geometric object.

Reading comprehension skills: Reading comprehension was assessed by a sentence comprehension test in single choice format (we also elicited children's reading abilities in the teacher questionnaire, see Section 1.3 — results are generally in line with the findings for the computer-based test). On the screen (see Figure S9), a sentence with one gap was presented in a line. To fill the gap, the children had to choose from a list of four alternatives presented below the gap. Tapping on one of the words in the list made it appear in the gap. Children could correct their choice by using the red X button below the list. Children had to confirm their choice by tipping on the green enter-button right beside the sentence.



Example for easy item:

Leo is at the ____.
(answer options: mum, lake, hat, name)

Example for difficult item:

In good weather, Fabian takes the bike ____ he better likes to go by foot in bad weather.
(answer options: while, during, as if, without)

Figure S9: Reading Comprehension Task, Screenshot Plus Two Further Examples

Generally, there was only one word missing in the sentence. In W3 and W4 there were also a few gaps to be filled with a combination of two short words. The difficulty of the items was multidimensional. It varied within a test, and in particular between the evaluation waves, where it was adjusted to the curriculum. In W1 and W2, the test contained 10 sentences consisting of 3 to 9 words per sentence. The words only contained those letters that had already been introduced to the children in earlier lessons during the school year. As most children become much faster in reading before W3, the reading comprehension task contained 16 sentences with 4 to 15 words per sentence in W3, and 16 sentences with 4 to 16 words per sentence in W4.

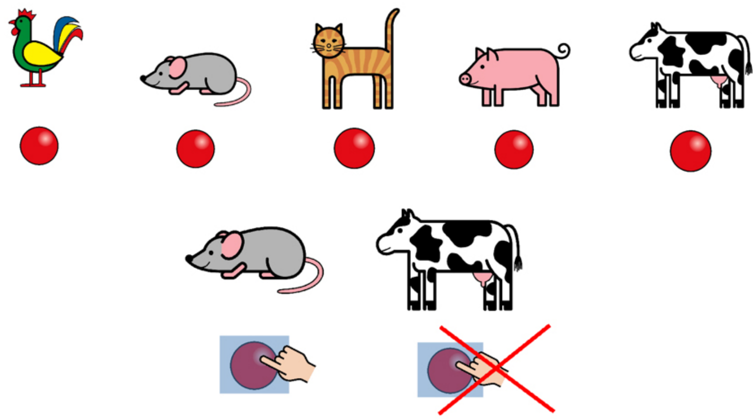


Figure S10: The Animals and the “Go-Button” in the Go/No-Go Task

Fluid IQ

Children’s fluid IQ was measured using a set of Colored Progressive Raven’s Matrices (Raven 1995). While no single measurement tool will cover all aspects of a construct like fluid IQ, there is probably a broad consensus that the Raven’s Matrices task captures important aspects of fluid IQ. We used two different sets of 17 items in W1/W3 and W2/W4, respectively. The child was shown a box with a pattern and had to choose which one out of six smaller patterns would fit into a missing part of the large pattern. The outcome score used in the main analysis is the standardized sum of correctly solved items.

Go/No-Go Task

To measure inhibitory abilities, we employed a go/no-go task that was adapted from Gawrilow and Gollwitzer (2008). In this task, the child had to push a red button on the touchscreen every time one of four different animals appeared on the screen (rooster, mouse, cat, pig — see Figure S10). However, the children were told *not* to push the red button for one other animal (cow). The procedure of the task is as follows: The red button is displayed on the touch screen throughout the task. In addition, the children first see an X in the middle of the screen for 0.6–1.2 seconds (these times randomly vary across items but are equal across waves). Then the picture of an animal appears with a display time of 1.55 seconds and a time slot for reaction of 1.55 seconds (the display time for the animal was reduced to 0.65 seconds in W2, W3, and W4.) In this time window, the children must decide whether to push the button and to implement the button press. Subsequently, the children again see the X, then the picture, and so on. In total, 50, 60, 70, and 80 items were presented in W1, W2, W3, and W4, respectively. In W1 and W3, the pictures were animals as described above. The pictures were vehicles in W2 and W4 (go = car, train, ship, airplane; no-go = truck). Because the target animals (or vehicles) occur much more frequently than the non-target animal, and the time window during which a decision can be made is short, the children are put in the “go-mode”. In other words, the pre-potent impulse is to push the red button. A key challenge in this task is, therefore, to inhibit the pre-potent impulse when a non-target animal appears.

We measure performance in this task in three ways. First, we simply compute the commission errors (i.e., the number of times a child fails to inhibit the “go-response” when a no-go item is displayed), multiply by -1, and standardize the score to mean

$= 0$ and SD $= 1$ within each wave. Thus, a higher score indicates better performance in the task (i.e., fewer mistakes). Second, we compute the d'-measure of performance. The d'-measure is the standardized fraction of commission errors in the no-go items subtracted from the standardized fraction of correct responses in the go items. We again standardize this score to facilitate better interpretation. We find similar treatment effects on the d'-measure, i.e., a significant increase in the performance of the treatment group relative to the control group in W4 (W2: d = 0.118, p = 0.410; W3: d = 0.071, p = 0.619; W4: d = 0.475, p < 0.0001). Third, we can analyze the omission errors (i.e., the number of times a child fails to push the red button when a go item is displayed) as a measure for "attention". We multiply the number of omission errors by -1, and standardize the score to mean = 0 and SD = 1 within each wave. Hence, a higher score indicates better performance in the task (i.e., fewer mistakes). Again, treatment effects on omission errors are very similar to those for commission errors, with large and significant improvements in W4 (W2: d = 0.282, p = 0.109; W3: d = 0.133, p = 0.357; W4: d = 0.416, p = 0.001).

Letter Discrimination Task

Our letter discrimination task ("bp task") measures attentional stamina and is taken from Esser, Wyschkon, and Ballaschk (2008). In this task, the child saw three lines filled with the letters "b", "d", "g", "q", "h", and "p", in total 45 letters on the touchscreen (see Figure S11 for an example of such a screen). The child had to go through the letters from left to right, row by row, and tap on all "b"s and "p"s without accidentally marking any other letter. The two target letters "b" and "p" were displayed at the top of the screen in a salient form so that the child is always reminded of the goal in this task in every single trial.

The screen emptied after 30 seconds, and a new screen appeared. This was repeated for 18 times (only 12 times in W1). To construct the outcome score we add up standardized scores for both types of errors (i.e., marking a wrong letter and failure to mark a "b" or a "p"). This score is then again standardized to mean $= 0$ and SD $= 1$ within each wave and multiplied by -1. Thus, a higher score indicates better performance in the task (i.e., fewer mistakes).

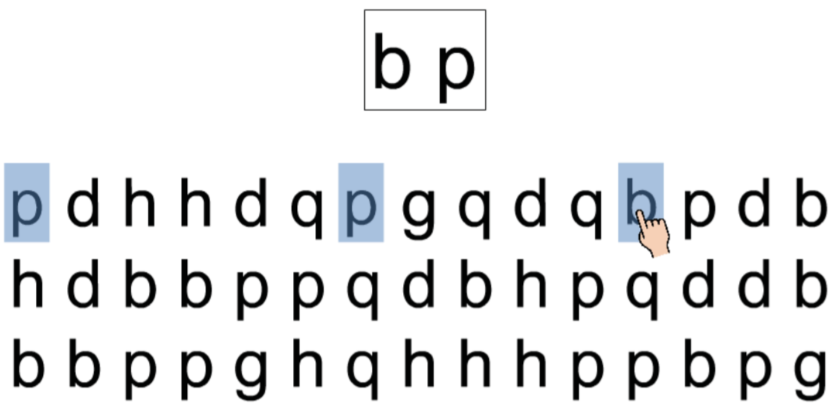


Figure S11: Example of a Screen in the Letter Discrimination Task

Overall Self-regulation

All above-presented tasks for measuring the skill consequences of WM training are based on objective tests and not on subjective assessments. One of these tasks — the go/no-go task — provides a measure of the extent to which the children are able to inhibit pre-potent impulses. As the ability to inhibit these impulses is often viewed as a component of self-regulation or self-control, we decided to complement this measure with teachers' assessments of the children's self-regulation skills. We are aware of the fact that the teachers' subjective assessments may be a less reliable measure than objective tests. However, if WM training affects the objective test measure and the subjective measure in similar ways, our confidence in the reliability of the treatment effect is strengthened.

In each wave, teachers assessed the self-regulation abilities of each child in their class by answering the questions listed below in each data collection wave. Questions 1–5 were answered by means of a 7-point Likert-type scale with 1 = "does not apply at all" and 7 = "fully applies". The answer options for questions 6 and 7 are indicated below.

1. The child works in a concentrated and enduring manner.
2. The child makes a large number of mistakes due to inattention (reverse coded).
3. The child has a lot of self-discipline.
4. The child has trouble waiting for his/her turn (reverse coded).
5. The child disturbs class instruction often (reverse coded).
6. Please indicate for each child how often he/she forgot his/her homework or did not do his/her homework despite having an assignment in the last six months? (1 = "never forgot homework" to 7 = "forgot homework often") (reverse coded)
7. How do you rate the child with respect to patience? (1 = "very impatient" to 7 = "very patient")

Importantly, teachers had to answer these questions subsequently for each child in their classroom, i.e., they first rated all children in their class on item 1, then all children in their class on item 2, etc. This makes it very unlikely that item correlations within child (or improvements on several dimensions for a child) were driven by teachers just using the same point on the scale for all items for a single child (e.g., by seeing child A's name and then clicking on a "6" for each item for this child — this was not possible in our survey design).

The items above were developed with the purpose of assessing young children's overall self-regulation skills in a classroom context. The items are partly based on (or adapted from) the Strengths and Difficulties Questionnaire (SDQ) proposed by Goodman (1997) and the Self-Control Scale developed by Tangney, Baumeister, and Boone (2004), which was translated into German and validated by Bertrams and Dickhäuser (2009).

We conducted a factor analysis of the items mentioned above (see Table S18). The results of this analysis show that all items exhibit considerable loadings on a single factor. All other factors have eigenvalues of less than 1. Table S18 shows this result for the teachers' answers in W1; the same results basically hold for all waves.

We used the standardized sum of the standardized answer values of the seven questionnaire items listed above as the dependent variable when estimating the effect of the WM training on overall self-regulation abilities.

1.5 Supplementary Details on the Data Analysis

Details on Descriptive Statistics

Supplementary Table S23 presents descriptive statistics for the whole sample. Overall, 15 classes (279 children, i.e., 49%) were assigned to the treatment group and 16 classes (293 children) to the control group. About 49% of the children were male, mean age at the beginning of the year (i.e., on January 1, 2013) was 82 months (6.84 years, SD = 0.36 years). Gender and age variables are taken from parental consent forms and are therefore available for all children. The variables migration background and language problems stem from the teacher questionnaire administered in W1, the variables household income and mothers' university degree stem from the parental questionnaire in W1. The information about secondary school track choice is taken from a separate parental survey administered three years after the intervention (see Section 1.3).

Estimating the Treatment Effect

To estimate the treatment effect of the WM training, we regress outcome scores measured after the training period on a treatment indicator. We also include school fixed effects (because randomization was conducted within schools) and some basic control variables (gender, age in months on January 1, 2013, and age in months at the relevant test days). Furthermore, we conducted other treatments in the same sample, with a randomly chosen part of the WM treatment group and a randomly chosen part of the control group, a learning software training and a self-regulation training (Schunk et al. 2022). The other treatments are unrelated to the WM training. Of the $n = 279$ children in the WM treatment group, $n = 145$ also received self-regulation training, and $n = 134$ received only WM training (pure WM training group). Of the $n = 293$ children in the control group, $n = 133$ received only self-regulation training, $n = 22$ received learning software training, $n = 37$ received self-regulation *and* learning software training, and $n = 101$ received regular classroom teaching (pure control group). We control for the other treatments as well as for their interactions in all our estimations (cf. Muralidharan, Romero, and Wüthrich 2023). Due to this econometric specification, the treatment effect of the WM training is identified by the comparison between the pure control group and the pure WM training group. Finally, in the estimation of each outcome score we also control for the pre-training baseline (W1) level of that score. This is done instead of using the difference-in-differences estimator. The justification for this follows from the fact that "our" estimate of the treatment effect (by controlling for baseline scores) has approximately a variance of

$$\frac{2\sigma^2(1-\rho^2)}{n}, \tag{1}$$

while the difference-in-differences estimator has a variance of

$$\frac{4\sigma^2(1-\rho)}{n}, \tag{2}$$

where ρ is the autocorrelation of the outcome measures and n is the number of observations. Thus, the advantage of the method we use is that the variance of the estimate is smaller, i.e., the treatment effect is estimated with higher precision if $\rho \neq 1$ (Frison and Pocock 1992; McKenzie 2012).

Adjusting p-Values for Multiple Testing

We estimate the treatment effect on several outcome variables at several points in time, i.e., we have a relatively large number of hypotheses. This boosts the probability of falsely rejecting null hypotheses. If we keep the significance level at 5% for each null hypothesis we test, this implies that the probability of wrongly rejecting each null hypothesis (i.e., detecting a "significant" effect even if there is none) is 5%. However, the probability of rejecting at least one out of many null hypotheses is much larger than 5%. Thus, the probability of over-rejecting (i.e., rejecting null hypotheses that should not be rejected, i.e., finding a significant effect where there is none) increases with the number of hypotheses we test. This has to be corrected in order not to arrive at wrong conclusions.

We refrain from using simpler multiple testing corrections such as Bonferroni (1935) or Holm (1979) because the method by Romano and Wolf (2005) is more powerful, since it accounts for the dependence structure of the test statistics (Clarke, Romano, and Wolf 2020). We apply the Romano-Wolf stepdown procedure to control the family-wise error rate (FWER, see Romano and Wolf 2005) — a technique which is increasingly used for large-scale intervention studies (see, for example, Heckman et al. 2010; Campbell et al. 2014; Gertler et al. 2014). Furthermore, we use an efficient method to adjust p-values according to this stepdown algorithm (Romano and Wolf 2016). In addition, we also combine this method of controlling the family-wise error rate (FWER) with the BRL (biased-reduced linearization) correction method, using code provided by Pustejovsky (2023). This method accounts for potential biases in estimation of standard errors when the number of clusters is relatively small (Bell and McCaffrey 2002).

For applying the multiple testing correction, we decided to group our outcomes into the following natural families, following our main hypotheses for treatment effects of the working memory training: 1) working memory outcomes (verbal simple span, verbal complex span, visuo-spatial complex span), 2) educational spillover outcomes (arithmetic, geometry, reading), 3) spillover effects on general cognitive skills (Raven's IQ), and 4) spillover effects on general noncognitive skills (Go/No-go task, bp task). In addition, we always include all three post-treatment outcome waves (i.e., W2, W3, and W4) in the respective families, rendering some of these families quite large. The reasoning behind this partitioning is as follows. First, any spillover effects on other skills would presumably build on direct improvements of working memory capacity ("near-transfer effects"). Second, these direct improvements in WM capacity could transfer to skills related to the school curriculum, such as arithmetic, geometry and reading, simply because children in the treatment group might benefit more from the regular classroom teaching. Third and fourth, based on the correlational evidence in the literature, improvements of working memory capacity might also transfer to more general sets of skills, namely general cognitive skills and general noncognitive skills. We ran M=10'000 bootstrap repetitions (stratifying on class-level and correcting standard errors using biased-reduced linearization). Subsequently, we apply the code to adjust p-values according to the stepdown procedure by Romano and Wolf (2005, 2016). The resulting p-values are reported in Table S9.

Yet, as our project does not have a pre-analysis plan, the choice of families for outcomes is apparently ex-post and potentially endogenous. While we believe that our preferred grouping of families is the most reasonable one, we also provide an additional multiple testing analysis in Table S10, using an even more conservative grouping of families, namely 1) direct effects and 2) (any) spillover effects. In our view, there should be little doubt that working memory capacity should be a separate family ("first stage" for any spillover effects). All spillover outcomes are grouped together in a second family. Even with this very conservative approach (controlling the FWER for a group of 6*3 = 18 outcomes), we find significant spillover effects on geometry (p = 0.073) and inhibitory control in the Go/No-Go task (p = 0.007). Treatment effects on Raven's IQ do no longer reach conventional levels of statistical significance but also remain quite close to the 10%-threshold (p = 0.136 in W3 and p = 0.114 in W4). Overall, our multiple testing corrections confirm that the treatment had both substantial direct effects on working memory capacity, especially in the visuo-spatial domain, as well as spillover effects on Geometry, the ability to inhibit pre-potent impulses in the Go/Nogo task, and — slightly less robust — on performance in the Raven's IQ task.

Treatment Effects on Overall Self-regulation

Working memory capacity is more than just the ability to temporarily store information — it also involves the capacity to process information in the presence of distracting impulses that are not conducive for the individual's goal. As such, WM capacity is closely related to inhibitory control and impulse control. Like WM capacity, inhibitory control and the ability to avoid goal-incongruent distractions are conceptualized as components of executive functions (EFs). Another component of EFs is self-regulation, which requires maintaining optimal levels of emotional, motivational, and cognitive arousal. Self-regulation behavior in a classroom context cannot be elicited with the objective computer-based measures used in our study. However, to obtain a measure for self-regulation ability in a classroom context, we asked the teachers to assess the children's self-regulatory abilities more broadly in a questionnaire using seven self-regulation items like "The child has problems waiting for his/her turn" or "The child disturbs class instruction often" (for details, see Section 1.4). A factor analysis on these items (shown in Table S18) reveals that teachers' responses can be captured by one factor — the children's overall self-regulatory ability. This measure is based on teachers' day-to-day experience with the children and thus has an empirical base, but it is also based on teachers' subjective perception of their experiences with the children. For this reason, we first validated the teacher ratings with the objective test results observed in the go/no-go task.

To assess the credibility of teachers' ratings, we computed the correlation between the performance objectively measured in the go/no-go task (averaged for each child over W1–W4)—that measures children's inhibitory abilities, which may well be considered as one important aspect of self-regulation—and the teachers' broader assessments of children's self-regulation skills (again averaged over W1–W4). This is based on the idea that if teachers' assessments contain an objective rationale, i.e., if they have a meaningful objective basis and are not purely subjective impressions, then we should observe a significantly positive correlation, which is indeed the case; the correlation between children's inhibitory skills measured in the go/no-go task and teachers' assessment of their overall self-regulation is 0.45 and 0.40 in the control and treatment group, respectively.

Based on this validation of teachers' ratings, it makes sense to examine whether the children in the treatment group are rated higher in terms of broader self-regulation. We find indeed that teachers rate the children in the treatment group as significantly better on "overall self-regulation" in W3 (d = 0.37, p = 0.040) and in W4 (d = 0.27, p = 0.026). The results of the corresponding regressions can be found in the Table S19. Thus, it appears that the WM training also led to a broader improvement in self-regulatory skills.

The Role of Working Memory Capacity for the Treatment Effect on Far-Transfer Outcomes

The key rationale behind our WM training intervention is that the training-induced increases in WM capacity will eventually enable the children to perform better in tasks that require WM capacity. Thus, it is natural to hypothesize that WM capacity is a mediator of the treatment effects of WM training on far-transfer outcomes. To gain insights into the quantitative importance of WM capacity as a mediator we conducted a mediation analysis.

The goal of this analysis is to decompose the total treatment effect of WM training on far-transfer outcomes into an effect that is due to increases in WM capacity and an effect that is due to other, unexplained factors. The total treatment effect is estimated by the equation

$$T_i^k = \delta_0^k + \delta_{WMT}^k WMT_i + \delta_X^k X_i + \varepsilon_i^k, \tag{3}$$

where T_i^k denotes the score of far-transfer outcome k of child i, WMT_i is the treatment indicator, and X_i denotes a vector of control variables such as age, gender, and the pre-treatment outcome score. The δ-parameters are to be estimated and ε_i^k is the error term. We carry out a decomposition analysis of the total treatment effect δ_{WMT}^k, focusing on those far-transfer outcomes that were significantly affected by the training: geometry, reading, Raven's IQ, and inhibitory ability in the go/no-go task.

Following Heckman, Pinto, and Savelyev (2013), and similar applications by Kosse et al. (2020) and Carlana, La Ferrara, and Pinotti (2022), our analysis is based on a linear production function for child i's transfer outcome k, T_i^k. Thus, we assume that T_i^k is a function of working memory capacity, WMC_i, a vector of unknown mediating variables, U_i, and a vector of pre-program control variables, X_i:

$$T_i^k = \alpha_0^k + \alpha_{WMC}^k WMC_i + \alpha_U^k U_i + \alpha_X^k X_i + \nu_i^k \tag{4}$$

In equation (4), α_{WMC}^k is a parameter vector denoting the effect of WM capacity (verbal and visuo-spatial) on transfer outcome k; α_U^k and α_X^k are parameter vectors related to the unknown mediating variables and pre-program control variables, respectively; ν_i^k denotes an error term that is independent of the mechanisms and predetermined variables.[7]

In the first step of the decomposition analysis, we estimate equation (4) based on the control group sample; the results are reported in Table S20 of this online ap-

7. Note that the parameters in equation (4) are not indexed by the treatment indicator. This reflects the assumption that the parameters are the same for treatment and control group. This is consistent with the findings by Heckman, Pinto, and Savelyev (2013), Kosse et al. (2020), and Carlana, La Ferrara, and Pinotti (2022).

pendix.[8] The significantly positive estimates of α^k_{WMC} for all outcomes k suggest that working memory capacity plays a significant role in children's far-transfer outcomes.

In the second step of the decomposition analysis, we estimate the treatment effect of the WM training on WM capacity, our mediation variable, by the following equation:

$$WMC_i = \beta_0 + \beta_{WMT} WMT_i + \beta_X X_i + \eta_i \quad (5)$$

The results of this regression are discussed in the results section of the paper (see Figure 1) and are reported in detail in Table S3.

In the third step, the decomposition is carried out in a straightforward way, assuming that program-induced increments in working memory capacity (WMC_i) and unmeasured mechanism variables (U_i) are statistically independent conditional on the controls X_i (following Heckman, Pinto, and Savelyev 2013). Taking the estimated parameters δ^k_{WMT} from equation (3) we decompose it for each transfer outcome k into the part explained by working memory capacity improvements, $[(\alpha^k_{WMC} * \beta_{WMT})/\delta^k_{WMT}] * 100\%$, and the unexplained part, $[1 - (\alpha^k_{WMC} * \beta_{WMT})/\delta^k_{WMT}] * 100\%$.

Estimation with Inverse Probability Weights

We collected information about secondary school track choice in a follow-up sample three years after the intervention. Due to attrition in in this sample (see Table S23), we estimate the secondary school choice outcome with inverse probability weights. The weights are based on three binary variables, (i) migration background, (ii) educational achievement, and (iii) cognitive skills. The educational achievement variable is constructed using the sum of standardized scores in geometry, arithmetic, and reading, and the binary variable is built based on a sample split. The cognitive variable is constructed using the sum of standardized scores in working memory capacity (all three working memory tests) and Raven's IQ, and the binary variable is again built based on a sample split. Missing values in the weighting variables were imputed using the modal value of the distribution. For example, the four missing values in the migration background are set to 0 (no migration background); the four missing values in the geometry score are set to 0 (which represents the mean due to standardization to mean $= 0$ and SD $= 1$).

1.6 Robustness Checks

We have several outcome measures and we examine the impact of WM training on them in three post-treatment evaluation waves. For this reason, we deal with the issue of multiple hypothesis testing below. It is worth emphasizing, however, that the time patterns of our results are consistent in the sense that they suggest an increasing impact of the WM training over time on all those variables for which we ultimately find a significant treatment effect. Furthermore, we observe insignificant treatment effects (and small point estimates) across all evaluation waves in those cases in which the treatment had no impact (i.e., arithmetic and bp task). If the observed significant effects were simply due to randomness and did not reflect true treatment effects, we would expect a more irregular pattern.

Nevertheless, it makes sense to check the robustness of our findings with respect to multiple hypothesis testing (Romano and Wolf 2005, 2016). We generate families

8. For the mediation analysis, we focus on those working memory capacity variables that are significantly affected by the WM training, i.e., the verbal simple span score and the visuo-spatial complex span score.

of outcome measures by bundling outcomes in a natural way for the purpose of multiple hypothesis testing. Within a family, we always control the family wise error rate (FWER) for all outcomes in all three evaluation waves (i.e., W2, W3, and W4). One family consists, for example, of our WM outcomes (verbal simple span, verbal complex span, visuo-spatial complex span). When we check for robustness to multiple hypothesis testing, we thus control the FWER for nine outcomes (three outcome measures at three points in time). In addition, we combine multiple hypothesis testing with the BRL (biased-reduced linearization) correction method that accounts for potential biases in the estimation of standard errors when the number of clusters is relatively small. When we simultaneously apply these two robustness checks, we still find significant direct treatment effects on WM capacity and spillover effects on geometry, Raven's fluid IQ measure, and the go/no-go task, while the effect on reading in W4 is no longer significant (see Table S9). We also provide results for a more conservative grouping of families as a further robustness check (see Section 1.5 and Table S10). Overall, the thrust of our treatment effects survives these checks, which lends credibility to our results.

Next, we discuss the concern that treated children might have improved their outcome scores solely because of a Hawthorne or demand type effect (Melby-Lervåg and Hulme 2013). In our view, several reasons speak against this possibility. First, the WM training was embedded into the normal school routine and was introduced like any other new sequence of exercises that children experience during a school year. Thus, the children in the treatment group did not know that they were part of an experiment. In addition, both the children in the control and the treatment group participated in the test tasks, implying that participation in these tasks also cannot explain differential performance across groups. In fact, both the children in the control group and the treatment group were highly motivated in performing the tasks and reported to enjoy taking part in them (see Figures S17–S18). We find neither a treatment effect on the subjective effort provided in the evaluation tasks nor on the extent to which children enjoyed these tasks (see Table S17). Second, we did not inform parents about the treatment assignment of their children, and we also did not provide information that would have enabled them to infer the treatment assignment of their children. Thus, parental behavior is unlikely to be the source of a Hawthorne type effect. This interpretation is also consistent with the absence of a treatment effect on parental investment (see Figure S16 and Table S16). Third, the time pattern of spillover effects speaks against Hawthorne type effects because if participation in an experiment affects general motivation and expectations, then the effects should be most visible shortly after the training when motivation and expectation effects are still fresh. In fact, however, we observe no significant spillover effects shortly after the training — instead, the effects only arise after 6 or 12–13 months. Finally, the specificity and plausibility of the pattern of our results across tasks speaks against Hawthorn type effects. Hawthorn type effects should rather lead to a general and not a specific change in performance. For example, general Hawthorn effects should induce effects across all outcome measures, but we observe no treatment effects in verbal complex span, arithmetic, and the bp task. Thus, taken together, Hawthorne type effects are unlikely to be the source of the observed treatment effect patterns.

We also conducted a robustness check related to the use of computers in school. During the computer-based WM training period, the children in the treatment group naturally used computers more frequently than the children in the control group. Based on the arguments in the previous paragraph, it is highly unlikely that this generated a Hawthorne type effect, but perhaps the teachers in the treatment group subsequently used computers more often in class and this could have had effects on the children. To examine this possibility, we asked the teachers in W3 and W4 how frequently computers were used in the classroom, and we use these data to re-estimate the relevant W3 and W4 treatment effects controlling for computer use (see Tables S11–S12). We find that computer usage neither significantly affects the outcome measures, nor does it change the previously estimated training effects.

Finally, we perform robustness checks with respect to attrition: First, we re-estimate the main results reducing the sample to only those children who are still in the sample in W4 (see Tables S13–S15), but none of the results changes when doing so. This is also consistent with the fact that attrition is generally very low across evaluation waves and not systematically different between treatment and control group. Second, we turn to the Secondary school track choice sample. If we regress participation in the long-run follow-up questionnaire for the school track choice on a treatment dummy, school fixed effects, and further controls, we find that the coefficient related to the treatment dummy is close to zero and insignificant ($p = 0.337$). This suggests that there are no systematic attrition differences between treatment and control group for the long-run follow-up questionnaire. Third, we examined differential attrition in the secondary school track choice sample using interactions between treatment status and socio-demographic variables — such as migration background, language problems, or mothers' education — or between treatment status and baseline outcomes measures. These analyses (see Tables S21–S22) show that there are no significant interaction effects with regard to participation in the long-run follow-up questionnaire.

Taken together, the evidence shows a consistent time pattern of spillover effects suggesting that the observed treatment effects do not simply reflect chance findings. Moreover, most of our spillover effects are robust to multiple hypothesis testing and the evidence also speaks against substantial placebo or Hawthorne type effects or an impact of computer use on treatment effects. Finally, attrition does not seem to pose a challenge for the reported results.

2 Further Supplementary Figures (S12–S18)

2.1 Distribution of Outcome Scores in W1–W4

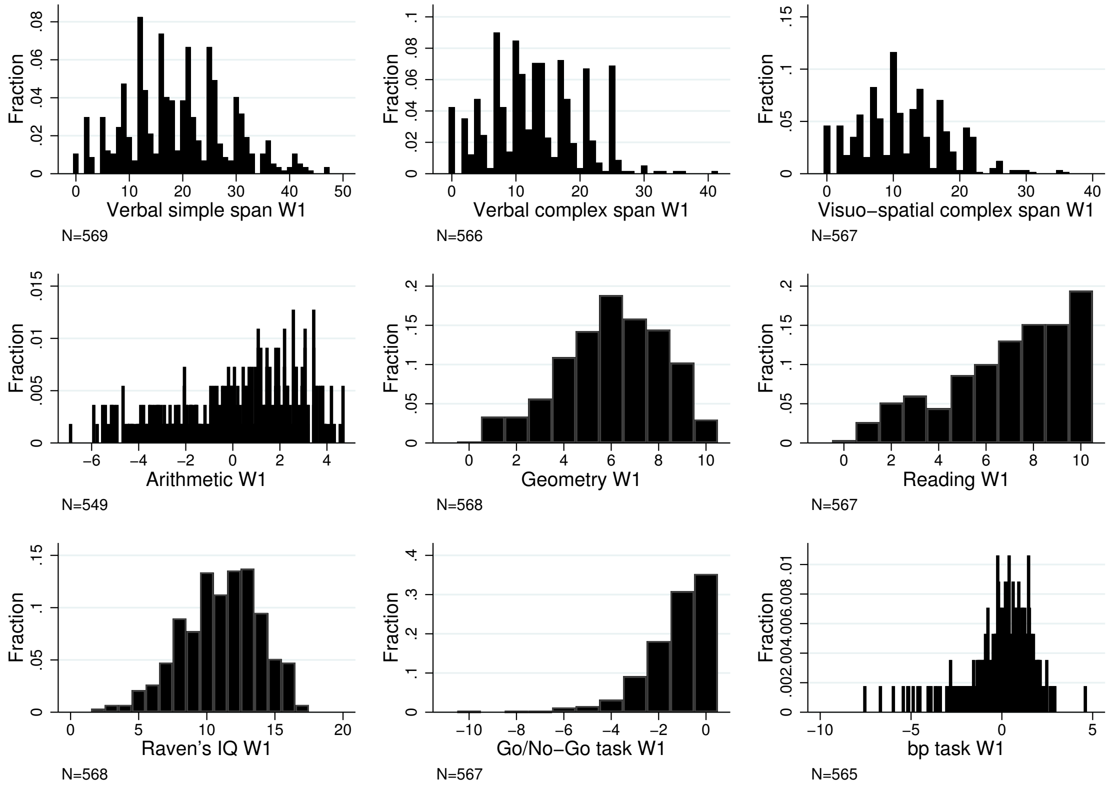


Figure S12: Distribution of Nonstandardized W1 Test Scores

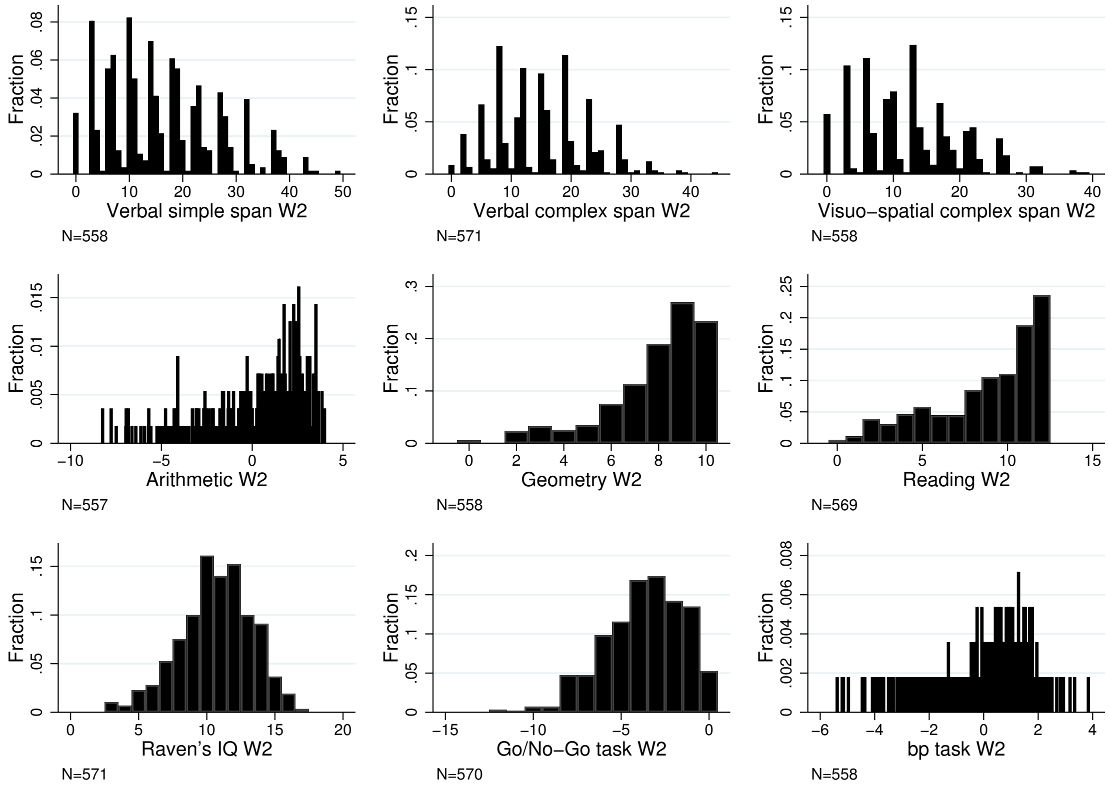


Figure S13: Distribution of Nonstandardized W2 Test Scores

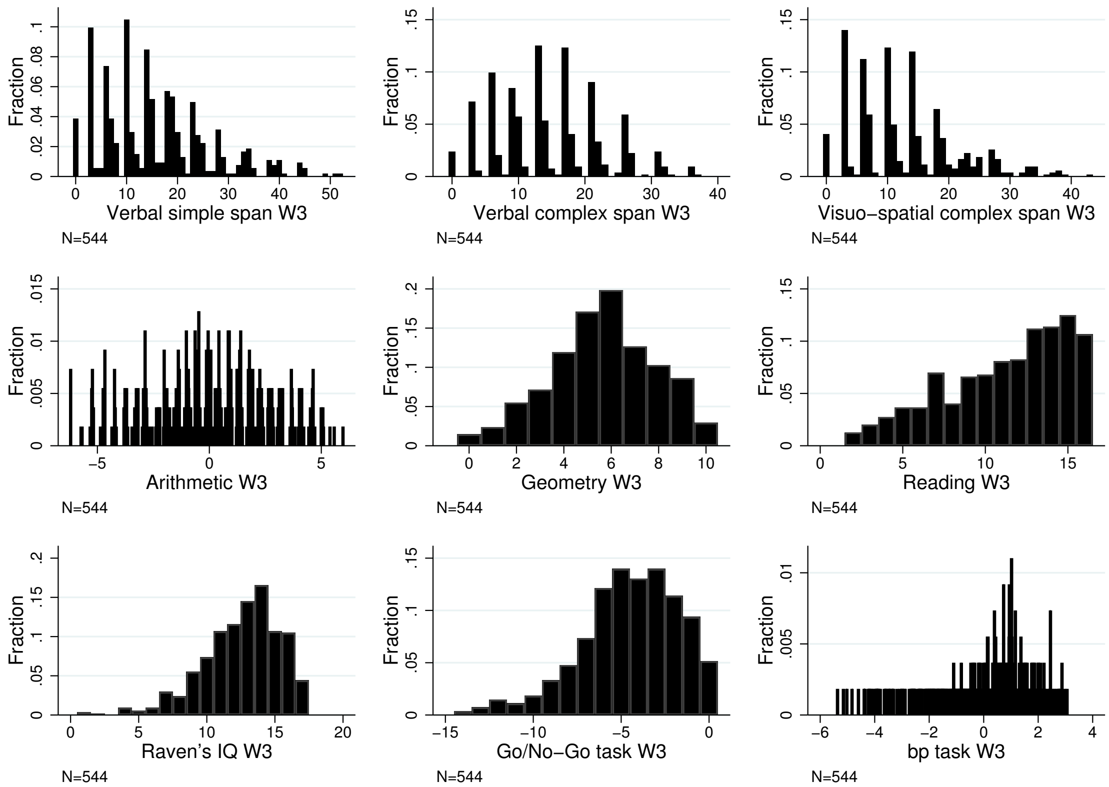


Figure S14: Distribution of Nonstandardized W3 Test Scores

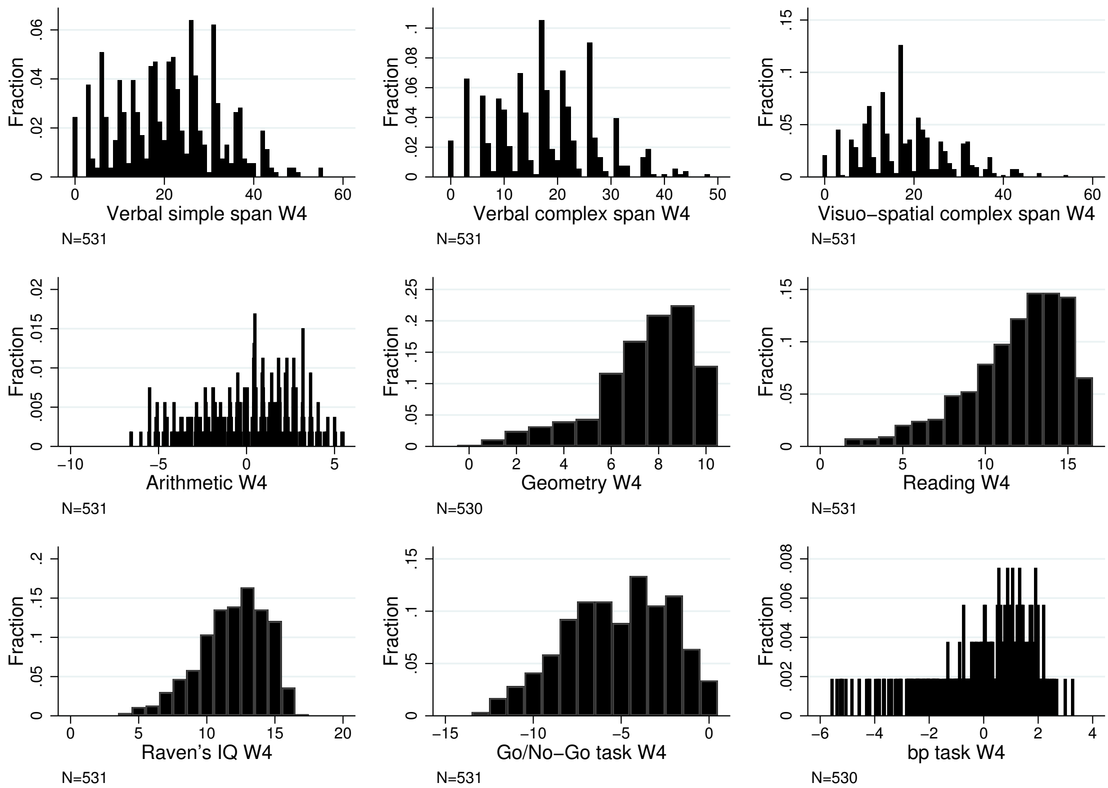
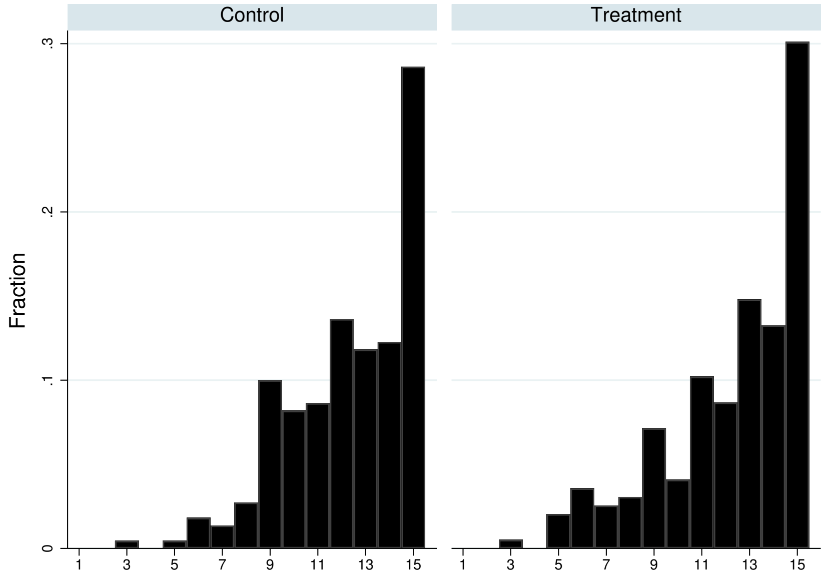


Figure S15: Distribution of Nonstandardized W4 Test Scores

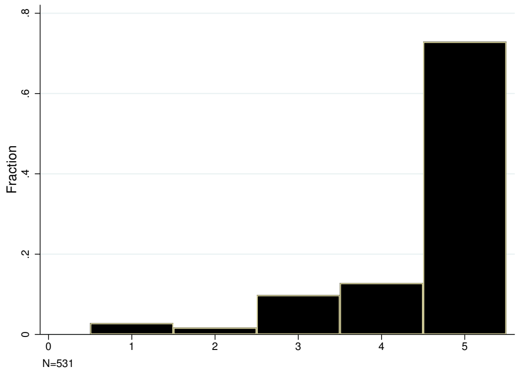
2.2 Parental Investment by Treatment Status



The figure shows the distribution of an index of parental investment, separately by treatment and control. The index is the sum of three variables based on questions in the parental questionnaire in W3 (collected 6 months after treatment): (1) "How many times do you control whether your child has packed the school bag for the next day?", (2) "How many times do you control whether your child has done her homework?", (3) "How many times do you control the content of your child's homework?". The answer options for each question are 1 = "Never", 2 = "Less than once a week", 3 = "1–2 times a week", 4 = "3–4 times a week", 5 = "Always". N = 416.

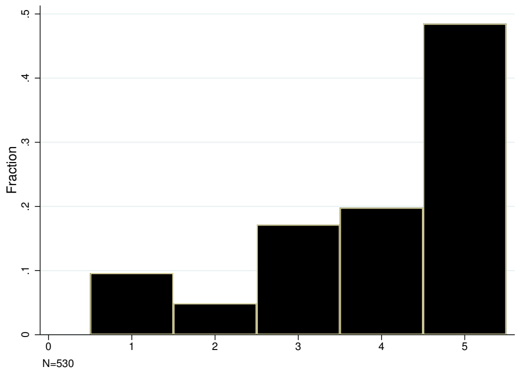
Figure S16: Parental Investment in W3 by Treatment Status

2.3 Children's Self-reported Motivation During Evaluation Tests



The figure plots children's answers to the question "How much did you enjoy doing the tasks on the computer just now?" asked in a computerized short questionnaire immediately after the W4 evaluation. Answer options are a Likert-type scale ranging from 1 = "very little" to 5 = "very much".

Figure S17: Children's Enjoyment in W4 Tasks



The figure plots children's answers to the question "How much did you try to do your best on the computer?" asked in a computerized short questionnaire immediately after the W4 evaluation. Answer options are a Likert-type scale ranging from 1 = "very little" to 5 = "very much".

Figure S18: Children's Effort in W4 Tasks

3 Supplementary Tables (Tables S1–S23)

3.1 Balance Tests

Table S1: Sample Balance: Regressing Socio-demographic Characteristics on the Treatment Indicator

	Male (1)	Age in months (2)	Migration Background (3)	Language Problems (4)	Income >2500 EUR (5)	Mother Univ Degr (6)
Working memory training	-0.013	-0.772	-0.128	-0.035	0.084	-0.051
	(0.034)	(0.501)	(0.081)	(0.071)	(0.087)	(0.072)
N	572	572	568	572	441	444

The results are based on least squares models including school fixed effects. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01. The coefficients for 'working memory training' in the first row and the associated standard errors indicate whether there are significant imbalances between the treatment and control group with respect to the socio-demographic characteristics described in the column titles. In every column, the coefficient for working memory training is small and insignificant. The sample in column 3 is smaller than the total sample size because the dependent variable 'migration background' is taken from the teacher questionnaire and for four children teachers reported not to know the migration background. The samples in columns 5 and 6 are smaller because the dependent variables are taken from the parent questionnaire, which has not been answered (completely) by all parents.

Table S2: Sample Balance: Regressing W1 Baseline Test Scores on the Treatment Indicator

	Verbal simple span (1)	Verbal complex span (2)	Visuo-spatial complex span (3)	Geometry (4)	Arithmetic (5)	Reading (6)	Raven's IQ (7)	Go/No-Go task (8)	bp task (9)
Working memory training	-0.079	0.274**	-0.160	0.082	0.102	0.136	0.060	-0.104	0.133
	(0.104)	(0.128)	(0.101)	(0.121)	(0.102)	(0.182)	(0.102)	(0.136)	(0.128)
N	569	566	567	568	549	567	568	567	565

The results are based on least squares models including school fixed effects and further controls (see Section 1.5 for details). All outcome scores are standardized to mean $= 0$ and SD $= 1$. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01. The coefficients in the first row and the associated p-values indicate whether there are significant imbalances between the treatment and control group regarding the respective baseline outcome measures. It turns out that all coefficients for 'working memory training' (except the one for verbal complex span) are close to zero and insignificant at the 5% level, i.e., there is no evidence for significant imbalances between treatment and control group for these outcome measures. Because the testing for imbalances involved many hypothesis tests, we further check whether the significant coefficient for verbal complex span survives multiple hypothesis correction. If we adjust the p-value for multiple hypothesis testing, the coefficient for verbal complex span turns insignificant (p = 0.332). Note that we control for the baseline (i.e., W1) score of each outcome variable when we estimate the treatment effect of working memory training.

3.2 Tables Identifying the Treatment Effect

Table S3: Treatment Effects on Working Memory Capacity

	Verbal simple span			Verbal complex span			Visuo-spatial complex span		
	W2	W3	W4	W2	W3	W4	W2	W3	W4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Working memory training	0.057	0.382***	0.295**	-0.144	-0.094	0.032	0.395***	0.458***	0.443***
	(0.058)	(0.062)	(0.115)	(0.108)	(0.059)	(0.082)	(0.105)	(0.078)	(0.108)
	[0.337]	[0.000]	[0.015]	[0.189]	[0.120]	[0.702]	[0.001]	[0.000]	[0.000]
N	555	541	528	565	539	526	554	540	527

The results are based on least squares models that regress the various standardized (mean = 0 and SD = 1) working memory scores on a dummy variable that takes on the value of 1 if the child received 'working memory training' and 0 otherwise. The regression also includes school fixed effects, the baseline outcome score (W1), and further controls (see Section 1.5 for details). W2, W3, and W4 refer to the evaluation waves shortly after, 6 months after, and 12–13 months after the working memory training period. Standard errors in parentheses are clustered at the classroom level. Related p-values are reported below in brackets (* p<0.10, ** p<0.05, *** p<0.01).

Table S4: Treatment Effects on Math, Reading, and Raven's IQ

	Arithmetic			Geometry			Reading			Raven's IQ		
	W2	W3	W4	W2	W3	W4	W2	W3	W4	W2	W3	W4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Working memory training	-0.062	-0.034	-0.048	0.166	0.236**	0.384***	0.022	0.089	0.230**	0.019	0.237***	0.237***
	(0.092)	(0.081)	(0.086)	(0.100)	(0.097)	(0.101)	(0.074)	(0.099)	(0.105)	(0.066)	(0.075)	(0.070)
	[0.506]	[0.681]	[0.576]	[0.108]	[0.021]	[0.001]	[0.765]	[0.377]	[0.037]	[0.780]	[0.004]	[0.002]
N	535	525	512	554	541	527	564	539	526	567	540	527

The results are based on least squares models that regress the various standardized (mean = 0 and SD = 1) outcome scores on a dummy variable that takes on the value of 1 if the child received 'working memory training' and 0 otherwise. The regression also includes school fixed effects, the baseline outcome score (W1), and further controls (see Section 1.5 for details). W2, W3, and W4 refer to the evaluation waves shortly after, 6 months after, and 12–13 months after the working memory training period. Standard errors in parentheses are clustered at the classroom level. Related p-values are reported below in brackets (* p<0.10, ** p<0.05, *** p<0.01).

Table S5: Treatment Effects on Performance in the Go/No-Go Task and bp Task

	Go/No-Go task			**bp task**		
	W2	W3	W4	W2	W3	W4
	(1)	(2)	(3)	(4)	(5)	(6)
Working memory training	-0.088	-0.021	0.330***	0.049	0.010	0.073
	(0.116)	(0.120)	(0.061)	(0.079)	(0.084)	(0.124)
	[0.454]	[0.862]	[0.000]	[0.539]	[0.907]	[0.559]
N	566	540	527	552	538	524

The results are based on least squares models that regress the various standardized (mean $= 0$ and SD $= 1$) outcome scores on a dummy variable that takes on the value of 1 if the child received 'working memory training' and 0 otherwise. The regression also includes school fixed effects, the baseline outcome score (W1), and further controls (see Section 1.5 for details). W2, W3, and W4 refer to the evaluation waves shortly after, 6 months after, and 12–13 months after the working memory training period. Standard errors in parentheses are clustered at the classroom level. Related p-values are reported below in brackets (* p<0.10, ** p<0.05, *** p<0.01).

3.3 Heterogeneous Treatment Effects

Table S6: Heterogeneous Treatment Effects on Working Memory Capacity—Interaction with Low WM Capacity (W1)

	Verbal simple span			Verbal complex span			Visuo-spatial complex span		
	W2	W3	W4	W2	W3	W4	W2	W3	W4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Working memory training	0.043	0.460***	0.350***	-0.088	-0.045	0.099	0.354***	0.474***	0.441***
	(0.067)	(0.064)	(0.108)	(0.117)	(0.061)	(0.109)	(0.107)	(0.090)	(0.116)
Low WMC W1 ($\leq$ 25-perc.)	-0.311***	-0.090	-0.201	-0.560***	-0.702***	-0.444***	-0.265***	-0.052	-0.326***
	(0.077)	(0.107)	(0.136)	(0.112)	(0.129)	(0.084)	(0.093)	(0.095)	(0.109)
Low WMC W1 x WMT	0.119	-0.315**	-0.319*	0.056	0.111	-0.072	0.178	-0.073	-0.062
	(0.127)	(0.149)	(0.169)	(0.157)	(0.157)	(0.161)	(0.123)	(0.144)	(0.154)
N	549	535	522	560	535	522	549	535	522

This table examines whether the treatment effect of 'working memory training' on the standardized (mean $= 0$ and SD $= 1$) outcome scores for working memory capacity is different for children that are in the lowest quartile of working memory capacity at baseline. For this purpose, we use a dummy variable 'Low WMC W1' which takes on the value of 1 if working memory capacity in wave W1 is below or at the 25th percentile. The cardinal variable used for that is the sum of the three standardized working memory test scores. We interact 'Low WMC W1' with the treatment dummy. Apart from the inclusion of 'Low WMC W1' and its interaction with the treatment dummy, the least squares regressions also include school fixed effects, the respective baseline working memory scores, and further controls (see Section 1.5 for details). W2, W3, and W4 refer to the evaluation waves shortly after, 6 months after, and 12–13 months after the working memory training period. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01. In this table, the coefficient related to the treatment dummy shows the treatment effect for children with a baseline working memory score above the 25th percentile. The results show that the treatment effects for these children are highly significant for verbal simple span in W3 and W4 and for visuo-spatial complex span in W2, W3, and W4. The coefficients related to the interaction term of 'Low WMC W1' with the treatment dummy show the extent to which the treatment effects are different for children with low working memory capacity at baseline. The table shows that—apart from the negative interaction effect for verbal simple span—there are no significant interaction effects. Also, for W4, the negative interaction for verbal simple span is no longer significant at the 5% level.

Table S7: Heterogeneous Treatment Effects on Math, Reading, and Raven's IQ—Interaction with Low WM Capacity (W1)

	Arithmetic			Geometry			Reading			Raven's IQ		
	W2	W3	W4	W2	W3	W4	W2	W3	W4	W2	W3	W4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Working memory training	-0.052	0.018	-0.007	0.118	0.200**	0.384***	0.088	0.132	0.317***	0.023	0.254***	0.263***
	(0.094)	(0.089)	(0.088)	(0.109)	(0.093)	(0.113)	(0.073)	(0.096)	(0.094)	(0.068)	(0.086)	(0.091)
Low WMC W1 ($\leq$ 25-perc.)	-0.257*	-0.279**	-0.334**	-0.626***	-0.539***	-0.433**	-0.224**	-0.358***	-0.254*	-0.408***	-0.405***	-0.303**
	(0.135)	(0.111)	(0.126)	(0.126)	(0.133)	(0.205)	(0.095)	(0.112)	(0.148)	(0.124)	(0.099)	(0.123)
Low WMC W1 x WMT	0.023	-0.131	-0.096	0.321**	0.260	0.094	-0.157	-0.034	-0.325	0.072	0.023	-0.175
	(0.179)	(0.139)	(0.156)	(0.153)	(0.207)	(0.257)	(0.187)	(0.169)	(0.215)	(0.139)	(0.135)	(0.166)
N	533	521	508	549	535	521	556	533	520	559	534	521

This table examines whether the treatment effect of 'working memory training' on the standardized (mean = 0 and SD = 1) outcome scores for arithmetic, geometry, reading, and Raven's IQ is different for children that are in the lowest quartile of working memory capacity at baseline. For this purpose, we use a dummy variable 'Low WMC W1' which takes on the value of 1 if working memory capacity in wave W1 is below or at the 25th percentile. The cardinal variable used for that is the sum of the three standardized working memory test scores. We interact 'Low WMC W1' with the treatment dummy. Apart from the inclusion of 'Low WMC W1' and its interaction with the treatment dummy, the least squares regressions also include school fixed effects, the respective baseline outcome scores, and further controls (see Section 1.5 for details). W2, W3, and W4 refer to the evaluation waves shortly after, 6 months after, and 12–13 months after the working memory training period. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01. In this table, the coefficient related to the treatment dummy shows the treatment effect for children with a baseline working memory score above the 25th percentile. The results show that the treatment effects for these children are significant for geometry in W3 and W4, for reading in W4, and for Raven's IQ in W3 and W4. The coefficients related to the interaction term between 'Low WMC W1' and the treatment dummy show the extent to which the treatment effects are different for children with low working memory capacity at baseline. The table shows that—apart from the positive interaction effect for geometry in W2—there are no significant interaction effects.

Table S8: Heterogeneous Treatment Effects on Performance in the Go/No-Go Task and bp Task—Interaction with Low WM Capacity (W1)

	Go/No-Go task			**bp task**		
	W2	W3	W4	W2	W3	W4
	(1)	(2)	(3)	(4)	(5)	(6)
Working memory training	-0.051	0.084	0.339***	0.166*	0.068	0.117
	(0.136)	(0.092)	(0.070)	(0.090)	(0.104)	(0.104)
Low WMC W1 ($\leq$ 25-perc.)	-0.265*	-0.306**	-0.357*	0.075	-0.161	-0.296
	(0.142)	(0.120)	(0.199)	(0.093)	(0.134)	(0.200)
Low WMC W1 x WMT	-0.058	-0.278	-0.019	-0.443***	-0.189	-0.185
	(0.206)	(0.254)	(0.261)	(0.131)	(0.185)	(0.236)
N	558	533	520	547	532	518

This table examines whether the treatment effect of 'working memory training' on the standardized (mean $= 0$ and SD $= 1$) outcome scores for performance in the go/no-go task and the bp task is different for children that are in the lowest quartile of working memory capacity at baseline. For this purpose, we use the dummy variable 'Low WMC W1' which takes on the value of 1 if working memory capacity in wave W1 is below or at the 25th percentile. The cardinal variable used for that is the sum of the three standardized working memory test scores. We interact 'Low WMC W1' with the treatment dummy. Apart from the inclusion of 'Low WMC W1' and its interaction with the treatment dummy, the least squares regressions also include school fixed effects, the respective baseline outcome scores, and further controls (see Section 1.5 for details). W2, W3, and W4 refer to the evaluation waves shortly after, 6 months after, and 12–13 months after the working memory training period. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01. In this table, the coefficient related to the treatment dummy shows the treatment effect for children with a baseline working memory score above the 25th percentile. The results show that the treatment effect for these children is significant for performance in the go/no-go task in W4. The coefficient related to the interaction term between 'Low WMC W1' and the treatment dummy shows the extent to which the treatment effect is different for children with low working memory capacity at baseline. The table shows that—apart from the negative interaction effect for the bp task in W2—there are no significant interaction effects.

3.4 Corrections for Multiple Hypothesis Testing

Table S9: Corrections for Multiple Hypothesis Testing

	W2 (1)	W3 (2)	W4 (3)
Panel A: Working Memory Outcomes			
Verbal simple span	0.057 (0.697)	0.382*** (0.001)	0.295 (0.173)
Verbal complex span	-0.144 (0.561)	-0.094 (0.477)	0.032 (0.747)
Visuo-spatial complex span	0.395** (0.021)	0.458*** (0.001)	0.443** (0.021)
Panel B: Educational Far-transfer Outcomes			
Arithmetic	-0.062 (0.946)	-0.034 (0.946)	-0.048 (0.946)
Geometry	0.166 (0.543)	0.236 (0.228)	0.384** (0.025)
Reading	0.022 (0.946)	0.089 (0.893)	0.230 (0.302)
Panel C: Far-transfer Outcomes on General Cognitive Skills			
Raven's IQ	0.019 (0.809)	0.237** (0.041)	0.237** (0.041)
Panel D: Far-transfer Outcomes on General Noncognitive Skills			
Go/No-Go task	-0.088 (0.956)	-0.021 (0.981)	0.330*** (0.004)
bp task	0.049 (0.969)	0.010 (0.981)	0.073 (0.969)

The results are based on our main specifications reported in Supplementary Tables S3–S5. The coefficients are the point estimates showing how working memory training changes the outcome score indicated at the left-hand side of the table (as a fraction of a standard deviation) relative to the control group. We report p-values corrected for multiple hypothesis testing and small number of clusters in parentheses below each point estimate (* p<0.10, ** p<0.05, *** p<0.01). The p-values are adjusted by controlling the family-wise error rate within each family of outcomes (a family corresponds to all outcome measures at all three points in time (W2, W3, and W4) in a given panel) using the step-down procedure by Romano and Wolf (2005, 2016), and by applying the conservative "biased reduced linearization (BRL) method" of Bell and McCaffrey (2002) to calculate clustered standard errors. The methods applied here are described in detail in Section 1.5. W2, W3, and W4 refer to the evaluation waves shortly after, 6 months after, and 12–13 months after the working memory training period. All treatment effects remain significant at the 5-percent level, except for the effect on verbal simple span in W4 (MHT-BRL corrected p-value = .173) and Reading in W4 (MHT-BRL corrected p-value = .302).

Table S10: Corrections for Multiple Hypothesis Testing — Direct vs. Spillover Effects

	W2 (1)	W3 (2)	W4 (3)
Panel A: Direct Effects			
Verbal simple span	0.057 (0.697)	0.382*** (0.001)	0.295 (0.173)
Verbal complex span	-0.144 (0.561)	-0.094 (0.477)	0.032 (0.747)
Visuo-spatial complex span	0.395** (0.021)	0.458*** (0.001)	0.443** (0.021)
Panel B: Spillover Effects			
Arithmetic	-0.062 (0.998)	-0.034 (0.997)	-0.048 (0.997)
Geometry	0.166 (0.768)	0.236 (0.376)	0.384* (0.073)
Reading	0.022 (0.997)	0.089 (0.987)	0.230 (0.489)
Raven's IQ	0.019 (0.997)	0.237 (0.136)	0.237 (0.114)
Go/No-Go task	-0.088 (0.996)	-0.021 (0.997)	0.330*** (0.007)
bp task	0.049 (0.997)	0.010 (0.997)	0.073 (0.997)

The results are based on our main specifications reported in Supplementary Tables S3–S5. The coefficients are the point estimates showing how working memory training changes the outcome score indicated at the left-hand side of the table (as a fraction of a standard deviation) relative to the control group. We report p-values corrected for multiple hypothesis testing and small number of clusters in parentheses below each point estimate (* p<0.10, ** p<0.05, *** p<0.01). The p-values are adjusted by controlling the family-wise error rate within each family of outcomes (a family corresponds to all outcome measures at all three points in time (W2, W3, and W4) in a given panel) using the step-down procedure by Romano and Wolf (2005, 2016), and by applying the conservative "biased reduced linearization (BRL) method" of Bell and McCaffrey (2002) to calculate clustered standard errors. The methods applied here are described in detail in Section 1.5. W2, W3, and W4 refer to the evaluation waves shortly after, 6 months after, and 12–13 months after the working memory training period.

3.5 Controlling for Computer Use

Table S11: Treatment Effects on Working Memory Capacity—Controlling for Computer Use in Class

	Verbal simple span		Visuo-spatial comp.	
	W3	W4	W3	W4
	(1)	(2)	(3)	(4)
Working memory training	0.352***	0.295**	0.425***	0.444***
	(0.051)	(0.118)	(0.077)	(0.110)
Use of computers in class W3	0.078*		0.085*	
	(0.040)		(0.049)	
Use of computers in class W4		0.004		-0.012
		(0.075)		(0.051)
N	541	528	540	527

This table shows the estimates of the treatment effect of working memory training on the standardized (mean = 0 and SD = 1) outcome scores for verbal and visuo-spatial working memory when we additionally control for computer use in classes. The results are based on least squares models that regress the various working memory scores on a dummy variable that takes on the value of 1 if the child received 'working memory training' and 0 otherwise. The regression also includes school fixed effects, the baseline outcome score (W1), and further controls (see Section 1.5 for details). W3 and W4 refer to the evaluation waves 6 and 12–13 months after the working memory training period. The coefficients in the first row are point estimates showing how working memory training changes the working memory capacity scores indicated at the top of the table (as a fraction of a standard deviation) relative to the control group. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01. Compared to Table S4, the coefficients in the first row of this table remain highly significant when we control for computer use.

Table S12: Treatment Effects on Geometry, Reading, Raven's IQ, and Go/No-Go Task—Controlling for Computer Use in Class

	Geometry		**Reading**		**Raven's IQ**		**Go/No-Go task**	
	W3	W4	W3	W4)	W3	W4	W3	W4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Working memory training	0.215**	0.377***	0.106	0.232**	0.269***	0.234***	-0.045	0.329***
	(0.093)	(0.096)	(0.099)	(0.105)	(0.064)	(0.069)	(0.118)	(0.063)
Use of computers in class W3	0.052		-0.043		-0.081**		0.060	
	(0.057)		(0.042)		(0.035)		(0.049)	
Use of computers in class W4		0.055		-0.024		0.028		0.011
		(0.058)		(0.061)		(0.048)		(0.042)
N	541	527	539	526	540	527	540	527

This table shows the estimates of the treatment effect of working memory training on the standardized (mean = 0 and SD = 1) outcome scores for geometry, reading, Raven's IQ and performance in the go/no-go task when we additionally control for computer use in classes. The results are based on least squares models that regress the various outcome scores on a dummy variable that takes on the value of 1 if the child received 'working memory training' and 0 otherwise. The regression also includes school fixed effects, the baseline outcome score (W1), and further controls (see Section 1.5 for details). W3 and W4 refer to the evaluation waves 6 and 12–13 months after the working memory training period. The coefficients in the first row are point estimates showing how working memory training changes the outcome scores indicated at the top of the table (as a fraction of a standard deviation) relative to the control group. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01. Compared to Tables S4 and S5, the coefficients in the first row of this table remain highly significant when we control for computer use.

3.6 Restricting the Analyses to the No-attrition Sample W1–W4

Table S13: Treatment Effects on Working Memory Capacity—Sample Reduced to Children Appearing in All Waves

	Verbal simple span			Verbal complex span			Visuo-spatial complex span		
	W2	W3	W4	W2	W3	W4	W2	W3	W4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Working memory training	0.089	0.394***	0.291***	-0.131	-0.099	0.021	0.459***	0.454***	0.430***
	(0.063)	(0.046)	(0.090)	(0.129)	(0.058)	(0.080)	(0.110)	(0.068)	(0.106)
N	515	515	515	525	525	525	515	515	515

This table estimates the treatment effect of working memory training when we constrain the sample to those children that stay in the sample for all evaluation waves (from W1–W4). The results are based on least squares models that regress the various standardized (mean $= 0$ and SD $= 1$) working memory scores on a dummy variable that takes on the value of 1 if the child received 'working memory training' and 0 otherwise. The regression also includes school fixed effects, the baseline outcome score (W1), and further controls (see Section 1.5 for details). W2, W3, and W4 refer to the evaluation waves shortly after, 6 months after, and 12–13 months after the working memory training period. The coefficients in the first row are point estimates showing how working memory training changes the working memory score indicated at the top of the table (as a fraction of a standard deviation) relative to the control group. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01.

Table S14: Treatment Effects on Math, Reading, and Raven's IQ—Sample Reduced to Children Appearing in All Waves

	Arithmetic			Geometry			Reading			Raven's IQ		
	W2	W3	W4	W2	W3	W4	W2	W3	W4	W2	W3	W4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Working memory training	0.032	-0.034	-0.048	0.200**	0.263***	0.356***	0.037	0.074	0.243**	0.070	0.290***	0.248***
	(0.099)	(0.080)	(0.077)	(0.095)	(0.090)	(0.101)	(0.073)	(0.098)	(0.102)	(0.076)	(0.088)	(0.072)
N	499	499	499	514	514	514	524	524	524	526	526	526

This table estimates the treatment effect of working memory training on various outcome scores when we constrain the sample to those children that stay in the sample for all evaluation waves (from W1–W4). The results are based on least squares models that regress the various standardized (mean $= 0$ and SD $= 1$) outcome scores on a dummy variable that takes on the value of 1 if the child received 'working memory training' and 0 otherwise. The regression also includes school fixed effects, the baseline outcome score (W1), and further controls (see Section 1.5 for details). W2, W3, and W4 refer to the evaluation waves shortly after, 6 months after, and 12–13 months after the working memory training period. The coefficients in the first row are point estimates showing how working memory training changes the outcome score indicated at the top of the table (as a fraction of a standard deviation) relative to the control group. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01.

Table S15: Treatment Effects on Performance in the Go/No-Go Task and bp Task—Sample Reduced to Children Appearing in All Waves

	Go/No-Go task			**bp task**		
	W2	W3	W4	W2	W3	W4
	(1)	(2)	(3)	(4)	(5)	(6)
Working memory training	-0.109	0.033	0.329***	0.103	0.002	0.062
	(0.121)	(0.109)	(0.061)	(0.083)	(0.075)	(0.130)
N	526	526	526	512	512	512

This table estimates the treatment effect of working memory training when we constrain the sample to those children that stay in the sample for all evaluation waves (from W1–W4). The results are based on least squares models that regress the various standardized (mean $= 0$ and SD $= 1$) outcome scores on a dummy variable that takes on the value of 1 if the child received 'working memory training' and 0 otherwise. The regression also includes school fixed effects, the baseline outcome score (W1), and further controls (see Section 1.5 for details). W2, W3, and W4 refer to the evaluation waves shortly after, 6 months after, and 12–13 months after the working memory training period. The coefficients in the first row are point estimates showing how working memory training changes the outcome score indicated at the top of the table (as a fraction of a standard deviation) relative to the control group. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01.

3.7 Treatment Effect on Parental Investment

Table S16: Treatment Effect on Parental Investment W3

	Parental investment
Working memory training	0.084
	(0.626)
N	416

The table reports results based on a least-squares model. The dependent variable is an index of parental investment. The index is the sum of three variables based on questions in the parental questionnaire in W3 (6 months after treatment): (1) "How many times do you control whether your child has packed the school bag for the next day?", (2) "How many times do you control whether your child has done her homework?", (3) "How many times do you control the content of your child's homework?". The answer options for each question are 1 = "Never", 2 = "Less than once a week", 3 = "1–2 times a week", 4 = "3–4 times a week", 5 = "Always". The regressions include school fixed effects and further controls (see Section 1.5 for details). Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01.

3.8 Treatment Effects on Children's Self-reported Motivation

Table S17: Treatment Effects on Children's Self-Reported Motivation (W4)

	Enjoyment (W4)	Effort (W4)
	(1)	(2)
Working memory training	-0.078	0.128
	(0.104)	(0.156)
N	531	530

This table examines whether children who received 'working memory training' exert different effort in the evaluation tasks or enjoy them differently compared to children in the control group. The dependent variables used here are taken from the children's answers to the two following questions that were asked immediately after completing the computer tasks 12–13 months after treatment: "How much did you enjoy doing the tasks on the computer just now?" (1 = "very little" to 5 = "very much") and "How much did you try to do your best on the computer?" (1 = "very little" to 5 = "very much"). The results are based on least squares regressions including school fixed effects and further controls (see Section 1.5 for details). Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01.

3.9 Factor Analysis of Overall Self-regulation Items

Table S18: Factor Analysis of Overall Self-regulation Items (W1)

	Eigenvalue
Factor 1	3.492459
Factor 2	.7630324
Factor 3	.0389734
Factor 4	-.0677124
Factor 5	-.0840027
Factor 6	-.1307162
Factor 7	-.134754

	Factor 1	Factor 2
The child works in a concentrated and enduring manner.	-.8003659	.3931277
The child makes a large number of mistakes due to inattention.	.4946404	-.1795061
The child has a lot of self-discipline.	-.8409723	.2673741
The child has trouble waiting until it is his/her turn.	.6540294	.4583938
The child disturbs class instruction often.	.7583489	.3070454
Please indicate for each child how often he/she forgot his/her homework or did not do his/her homework despite having an assignment in the last six months. (1 = “never forgot homework” up to 7 = “forgot homework often”)	.5795479	-.2831417
How do you rate the child with respect to patience? (1 = “very impatient” up to 7 = “very patient”)	-.7491641	-.3466996

This table shows the factor analysis for the items that were used to construct the overall self-regulation score W1. Questions 1–5 were answered by means of a 7-point Likert-type scale where 1 = “is not at all the case” and 7 = “is completely so”. The answer options for the questions 6 and 7 are indicated behind the question. The factor analysis shows that it is appropriate to extract only one factor. This factor has an eigenvalue > 1 and all survey items display strong factor loadings on this factor, while for all other factors the eigenvalue is clearly below one and factor loadings are small.

3.10 Treatment Effects on Overall Self-regulation

Table S19: Treatment Effects on Overall Self-regulation

	W2	W3	W4
	(1)	(2)	(3)
Working memory training	0.224*	0.369**	0.269**
	(0.111)	(0.171)	(0.114)
N	555	527	517

This table reports the results of least squares regressions of the standardized (mean = 0 and SD = 1) overall self-regulation scores in the different evaluation waves on a dummy variable that takes on the value of 1 if the child received 'working memory training' and 0 otherwise. The regression also includes school fixed effects, the baseline outcome score (W1), and further controls (see Section 1.5 for details). W2, W3, and W4 refer to the evaluation waves shortly after, 6 months after, and 12–13 months after the working memory training period. The coefficients in the first row are point estimates showing how working memory training changes overall self-regulation scores in the various evaluation waves (as a fraction of a standard deviation) relative to the control group. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01.

3.11 Decomposing the Treatment Effect of Working Memory Training

Table S20: The Relevance of Working Memory Capacity in Geometry, Reading, Raven's IQ, and Go/No-Go Task

	Geometry	Reading	Raven's IQ	Go/No-Go task
Visuo-spatial complex span W4	0.332***	0.099*	0.226***	-0.015
	(0.078)	(0.047)	(0.056)	(0.052)
	[0.001]	[0.052]	[0.001]	[0.783]
Verbal simple span W4	0.208***	0.226***	0.188***	0.211***
	(0.068)	(0.068)	(0.041)	(0.052)
	[0.008]	[0.005]	[0.000]	[0.001]
N	271	270	269	269

The results are based on least squares models that regress the various standardized (mean $= 0$ and SD $= 1$) far-transfer outcome scores from W4 (i.e., evaluation wave 12–13 months after the working memory training period) on the standardized outcomes scores for various working memory scores W4. All models additionally include school fixed effects, the pre-training baseline (W1) level of the respective far-transfer outcome score, gender, age, and age at test day. The sample is restricted to the control group. Standard errors in parentheses are clustered at the classroom level. Related p-values are reported in brackets. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

3.12 Analyzing Attrition for Secondary School Choice Sample

Table S21: Treatment Effect on Probability of Being in Sample of Secondary School Choice—Interactions between Socio-Demographic Characteristics and Treatment Status

	(1)	(2)	(3)	(4)	(5)	(6)
Working memory training	-0.080	-1.193	-0.094	-0.113	-0.000	0.079
	(0.082)	(0.815)	(0.077)	(0.070)	(0.110)	(0.082)
Male × WMT	0.018					
	(0.074)					
Age × WMT		0.014				
		(0.010)				
Migration background × WMT			-0.005			
			(0.086)			
Language problems × WMT				0.128		
				(0.100)		
Income Eur 2500+ × WMT					-0.013	
					(0.106)	
Mother university × WMT						-0.114
						(0.072)
N	572	572	568	572	441	444

The table reports results based on least-squares models. The dependent variable is a dummy variable taking on the value one if the child is in the secondary school choice sample and zero otherwise. All models include school fixed effects and further controls (see Section 1.5 for details) as well as the main effects of the variables interacted. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01.

Table S22: Treatment Effect on Probability of Being in Sample of Secondary School Choice—Interactions between Outcome Scores at Baseline and Treatment Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
WMT	-0.080	-0.102	-0.062	-0.101	-0.092	-0.101	-0.098	-0.078	-0.099
	(0.075)	(0.080)	(0.082)	(0.074)	(0.080)	(0.075)	(0.076)	(0.081)	(0.082)
VSS×WMT	0.028								
	(0.035)								
VCS×WMT		0.027							
		(0.038)							
VSCS×WMT			-0.027						
			(0.038)						
Arithmetic×WMT				0.049					
				(0.034)					
Geometry×WMT					0.046				
					(0.033)				
Reading×WMT						0.035			
						(0.047)			
Raven's ×WMT							0.052		
							(0.032)		
Go/No-Go×WMT								-0.046	
								(0.034)	
bp task ×WMT									0.077*
									(0.042)
N	569	566	567	549	568	567	568	567	565

The table reports results based on least squares models. The dependent variable is a dummy variable taking on the value one if the child is in the secondary school choice sample and zero otherwise. WMT is the treatment indicator (working memory training); VSS is the verbal simple span score from W1; VCS is the verbal complex span score from W1; VSCS is the visuo-spatial complex span score from W1. All models include school fixed effects and further controls (see Section 1.5 for details) as well as the main effects of the variables interacted. Standard errors in parentheses are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01.

3.13 Sample Composition at W1, W4, and at the Time of Secondary School Track Choice

Table S23: Sample Composition at W1, W4, and at the Time of Secondary School Track Choice

	Mean for Sample at W1	**Mean for Sample at W4**	**Mean for Sample at School Track Choice**
Working memory training	0.488	0.488	0.483
Male	0.490	0.493	0.455
Children's age in months on Jan 1, 2013	82.129	82.177	81.943
Children's age on test day W1 (in months)	84.247	84.295	84.022
Children's age on test day W2 (in months)	87.288	87.335	87.101
Children's age on test day W3 (in months)	92.368	92.427	92.184
Children's age on test day W4 (in months)	99.582	99.582	99.336
Migration background	0.451	0.446	0.381
Language problems	0.247	0.241	0.183
Monthly HH-Net Income $<$750 EUR	0.023	0.017	0.012
Monthly HH-Net Income 750–1500 EUR	0.120	0.107	0.087
Monthly HH-Net Income 1500–2500 EUR	0.209	0.209	0.193
Monthly HH-Net Income 2500–5000 EUR	0.433	0.439	0.477
Monthly HH-Net Income $>$5000 EUR	0.215	0.228	0.231
Mother university degree	0.446	0.464	0.488
Mother vocational degree	0.423	0.413	0.422
Mother no professional degree	0.131	0.123	0.090
Verbal simple span W1	0	0.062	0.127
Verbal complex span W1	0	0.043	0.137
Visuo-spatial complex span W1	0	0.060	0.130
Arithmetic W1	0	0.056	0.138
Geometry W1	0	0.034	0.157
Reading Test W1	0	0.070	0.177
Raven's IQ W1	0	0.050	0.135
Go/No-Go task W1	0	0.014	0.064
bp task W1	0	0.033	0.082

The table provides socio-demographic information about our sample at different points in time. Column (1) provides means for the sample at W1 (baseline, with a gross sample of n = 572). Column (2) reports the means for the sample at W4 (12–13 months after treatment, with a gross sample of n = 531). Column (3) provides the means for the sample at school track choice (three years after treatment, with a gross sample of n = 393). The gender and age variables have been reported by the schools and are therefore available for all children. The variables 'migration background' and 'language problems' are taken from the teacher questionnaire in W1; for four children teachers reported not to know the migration background. The income and maternal education variables are taken from the parent questionnaire in W1. We standardized the working memory scores (verbal simple span, verbal complex span, visuo-spatial complex span), educational scores (arithmetic, geometry, reading), and other outcome scores (Raven's IQ, Go/No-Go task score, bp task score) to mean $= 0$ and SD $= 1$ in the W1 sample. Thus, the increased means of these variables in W4 and at the time of secondary school track choice indicate low-score-specific attrition. We control for attrition effects with inverse probability weighting and other analyses.

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