A multilevel meta-analysis on the causal effect of ANS training on symbolic math performance

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Abstract

We performed a meta-analysis of approximate number system (ANS) training studies to investigate the strength of the causal effects of practicing ANS related tasks on symbolic math performance. Across 33 effect sizes from 11 studies involving 754 participants, for which neither the treatment nor control group received symbolic training, we found a small non-significant and sensitive effect of ANS training on symbolic math task performance ($g = .11$, $CI[-0.01, 0.22]$; PEESE adjusted $g = -.04$, $CI[-0.58, 0.50]$). Some heterogeneity was accounted for by participant age, with larger estimates for adults than for children. Estimates did not vary significantly by ANS training type, training duration, and control group type. An exploratory analysis on the transfer effects of ANS training on untrained non-symbolic tasks suggests weak support for the key auxiliary assumption that ANS training has substantial effects on a general ANS, indicating that the training literature may not adequately represent theories of how ANS influences symbolic number performance.

Keywords: ANS, cognitive training, transfer, math achievement, meta-analysis

A multilevel meta-analysis on the causal relation between ANS training and symbolic math performance

Humans have an intuitive, abstract sense of number, allowing us to compare approximate magnitudes and estimate or manipulate large numerical quantities without language or the use of symbols (Feigenson et al., 2004). This non-symbolic number sense, termed the Approximate Number System (hereafter, ANS), is hypothesized to be present from birth in humans across cultures (Gordon, 2004; Izard et al., 2009; Pica et al., 2004; Xu & Spelke, 2000), and even shared with a wide range of non-human animals (Dehaene et al., 1998). However, in contrast to other animals, humans also acquire a specialized exact, symbolic number system to represent precise magnitudes. Some have suggested that the ANS representations of numerical magnitudes are also involved in processing symbolic mathematical information (Halberda et al., 2008; Lemer et al., 2003; Piazza et al., 2010).

Individual differences in the precision of the ANS correlate with current and future mathematics competence both in children and adults (Geary & vanMarle, 2016; Halberda et al., 2008; Inglis et al., 2011; Lourenco et al., 2012; Mazzocco et al., 2011; Mussolin et al., 2014; Starr et al., 2013). Meta-analyses have consistently found modest but significant correlations between ANS acuity and symbolic math skills (Chen & Li, 2014; Fazio et al., 2014; Schneider et al., 2017). Chen & Li (2014) estimated a moderate and significant correlation both cross-sectionally and longitudinally ($k = 47$, $r = .20$ for cross-sectional studies; $k = 6$, $r = .24$ for longitudinal studies). Fazio, et al. (2014) and Schneider et al. (2017) respectively

included 34 and 195 effect sizes and reported meta-analytic correlations of $r = .22$ and $r = .241$ for cross-sectional studies.

However, these associations may at least partially result from confounding domain-general cognitive skills, including visuospatial ability, inhibitory control, or individual differences in sensory integration (Clayton et al., 2015; Cui et al., 2019; Fuhs & McNeil, 2013; Gebuis et al., 2016; Gilmore et al., 2013; Zhou et al., 2015). Thus, the partial correlations between ANS acuity and symbolic math skills, statistically controlling for measures of hypothesized confounds, are observed to be smaller, and sometimes not statistically distinguishable from 0 (Chen & Li, 2014; Keller & Libertus, 2015; Szűcs et al., 2014; Szűcs et al., 2013). The converging evidence from correlational studies thus leaves the possibility open that hypothesized confounders may be responsible for the observed association between ANS acuity and symbolic math performance.

Designs that attempt to train ANS acuity and measure transfer to symbolic math performance circumvent some of the limitations of correlational studies for examining the causal link between ANS and symbolic math performance. Specifically, ANS training can be randomly assigned, ruling out confounding explanations. Additionally, an ANS training program that reliably improved symbolic math performance might be useful for educational purposes (Odic & Starr, 2018).

However, the ANS training literature has yielded mixed results, and the theoretical status of claims about causal effects of changes to ANS acuity on symbolic math performance is disputed (see Libertus, 2019; Odic & Starr, 2018; Szűcs &

Myers, 2017 for a review). In the current study, we systematically investigate the strength of the evidence for transfer effects from ANS training to performance on symbolic math tasks, in order to understand the strength of this evidence for existing theories of the influence of individual differences in ANS acuity on symbolic math performance. We begin with a discussion of these theoretical accounts.

Causal relations between ANS and symbolic math

The ANS is hypothesized to be an innate core system and to play a foundational role in the development of children's symbolic numerical representations (Dehaene & Changeux, 1993; Feigenson et al., 2004; Piazza, 2011). Research on the ANS was influenced by the several related theories. Dehaene posited that humans are born with a "sense of number" and emphasized its role in the process of numerosity estimation independent of continuous dimensions that correlate with number, such as the size or density of the items (Dehaene & Changeux, 1993; Dehane, 1997). Feigenson et al. (2004) expanded on the concept of "number sense", proposing the existence of two core subsystems of numerical representation: core system 1 representing large non-symbolic numerosities (>4) and core system 2 representing small non-symbolic numerosities (1-4). According to the theory, core system 1 enables people to discriminate non-symbolic numerosities regardless of continuous magnitudes and this discrimination depends on the ratio between the two quantities (Feigenson et al., 2004). The modulating role of numerical ratio has long been observed in symbolic numerosity comparison, leading some to hypothesize that

overlapping systems may also be involved in both non-symbolic and symbolic numerical cognition (Moyer & Landauer, 1967).

However, it is not yet clear how the ANS interfaces with symbolic mathematics performance. Several mechanistic hypotheses driving from behavioral and neural evidence have been proposed to explain the link between ANS and symbolic mathematics performance.

Theories of the link between ANS and symbolic math performance

One prominent theory (Theory One) suggests that numerical representations are grounded in ANS representations (Dehaene & Changeux, 1993; Gallistel & Gelman, 1992). Specifically, during the initial acquisition of number words, children map these number words onto the pre-existing core magnitude system (i.e. ANS representations) to assist learning their meanings. On this view, children's ANS acuity may have significant effect on mastering the counting sequence and other symbolic skills later in formal math education. In support of this theory, Gilmore et al. (2010) reported that performance on non-symbolic numerical tasks statistically predicts children's mastery of number words and school mathematics performance. Perhaps consistent with the idea that the ANS is more important during early learning, one meta-analysis found that relations between ANS acuity and symbolic math performance was stronger for younger children (Fazio et al., 2014; although age was not a significant moderator of the strength of this correlation in Chen & Li, 2014 and Schneider et al., 2016). Claims about the foundational role of the ANS for symbolic number representation also rely on evidence for overlapping subset of brain regions

(i.e. the intraparietal sulcus) responding similarly to symbolic and ANS numerical representations (Piazza et al., 2007), suggesting that ANS and symbolic number system are related on a neuroanatomical level. This theory predicts that at least during early math learning, the training of ANS may improve children's ability to form accurate symbolic numerical representations as well.

Further, some have hypothesized that the ANS serves as an online error detector to help avoid possible errors during arithmetic calculation (Feigenson et al., 2013; Lourenco et al., 2012) and thus helps students perform symbolic manipulations in real time (Theory Two). Students with more precise representations of non-symbolic magnitudes may be able to catch more implausible incorrect solutions resulting from procedural errors while completing symbolic arithmetic problems, resulting in greater symbolic arithmetic performance. Under this view, improved precision of ANS representations induced by ANS training might lead to more accurate answers to symbolic arithmetic problems by improving participants' abilities to detect gross errors. However, there is little empirical evidence available to directly test this claim.

A third theory suggests that the training of ANS may lead to greater interest in or attention to number-related information, which results in an improvement in symbolic math performance on a longer timescale (Libertus, 2019).

However, as noted above, studies cited in support of these hypotheses suffer from some common limitations. Principally, because most of the work cited in this section does not rely on studies that experimentally manipulate ANS acuity, this work

is unable to fully rule out the possibility that cognitive systems common to both performance on ANS tasks and symbolic math performance, and not a causal effect of ANS acuity on symbolic math performance, account for these associations.

Alternatives to these theories suggest that performance on ANS tasks and symbolic mathematics performance reflect individual differences in the hypothesized confounds listed above, and training the ANS might have small or null impacts on symbolic math performance.

A fourth theory hypothesizes that ANS training may improve symbolic number performance not via ANS acuity, but by training cognitive processes common to non-symbolic and symbolic number tasks such as arithmetic operations; that is, some of the cognitive processes involved in a mental arithmetic task with non-symbolic numerosities (e.g., visual attention, working memory) are the same as for symbolic arithmetic. If so, then perhaps training mental manipulations of non-symbolic number can be transferred to symbolic math performance (see Hyde et al., 2016 for a review; Park & Brannon, 2014).

These theories can be broadly understood to fit in two categories (although some combination is also possible): theories that posit ANS influences symbolic math learning directly (Theory One and Theory Two) and theories that posit a third variable influences both ANS and symbolic math skills (Theory Three and Theory Four).

Evidence from ANS Training Studies

Experiments in which individuals are randomly assigned to receive ANS training or not, attempt to circumvent these critiques. The logic is that if ANS and

symbolic math is causally related, experimentally manipulating ANS will improve performance on symbolic math tasks compared to non-ANS training conditions.

Typically, participants engage in an activity or game that aims at training ANS acuity or to some control group, and after ANS training, participants are given posttests measuring ANS acuity and/or symbolic math skills.

The training tasks vary in the targeted facets of ANS, including the acuity of the ANS and the ability to manipulate approximate quantities. In an intervention training ANS acuity, participants might receive extensive practice on an approximate numerical comparison task, wherein participants saw two arrays of dots with the same color either simultaneously or sequentially, or were presented with an intermix of one array of dots with two different colors and had to indicate which array or color had more dots. Typically, proficiency on the comparison task is measured as accuracy or reaction time. Besides, specific measures like the Weber fraction (w)¹ are usually computed as an alternative.

This approach has produced some positive effects. For example, intervention studies adopted or developed a computer program called “the Number Race” (NR), a set of adaptive computer games that include a mix of symbolic and non-symbolic number comparison, verbal counting, and basic arithmetic, has been found to increase children’s speed and accuracy at comparison tasks and on arithmetic performance (Obersteiner et al., 2013; Wilson et al., 2009). However, other studies failed to

¹ Weber fraction (w) is a measure aimed to describe the ratio-effect of non-symbolic comparison task, which is the smallest ratio of two numerosities that a participant can reliably discriminate (Halberda, Mazocco & Feigenson, 2008).

observe such training-induced effects on symbolic arithmetic performance from approximate number comparison training (Cochrane et al., 2019; Ferres-Forga & Halberda, 2020; Maertens et al., 2016; Park & Brannon, 2014).

Other studies have trained participants to manipulate approximate quantities, for example by having participants practice approximate addition or subtraction. In an approximate addition or subtraction task, participants view two arrays of dots sequentially and then a third array of dots. Participants then judge whether the outcome array was more numerous than the actual sum or difference of the initially presented two arrays. Reaction time and accuracy are commonly adopted as measures for the performance in the approximate arithmetic task. This approach has also yielded both positive and null findings of transfer to symbolic math tasks (Au et al., 2018; Gimbert et al., 2014; Khanum et al., 2016; Park & Brannon, 2013; Park & Brannon, 2014; Szkudlarek et al., 2020). For instance, Hyde et al. (2014) trained first graders on approximate addition, approximate number comparison, line-length addition, or brightness comparison and found that children benefited on the symbolic arithmetic test from approximate arithmetic and number comparison training relative to the other two groups. In contrast, no significant difference was found in symbolic arithmetic improvement from approximate arithmetic training compared to other training conditions in adults (i.e., numerical symbol ordering, approximate range, approximate number comparison) (Szkudlarek et al., 2020).

In addition, recent efforts have expanded on these findings to examine whether ANS training can lead to changes in performance on broader tests of

children's math achievement (DeWind & Brannon, 2012; Halberda et al., 2008; Libertus et al., 2020; Park et al., 2016; Szklarek & Brannon, 2018; Van Herwegen et al., 2018; Wang et al., 2016). These studies have reported both positive and null estimates of the causal effect of ANS training. For instance, Park and colleagues reported that children showed greater improvement on standardized math achievement after practicing non-symbolic arithmetic compared to children who practiced a picture-memory game (Park et al., 2016). In contrast, Szklarek & Brannon (2018) found no significant training effect on formal math performance with children who received approximate arithmetic training compared to those who received numeral identification and letter identification trainings.

Taken together, across intervention studies, ANS training has sometimes been found to improve children's symbolic math performance, although variation across training plans and other study features does not immediately clearly differentiate between which kinds of training work and which do not.

Critiques of the causal evidence from ANS training studies

Some researchers have questioned whether ANS training studies provide strong evidence of the effects of ANS acuity on symbolic math performance. One narrative review argued that the literature suffers from design and analytic limitations that preclude strong inferences about the causal effects of ANS on symbolic math performance (Szűcs & Myers, 2017). First, Szűcs and Myers argue many of the existing studies do not include a strong control group, because the training may be

more effective than a passive control group or an active control group with math-irrelevant interventions.

Second, Szűcs and Myers argue that some ANS training literature suffers from construct confounding, because training combines symbolic and non-symbolic stimuli. For example, Wilson et al. (2009) trained children with “The Number Race” (mixed approximate and symbolic numerical tasks) and reading software and found significant improvements in symbolic number comparison performance only in the former group. If the treatment involves symbolic number training, it is very difficult to attribute transfer to increased ANS acuity in the treatment group, particularly because the existence of transfer from some symbolic number skills to some other symbolic mathematical tasks is uncontroversial (e.g., Booth & Siegler, 2008; Siegler & Ramani, 2009).

Finally, Szűcs and Myers argue that some features of the ANS training literature suggest it may suffer from an inflated rate of false positives and overestimated effect sizes. These features include small samples underpowered to detect realistically sized effects, measuring multiple outcomes or comparing multiple treatments without corrections for multiple testing, and flexibility in data analysis when reporting pairwise comparisons. Inglis et al. (2017) conducted a p -curve analysis (a method to test whether published statistically significant p values from a set of previous studies are consistent with the null hypothesis, which predicts a uniform distribution of p values) of all the published ANS training studies at the time. The observed p -curve was upward slopping, indicating there were far more p values

close to .05 than would expected if they were drawn from a population distribution of modestly powered hypothesis tests. Consistent with concerns about a risk for false positives, Inglis and colleagues' findings suggested that the existing studies did not contain evidential value and are substantially underpowered with an estimated average power of 13%.

Specific ANS training studies have received additional methodological or theoretical critiques. Merkley et al. (2017) argued that Wang and colleagues (2016) failed to give pretests to rule out baseline imbalance on either the trained construct or the outcome task. Lindskog and Winman (2016) argued that the adaptive training method used in Park and Brannon (2014) made it appear that participants learned during the task, but when comparing performance on the task at baseline to performance after training, participants did not substantially improve. In other words, perhaps ANS acuity was not improving, and thus could not be responsible for an observed improvement in symbolic math performance. Park and Brannon (2016) responded that 1) ANS task improvement was not necessary to demonstrate effects of transfer from ANS to symbolic math performance, and 2) participants in the treatment group did improve at the trained approximate arithmetic task during the course of the study.

To summarize, up until now, there has been substantial disagreement about the extent to which ANS acuity influences symbolic math performance and about the extent to which ANS training studies provide support for this theoretical claim.

Current Study

The purpose of the present study is to test the effect of ANS training on symbolic math skills by synthesizing the evidence from the ANS training studies conducted through 2020. First, we examined the estimated transfer effects from manipulations designed to train the ANS onto symbolic math performance in a meta-analysis.

Second, we addressed how the effects of ANS on symbolic math might depend on factors hypothesized to be relevant to the observed transfer effects: the age of participants, the effectiveness of the intervention at increasing participants' ANS acuity, the type of ANS training, the type of the control group, and the duration of training.

As noted above, all these features have been hypothesized to be influential factors in previous review papers and empirical studies. Younger children have been hypothesized to benefit more from ANS training, because non-symbolic and symbolic representations may be more connected when children are learning about symbolic representations. Consistent with this possibility, one meta-analysis found a stronger correlation between ANS acuity and symbolic math performance in younger children than in older participants (Fazio et al., 2014), and thus we included *the age of participants* as a moderator. We also included *ANS task improvement from pretest to posttest* as a moderator: If ANS task improvement is a proxy for the extent to which ANS training improves ANS acuity, then the more participants' ANS acuity improves as a result of training, the more gains one might predict participants should obtain on their symbolic math performance. However, findings from several training studies

failed to demonstrate a convincing training-related improvement on a measure of ANS acuity after ANS task training (e.g., Au et al., 2018; Park & Brannon, 2014), which leaves an open question as to whether symbolic math improvement is specially related to the changes in ANS. Similarly, *the duration of training* may be a proxy for the extent to which training durably affected ANS representations, and thus was also included as a moderator. As claimed by Szűcs & Myers (2017), brief ANS exposure experiments may not be expected to have strong impact on numerical skills in general, especially with few training trials. However, empirical finding from a few ANS training studies demonstrated that training with long-term practicing was not sufficient to improve symbolic math skills (Cochrane et al., 2019). As such, it is hard to predict how the extent of training will influence the training effect. Based on findings from a few ANS training studies that have reported that approximate arithmetic training was more effective than approximate number comparison training, it is plausible that different ANS training programs are not equally effective (e.g., Park & Brannon, 2014), so we consider *training type* as a moderator and posit that training with approximate arithmetic task might have a larger effect on symbolic math than other training programs. The theoretical justification for this is not completely clear. Finally, *the type of control group* was hypothesized by Szűcs and Myers (2017) to be an important potential moderator, as passive control groups may perform worse than ANS training groups for reasons unrelated to approximate number representations (e.g., motivation). Thus we hypothesize a larger effect might be

observed with control groups with no training than those with any other kind of non-ANS training tasks.

Third, as a small effect size was observed for transfer to symbolic math, we conducted an exploratory analysis to test whether small impacts on a hypothesized underlying ANS construct are a plausible explanation for the observed small effect size by examining the transfer effect of ANS training to untrained non-symbolic tasks hypothesized to measure the ANS.

Method

Inclusion criteria and screening

Articles for this meta-analysis were identified at two stages. A flowchart of the identification and screening process for our original search can be found in Figure 1. At first stage, articles were identified in two ways. First, a computer search of ProQuest (databases used were ERIC (1966-Current) and PsycINFO (1806-Current), ProQuest dissertations, and PsycARTICLES) for obtaining samples of studies to be screened was conducted. Abstract, title and keyword were searched for the following search terms: The following search terms used included: ("approximate number system*" OR "ANS" OR "analog number system*" OR "number sense*" OR "non-symbolic number*" OR "number race*" OR "approximate number*") AND (train* OR interven*). The asterisk symbol following a specific term allowed for different forms of the term (e.g., interven* can include intervention and intervene, train* can include train and training). Second, we searched citations in previous reviews (Szűcs & Myers, 2017; Libertus, 2019). In total, our search yielded 1293 results. After

removing duplicates, we were left with a total of $n=1065$ records. We then pre-screened titles and abstracts for papers written in English and then for relevancy bringing the number of records down to $n = 498$ and then $n = 24$, respectively.

We imposed the following inclusion criteria for eligibility in the meta-analysis. First, studies had to provide approximate number training through either dot-array comparisons or approximate arithmetic. Second, studies needed to have some form of randomized control group whether it was another form of training, no training at all, or the counterfactual was approximated using the treatment group pretest performance in a within-subjects design. Third, studies were required to report impacts on transfer to symbolic math performance. Fourth, to be included in the database, studies had to report sufficient statistical information to enable the computation of an effect size (e.g., accuracy means and standard deviations). In cases where statistical information could only be found within a figure, such as a bar or line graph, the WebPlotDigitizer software was used to estimate the approximate means and standard deviations ($n=6$) (Rohatgi, 2019). In other cases where data were not readily available, we requested the information from authors ($n=3$) in which all but one provided missing information. However, only one of those effect sizes met the model specification for the exploratory analysis while the remaining did not meet our main model specifications and were thus excluded from analyses.

The original literature search yielded 24 records after title and abstract screening. Of these, 13 were excluded during the full text screening. In some cases, it was because control groups received a kind of training that did not allow us to isolate

an ANS training effect ($n=5$). For example, in a study that compared a group who received ANS training to another that received ANS+symbolic training or ANS+brain stimulation, a transfer effect could be attributed either the addition of symbolic or brain stimulation, but not to ANS training alone. In other cases for exclusion, no symbolic math transfer task was measured ($n=2$), only impure training or transfer tasks were included (e.g. if ANS training was combined with symbolic math training and the control group received neither, or if the only available transfer task tested both symbolic and non-symbolic knowledge) ($n=4$), or when studies double-dosed or implemented multiple rounds of training within the same session in which transfer was measured after each round ($n=2$). In total, our initial search yielded 11 eligible studies.

After completing the working version of this meta-analysis for circulation, a set of pre-registered experiments was published (Szkudlarek et al., 2020). We decided to update our estimates based on this and other new studies that met our search criteria published between the year of 2018 and 2020 (Ferres-Forga & Halberda, 2020; Libertus et al., 2020). Since Szkudlarek's study was an unpublished dissertation at the time of our original search which was overlapped with the published one in our second search, we only kept the published paper in our meta-analysis. In this way, effect sizes now included 13 eligible papers containing 116 effect sizes from 25 unique treatment vs control contrasts. The contrasts indicated whether the comparison for a particular effect size used a unique combination of treatment and control groups, since several studies included more than two groups.

Coding

The following variables were coded for the dataset. Study characteristics included: (a) author and year (b) a unique identifier for the experiment number, because some studies included multiple experiments with different samples or more than two training or control conditions within the same experiment, (c) an indicator for within or between-subjects design, (d) unique identifiers for treatment and control groups, because some comparisons use the same treatment or control group, (e) sample sizes for treatment and control groups, (f) age of the samples which was then dichotomously coded as child or adult, (g) descriptions of the training and control tasks, (h) the training and control task type, (i) descriptions of the effect size type calculated within the study, (j) relevant accuracy and reaction time means and standard deviations for transfer tasks, (k) descriptions of the transfer tasks, (l) the transfer task type, (m) training time in minutes when available, and (n) an estimate of ANS acuity training effect.

For transfer tasks, we included a short description of the tasks as well as categorized them as symbolic math (e.g., standardized math assessments, number ordering tasks, or any quantitative task using Arabic numerals), other non-symbolic (e.g. dot comparison, non-symbolic arithmetic, non-symbolic counting, or non-symbolic number-line estimation), or other tasks that did not fall in the first two categories (e.g. working memory, vocabulary, or general knowledge). For treatment training, we categorized them as approximate arithmetic (e.g., non-symbolic addition

or subtraction), approximate comparison (e.g., dots comparison), or combination (i.e. a combination of approximate comparison and comparison or some other task, such as approximate number-line estimation). For control training, we also categorized them as active (with there being another form of training), passive (no training), or self (when using the pre-test in a within-subjects design). We further expanded on this by coding for whether the active control group received numerical training, such as symbolic number ordering, counting, identification or other training task using only Arabic numerals or other training that did not use numerals, such as general knowledge, vocabulary, working memory, or brain stimulation. For ANS acuity, we used available standardized differences (Cohen's d) in either the Weber fraction, if available, or with the pre-post practice effect in the treatment group. When neither of these were available, the ANS acuity training effect was coded as missing. In our coding, we classified tasks as far-transfer if the task shared few common features (e.g., material properties, obvious cognitive demands) with the training program. Conversely, tasks were classified as near-transfer if the task was very similar with the training program. For example, a formal math test would be a far transfer task for a letter identification training program, whereas a letter identification task would be a near transfer task. Further, a dot comparison task with objects in place of dots would be a near transfer task for a dot comparison training program, but a non-symbolic addition test would be a far transfer task. The meta-analytic dataset is available for download as online supplementary material. The first two authors coded each effect size independently and reached 100% agreement.

Sample for meta-analysis

In order to estimate the far transfer effect of ANS training to symbolic math performance, we narrowed down the meta-analytic dataset through a number of decisions. First, we assembled a count of the number of effect sizes in which the transfer task for the treatment and control groups were both near transfer ($k = 1$) (the effect size was from a within-subject paper and had a different non-symbolic transfer task), near for treatment but far for control (these tasks were all non-symbolic and therefore irrelevant to our research questions; $k = 11$), far for treatment but near for control (these tasks were deemed irrelevant, because we deemed it unlikely that ANS training would make individuals better at any task than training that task specifically would; $k = 19$), and far for both treatment and control ($k = 84$). Within the far/far transfer category, the number of effect sizes were broken down by type of transfer task: symbolic math ($k = 44$), other non-symbolic tasks ($k = 8$), and other cognitive tasks unrelated to symbolic or non-symbolic math ($k = 32$).

Because our primary research question was about whether ANS training improves symbolic math performance, we restricted the analysis to the far/far transfer category. That is, we included only symbolic math tasks that were far transfer for both the treatment and control group as well as excluded control groups that received symbolic training. It is worthy to note that, despite the significance of Szklarek et al.'s (2020) study for this literature, the reported effect sizes of all the four experiments in the study were not included in the main specification of our meta-analytic models

estimating the transfer effect of ANS training on symbolic performance, as the control conditions included symbolic numerical content.

Thus our final analysis sample of effect sizes contained $k = 33$ effects from 11 unique studies involving a total of 754 participants (see Table 1 for the description of the included studies).

Moderators

In order to quantify the possible heterogeneity in effect sizes, we evaluated five potential moderators for all studies, based on the narrative review by Szűcs and Myers (2017), as well as one additional moderator:

1. *Age (categorical variable with two levels: child or adult)*: Data on participant age showed a split between young children (ages: 3-6 years) and young adults (ages: 18-26 years), so these were coded dichotomously as either child (1) or adult (0)
2. *Control group (categorical variable with two levels: active or passive)*:
Whether the ANS training group was compared to another cognitively demanding activity (e.g., working memory training or number-line estimate); business-as-usual groups were considered “passive”
3. *Training time (continuous variable)*: Mean number of hours spent ANS training.
4. *Type of training task (categorical variable with three levels: approximate arithmetic, approximate comparison or combination)*: The type of training task used in the study. This moderator included approximate arithmetic (e.g.

Park & Brannon, 2013, 2014), approximate comparison (e.g. Libertus et al., 2020), combination (e.g. Szudlarek & Brannon, 2018; Honoré & Noël, 2016).

5. *ANS acuity (continuous variable)*: The standardized mean difference (Cohen's d) between pre and post ANS training groups' scores on training task. These were independently calculated by each author, and then disagreements were revised until there was 100% agreement.

Effect size calculation

Hedges' g as effect size to measure the standardized mean difference was calculated for each comparison in the primary studies that met the inclusion criteria using the `escalc` function in `metafor` (Viechtbauer, 2010). This method also provided the sampling error variances.² We used Hedges' g for effect size because it provided a bias correction for small samples (below 20).

Modeling approach

A multilevel random-effects meta-analysis of these data was used to estimate and account for the amount of heterogeneity between papers and between different samples included in the same paper. Effect sizes were modeled as nested within comparisons containing the same treatment and control group in which each unique

² The meta-analytic dataset included 1 paper using a within-subject repeated measures design, so the following formula was used to determine the sampling error variance for those comparisons:

$$V(d) = (1/n + d^2/(2n)) \times 2(1-r)$$

In this model, V is the variance, d is the effect size, n is the number of studies, and r is the assumed correlation between measures. For the effect size, Hedges' g was used and we assumed a moderate correlation of $r = 0.5$ between the observations.

combination received a different identifier and repeating combinations had the same identifier. These comparisons were then nested within the unique study identifiers across all model specifications. The Metafor R package was used (Viechtbauer, 2010). In addition, robust variance estimation (RVE) with hierarchical weights was used to perform the intercept and meta-regression models as an alternative strategy to test the robustness of our main model results (Hedges et al., 2010; Tanner-Smith & Tipton, 2014; Tanner-Smith et al., 2016). RVE allowed us to model nested effect sizes (i.e., comparisons extracted from the same study). The effect sizes extracted from one treatment-control comparison were thus grouped into the same cluster. These analyses were performed with the Robumeta R package (Park & Beretvas, 2018; Fisher & Tipton, 2015). We ran an additional specification for a sensitivity analysis in which the within-subject comparisons for the main specification were removed ($n=1$; $k=1$). The effect sizes were removed to ensure that results were not sensitive to the inclusion of this study, which may have been more prone to history, maturation, and testing effects than studies with a randomized control group.

We also included one additional sensitivity analysis using symbolic controls, but we restricted effect sizes to those that were far transfer for both treatment and control groups. For example, a symbolic control that used numerical order training but was tested in arithmetic fluency would be treated as far transfer whereas a number identification training has near transfer to a number word knowledge task. Our preferred specification does not include these studies, because we thought these might unfairly bias the effect of ANS training downward given the reasonable prior that

symbolic training is likely to transfer more readily to symbolic tasks, on average. But we include this model, because symbolic control tasks were used in several articles, including Szkudlarek et al. (2020).

Publication bias

We used three separate methods to test or adjust for publication bias in the dataset because no single publication bias adjustment works under all data generating processes. First, we examined a funnel plot of the effect sizes included in our model to examine potential asymmetry and heterogeneity in the effect sizes (Egger et al., 1997). Effect estimates from studies are plotted against a precision measure from those studies (e.g., the standard error). Estimates of effects from smaller studies are more variable than those from larger studies leading to a larger amount of scatter towards the base of the plot. In the absence of bias, a symmetrical funnel shape is observed. We supplemented the funnel plot figure with an Egger's regression test to test for funnel plot asymmetry using the sampling variance as the predictor.

Second, we test the robustness of our overall estimate to a PEESE (precision effect estimate of standard error) adjustment, which uses a weighted-least-squares regression model where the variance (squared standard errors) of each sample is entered into the model as an additional predictor of effect sizes (Stanley & Doucouliagos, 2014). Assuming a true effect, publication bias is stronger for studies when the standard error is large and weaker when the standard error is small. In addition, we included an RVE adjustment for the PEESE model.

Third, we updated Inglis et al. (2017)'s *p*-curve analysis, following Simonsohn et

al. (2014)’s steps (for disclosure table, see Table S2 in Online Supplementary Materials). The p -curve analysis (Simonsohn et al., 2014; Simonsohn et al., 2015) relies on the assumption that p -values follow a uniform distribution under the null hypothesis. In contrast, when the null is false, p -values are right skewed (i.e., there are more low values than high values). This is true for the full 0 to 1 interval, but also for the interval from 0 to 0.05. Simonsohn et al. proposed that the shape of the distribution of significant p -values in a set of studies can be used to assess if they collectively contain evidential value. If the significant p -values follow a roughly uniform distribution, the pattern of results is more consistent with a population of null effects (Simonsohn et al., 2015). We followed Simonsohn et al.’s (2015) procedure and, for each included study based on the same inclusion criteria as our main meta-analysis, extracted the test statistic associated with the hypothesis of interest (whether the ANS training influenced mathematics performance). If there was doubt about which statistic to select (e.g., the study contained two control groups), we conservatively selected the comparison with the smaller p -value (retaining the other for a robustness check). We verified 4 studies included in Inglis et al.’s paper in accordance with our main specification and coded 8 additional studies independently.

Results

Descriptive statistics

The selection process according to our main specification model resulted in a final meta-analytic data set including 33 effect sizes from 15 contrasts in 11 studies.

The total sample size was 754. See Table 2 for descriptive statistics of the categorical and numerical moderators.

Far transfer to symbolic math

In this section, we examine the effect of ANS training on the performance in symbolic math tasks categorized as far transfer tasks for both treatment and control groups. Several estimates are shown in Table 3. The overall summary effects of the main and RVE models were identical: $b = .11$, with p values of .07 and .11, respectively (Table 3). Robustness checks included a model in which we removed effects from within-participants studies ($k = 1$): $b = .11$, $p = .08$, a model that added a PEESE adjustment ($b = -.04$, $p = .89$) as well as its robust variance estimation adjustment, PEESE (RVE) ($b = -.04$, $p = .85$), and a model including the symbolic controls ($k = 11$) ($b = .07$, $p = .17$). The Q test for heterogeneity from the main model did not indicate any significant heterogeneity within the effect sizes $Q(df = 32) = 44.57$, $p = 0.07$; however, moderator analyses of study-level covariates were predicted *a priori*, and given the plausibility of *some* systematic heterogeneity across these studies despite non-significant heterogeneity tests, we proceeded with our planned moderator analyses.

Moderator analysis

Given that the number of studies and total number of participants in the analysis sample was low ($k = 11$ and $N = 754$, respectively), we are unlikely to have sufficient power to reliably detect small interaction effects. Thus, we recommend the following results be interpreted with some caution. Participant age (category), type of ANS

training, training time, and control group type were entered together and separately as moderators in different models following our main specification of far transfer to symbolic math (Tables 4 & 5). The full moderator model did not explain a significant amount of heterogeneity $QM(df = 5) = 7.38, p = 0.19$ indicating that the combination of these factors did not significantly contribute to the heterogeneity in the effect sizes (Table 4). The test for residual heterogeneity was also non-significant $QE(df = 27) = 36.25, p = 0.11$, but again, this should not be taken as strong evidence for the null hypothesis of no residual heterogeneity. Across individual moderator analyses, we found that the ANS training effect differed by age, $b = -.26, p = .02$, such that the effect size of transfer to symbolic math in children was .26 SD less, on average, than the effect size found in adult samples, which was positive $b = .28, p < .01$ (Table 5). Individual moderator analyses for ANS training type, training time (in hours), and control group type were non-significant; however, larger positive effects were seen in ANS training type with the largest effect in approximate arithmetic compared to other forms of ANS training $b = .16, p = .07$ (approximate arithmetic), with negative but nonsignificant estimates indicating smaller estimated effects of approximate comparison or combination training types.

Because we were interested in how the pre-to-post ANS training effect moderated the effect sizes, we calculated Cohen's d for posttest-pretest scores on the trained task for each study in our main model to test whether effects from studies that produced larger ANS gains also showed larger impacts on transfer tasks. Four of the effect sizes came from a study in which the treatment group's ANS training effect was very large

($d = 18.03^3$). We thus decided to analyze models under three alternative exclusion criteria: (1) keeping the large ANS training effect ($d = 18.03$), (2) replacing the large ANS training effect by the second highest value in the data ($d = 3.16$), and (3) removing the large ANS training effect from that study, thus omitting that study from the moderator analysis. Estimates were highly variable across these models (Table S1). Estimates of the effect of an ANS training that produced a 1 *SD* increase in performance on the trained task ranged from -0.01 in the first model to 0.17 and statistically significant in the third model. Thus, we are hesitant to come to any strong conclusions regarding the moderating role of training effectiveness.

Publication bias

Funnel plot and PEESE model

A funnel plot of the distribution of effect sizes was used as a visual aid to detect publication bias (Figure 2). The effect estimates (Hedges' g) were plotted along the x-axis while the standard errors of the effect estimates were plotted along the y-axis. While the funnel plot does not clearly indicate publication bias, we do see that there is some heterogeneity in the effect sizes based on the scatter. The vertical line in the middle of Figure 2 represents the location of the estimated effect of ANS training on performance ($g = .11$, consistent with estimates in Table 3). Some negative effective sizes were as large as positive ones. The Egger's regression test also did not yield statistically significant evidence for asymmetry in the funnel plot ($p = .43$). The

³ We contacted the first author to verify these values. The author promptly sent the data used to calculate this value, which we reproduced exactly in our own calculations.

PEESE test and its RVE adjustment indicated a nonsignificant relation between the standard error on transfer effect size, $b = 0.52$, $p = .59$, $p = .41$, indicating no clear evidence for publication bias, although as noted, the PEESE adjustment caused the estimated effect of ANS training on symbolic tasks to flip from positive to negative in sign (Table 3).

p-curve analysis

We used *p-curve* to examine the distribution of study *p* values by using the *p-curve* app v4.05 (<http://www.p-curve.com/app4/>). The *p-curve* is shown in Figure 3. Of 6 significant *p* values, 2 were below .025. The binomial test for a right-skewed *p-curve* was not significant ($p = .8906$), suggesting limited evidential value of previously reported significant ANS training effects; this was also the case for the continuous test (full *p-curve*: $z = 0.43$, $p = .6655$; half *p-curve*: $z = -0.68$, $p = .2495$). The *p-curve* method also estimated the power of the studies included was 5%, 90% CI [5%, 32%], also indicating an absence of evidential value. The observed estimated power and 90% CI are similar with Inglis et al.'s findings, which were 13% and [5%, 54%], respectively. Thus, overall, *p-curve* indicated that the published literature did not provide strong evidence of a an effect of ANS training on symbolic math abilities.

Exploratory analysis of ANS task impacts

At least two explanations are consistent with small effect sizes observed for transfer to symbolic math. The first is that ANS acuity may have a very small influence on symbolic math performance. A second potential explanation is that the trained ANS acuity measured in these experiments may not be strongly affecting an

underlying, unified ANS construct. In this case, the ANS training literature might not bear strongly on the influence of ANS acuity on symbolic math performance one way or the other.

In order to test this second explanation, we conducted an exploratory analysis where we examined the transfer of ANS training to untrained tasks hypothesized to measure the ANS. We restricted the effect sizes to only include transfer tasks that were also non-symbolic. We allowed for the transfer for the treatment group to be coded as either “near” or “far” to examine whether there were larger effects for the tasks we had coded as near transfer. Near transfer tasks included ANS tasks that were very similar to the trained task but not exactly the same (e.g. dot comparison training to dot comparison task with objects in place of dots). Far transfer tasks included different non-symbolic transfer tasks (e.g. dot comparison training to non-symbolic addition). Our analysis relies on the assumption that authors included these tasks because they thought ANS training might affect them. We only included tasks labeled as “far transfer” for the control group, as we were interested in the transfer of the treatment group only and none of the control groups should have similar training to the treatment group. In total, we included 19 effect sizes to measure the transfer of ANS training to untrained non-symbolic tasks.

Table 6 reports the results of these exploratory analyses. The transfer effect of ANS training to other non-symbolic tasks was not statistically significant, $b = 0.16$, $p = .32$, although this estimate is larger than the estimated effects on symbolic transfer tasks in Table 3. Surprisingly, we find that the effect is marginally larger for near

transfer tasks compared to far transfer; $b = 0.08$, $p = .72$. In other words, impacts on near transfer effects were larger by about 0.08 SD compared to far transfer effects.

Thus, although estimated transfer effects from ANS training to untrained non-symbolic tasks were larger than to symbolic tasks, the estimates were still small.

Discussion

In this meta-analysis, we investigated the effect of approximate number representation on symbolic number representation by synthesizing the evidence from 11 ANS training studies. We found a small, non-significant effect of ANS training ($g = 0.11$, $p = 0.07$). Results from a p -curve analysis also indicate weak evidence for robust training effects. The results are largely consistent with Szkudlarek et al. (2020), even though these results were not included in our main specifications. Those estimates are consistent with small positive, null, or small negative effects of non-symbolic training on symbolic math performance.

Our examination of heterogeneity revealed that the meta-analytic models exhibit a nonsignificant amount of heterogeneity, some of which is accounted for by participant age, ANS training type, training time, and control group type. Given the number of studies we found and the average number of participants per study, we are unlikely to have sufficient statistical power to have detected smaller interaction effects. Therefore, these interpretations should be read with some caution. The ANS training effect in children was less than the effect size found in adults. This finding of a weaker training effect for children was surprising because theories of numerical development posit that ANS might transfer most strongly to symbolic number

learning in childhood, when children are learning the meanings of symbolic numbers.

Further, consistent with this theory, a previous meta-analysis reported that the correlation between the ANS and symbolic mathematics is stronger with younger children (mean age < 6 years) than older children (6-18 years) and adults (> 18 years) (Fazio et al., 2014). One possible reason for that might be there is larger measurement error in young children's skills than in older participants' (e.g., Bond & Lang, 2018). However, given the large number of moderators examined, along with the lack of studies that use the same combination of training and measures in both children and adults (e.g., ANS measurement, dose of training), this finding is open to alternative explanations as well; it may be a false positive or could have been produced by varying designs and analytic practices across these different kinds of studies.

As Szűcs and Myers argued in their review, the ANS training literature suffers from general design and analytic limitations that preclude strong inferences about the causal relationship between ANS training and symbolic math performance. Although we did not find strong evidence for larger impacts from studies lacking an active control group, the results from our *p*-curve analysis suggest that the previous ANS training studies are substantially underpowered to detect realistically sized effects.

In our exploratory analysis of the effects of ANS training on other untrained non-symbolic tasks, we found that such training effect ($g = .16$) was directionally larger than the effect of transfer to symbolic math ($g = .11$; although effects did not differ significantly from each other or 0), and perhaps small for a set of tasks assumed to be measuring the same construct as the trained task. To be specific, a standard

deviation boost in trained ANS task returns .16 SD on the transfer non-symbolic task, compared to our largest estimate of .11 SD on the symbolic math task. This makes the implications of the ANS training literature for theories of the development of children's mathematical cognition less clear. Depending on one's assumptions about the extent to which the ANS training effects influence the ANS, one could come to very different conclusions about the effect of the ANS on children's symbolic math performance (see Figure 4 for detailed information).

Under one interpretation (See Panel A in Figure 4), if we assume that performance on the trained tasks was improved completely via improvements to the ANS (and, presumably, that transfer tasks are substantially less influenced by the ANS), and pre-post training gains often exceed a full standard deviation, then we might conclude that the effects of the ANS on symbolic math performance are small (a standard deviation or more boost in the ANS returns approximately .11 SD on symbolic math performance). In this case, the small observed transfer effect of ANS training on symbolic math could result from a large effect of training on a unified ANS system and a small transfer effect from ANS to symbolic math. Under this interpretation, these findings provide evidence for a weak causal link between ANS improvements and symbolic mathematics. The observed correlations between ANS and symbolic mathematics might be therefore accounted for by domain-general cognitive skills, including visuospatial ability and inhibitory control (e.g., Clayton et al., 2015; Cui et al., 2019; Fuhs & McNeil, 2013; Szűcs et al., 2014).

In contrast, under an alternative interpretation (see Panel B in Figure 4), if we assume that improvement on trained tasks reflects a combination of test-specific improvement and real ANS improvement, and that the non-symbolic transfer task effect size of .16 is closer to the average effect on the ANS from ANS training, results change dramatically. In this case, an average symbolic transfer effect of .11 could be quite large! Assuming that training improves symbolic performance only via ANS improvement (a very strong assumption) the implied effect of a 1 SD boost to the ANS on symbolic math performance would be approximately $.11/.16$, or close to .7 SD. In other words, the small observed transfer effect of ANS training on symbolic math could result from the combination of a large effect of training on ANS system and a small transfer effect from ANS to symbolic math (see, e.g., Gilmore et al., 2013, for arguments for the latter view) or a small training effect on ANS and a large transfer effect from ANS to symbolic math (see, e.g., Feigenson et al., 2013, for arguments for the latter view). Therefore, this analysis suggests the ANS training literature might benefit from a stronger focus on whether, how, and how much ANS training influences the key causal cognitive variable(s) of interest.

Limitations

One of the key limitations of was that our meta-analyses including the main specification and exploratory analysis lacked sufficient power to reliably detect smaller interaction effects.

Because the lack of studies using the identical combination of training tasks and measures with both children and adults, our unexpected finding of the moderating effect of age with larger estimates for adults than for children is open to alternative explanations; it may be a false positive or could have been produced by varying analytic practices across these different kinds of studies, for example.

Conclusion

In this meta-analysis, we examined the impact of ANS training on symbolic math performance, using a multilevel approach. Overall, our analyses do not provide evidence for a robust causal effect of ANS training on symbolic math performance. We are able to draw some conclusions about the impacts of ANS training on symbolic math performance: Specifically, they are small, on average, and consistent with a small positive average impact an impact of 0, or even a small negative impact. Estimates are imprecise and not statistically significant in any specification; they are even negative in direction in the PEESE models; and the *p*-curve analysis raises additional concerns about the robustness of published statistically significant findings. Therefore, we caution the reader against treating .11 as an unbiased average estimate of the effect of ANS training on symbolic math performance.

However, ANS training studies are also designed to answer a more theoretical question: Does ANS acuity have a causal effect on symbolic math skills? We do not think we can draw strong conclusions about this claim on the basis of this meta-analysis. Our exploratory analysis makes clear that, for obtaining an unbiased estimate of the effect of ANS acuity on symbolic math performance, a difference in

the ANS training effect between exactly 0 and approximately .10 could be theoretically meaningful when these impacts are scaled up by dividing by the impacts of training on a general ANS acuity construct, as indexed by a non-symbolic transfer task. Unfortunately, existing sample sizes and training effectivenesses do not allow for precise estimates of this parameter, which would require some combination of much larger samples and much larger effects on non-symbolic transfer tasks. Therefore, the theoretical contribution of the ANS training literature on the four main theories of numerical development concerning the way in which ANS interfaces with symbolic mathematics performance is limited.

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Table 1

Description of Included Studies in Main Model

Author	Control conditions	Training plans	Age of samples	Training duration (hour)	Outcome measures
Park & Brannon (2013)	No training Numerical symbol ordering General knowledge training (general knowledge questions, e.g., What is a group of toads called? Club; Knot; Group; Hub; Pack.)	Approximate arithmetic training	Adult	2.5-4.17	Exact symbolic arithmetic (two- and three-digit addition and subtraction problems) Vocabulary Numeral order judgement
Gimbert, Gentaz, & Matenz (2019)	Sound identification training (to find the words which contained the sound they were studying e.g, [a], [i], [m], or [l]) in a nursery rhyme or on picture cards)	Approximate arithmetic training	Child	1	Visual non-symbolic comparison Haptic non-symbolic comparison Symbolic addition Symbolic verbal problems (e.g., “Denis has three marbles. He gets two more. How many marbles does he have now?”)

Table 1 (*continued*)

Author	Control conditions	Training plans	Age of samples	Training duration (hour)	Outcome measures
Park et al. (2016)	Picture memory training (to flip the faced-down cards over to make a pair)	Approximate arithmetic training	Child	2	Math assessment (TEMA-3) Vocabulary (PPVT-4) Letter-span task (to repeat a string of letters in the correct order) Stroop interference test Card sorting test
Cochrane et al. (2019)	No control	Approximate comparison training	Adult	13.08	Dot enumeration Ratio comparison Symbolic arithmetic (standard Woodcock Johnson math fluency sub-test) Multi-object tracking
Szkudlarek & Brannon (2018)	Letter identification training (to swipe the letter corresponding to the number word they hear with their finger) Numeral identification training (to swipe the numeral corresponding to the number word they hear with their finger)	Combination training (approximate arithmetic+ approximate comparison)	Child	2	Informal math test (e.g., Which is bigger or more, 6 or 8?) Formal math test (numeral identification questions, e.g., What number is this?) Vocabulary (PPVT-4) Card-sorting and stroop interference tests Letter span task Alphabet knowledge (to name each presented alphabet) Number word knowledge (give-a-number task)

Table 1 (*continued*)

Author	Control conditions	Training plans	Age of samples	Training duration (hour)	Outcome measures
Honoré & Noël (2016)	<p>Story understanding (to answer questions on the told story from a picture book)</p> <p>Arabic number training (comparison of Arabic numbers and Arabic numbers positioning on a number line)</p>	<p>Combination training (approximate comparison+ approximate number line estimation)</p>	Child	5	<p>Collection comparison (to select the box from two boxes containing the larger sets of puzzles)</p> <p>Dot sets on a number line (to place dots collections on a number line marked with one dot and 20 dots on the left and right extremities)</p> <p>Arabic number comparison</p> <p>Verbal number comparison</p> <p>Arabic digits on a number line</p> <p>Exact additions</p>
Park & Brannon, (2014)	<p>Visuo-spatial short term memory (to reproduce the block sequence in order)</p> <p>Numerical symbol ordering</p>	<p>Approximate arithmetic training</p> <p>Approximate comparison training</p>	Adult	2.5	<p>Exact symbolic arithmetic (two- and three-digit addition and subtraction problems)</p> <p>Vocabulary</p> <p>Non-symbolic numerical comparison</p> <p>Spatial 2-back test (to judge whether the location of the circle on the current trial matched the location of the circle two trials back in time)</p> <p>Numerical order judgement</p>

Table 1 (*continued*)

Author	Control conditions	Training plans	Age of samples	Training duration (hour)	Outcome measures
Au et al. (2018)	Vocabulary and general knowledge (e.g., “What did the Greeks call the present day Amu River in Iran? Indus; Ganges; Exibus; Jhelum; Oxus.)	Approximate arithmetic training	Adult	2.92	Non-symbolic estimation (the estimation of non-symbolic quantities by generating symbolic numbers) Symbolic estimation (the estimation of the magnitude of a symbolic number by creating a non-symbolic representation) Non-symbolic nonverbal count (to count the presented dots nonverbally by pressing the spacebar a corresponding number of times) symbolic nonverbal count (to count the presented numeral nonverbally) non-symbolic comparison fraction comparison symbolic arithmetic
Gouet et al. (2018)	Color comparison (to compare the color intensity of a target dot array, with the color of one of the two arrays previously seen) No training	Approximate arithmetic training	Child	2.46	Numeration (e.g., counting) Mental operations Written arithmetic Number line task Vocabulary (TEVI-R test)

Table 1 (*continued*)

Author	Control conditions	Training plans	Age of samples	Training duration (hour)	Outcome measures
Libertus et al. (2020)	Phonological awareness training (rhyming, syllable blending, and phoneme blending)	Approximate comparison training	Child	4	Visual ANS precision task Auditory ANS precision task Standardized math assessment (TEMA-3) Standardized phonological awareness assessment (Comprehensive Test of Phonological Processing) IQ Inhibitory control (the Conners' Kiddie Continuous Performance Test Version 5)
Ferres-Forga & Halberda (2020)	painting and word training (Tux Paint and Microsoft Word)	Approximate comparison training	Child	2.6	Symbolic math assessment

Table 2

Descriptive Statistics of Analysis Variables

	k studies	k contrasts	k effect sizes ¹
Age			
adult	4	6	11
child	7	9	22
Control group type ²			
active	11	13	28
passive	2	2	5
Type of training ²			
approximate comparison	4	4	5
approximate arithmetic	6	9	21
combination	2	2	7
	mean	sd	range
sample size	27.73	13.26	10-53
training duration (hour)	3.41	3.00	1-13.08
effect sizes of ANS acuity (Cohen's <i>d</i>)	2.96	4.86	-0.35-18.03

Note. Effect sizes are Hedges' *g* used in the modelling. The sums of k studies for moderator "control group type" and "type of training" are larger than the number of included studies (i.e. 11) for some studies having both active and passive control groups or more than one type of training.

Table 3

Main Effect of ANS Training Effect on Symbolic Math

	k	b	Q	QE	QM
Main (new)	33	0.11' (.06)	44.57		
RVE	33	0.11 (.06)			
Between only	32	0.11 (.06)	44.51		
Symbolic controls	44	0.07 (.05)	67.71*		
PEESE	33	-0.04 (.27)		43.96	.29
PEESE (RVE)	33	-0.04 (.52)			

Note. Main specification: far transfer to symbolic math for treatment and control with no symbolic math controls; Main (old) contains estimates prior to our updated search; RVE: Robust variance estimation of main model; Between: Includes only between subject comparisons; Symbolic controls: Includes estimates that have symbolic active controls and with symbolic tasks that are far transfer for both treatment and control; PEESE: corrects for publication bias.; PEESE (RVE): PEESE model with robust variance adjustment; SE in parentheses. b = Hedges' g; RVE = robust variance estimation; QE = test for residual heterogeneity; QM = test of moderator; k = sample size. ' p<.1 * p<.05 ** p<.01 *** p<.001

Table 4

Full Moderator Analysis Model of ANS Training Effect on Symbolic Math

	b	RVEb
Intercept	0.26'	0.26'
	(.15)	(.10)
child	-0.22	-0.22
	(.13)	(.11)
approximate comparison	-0.10	-0.10
	(.17)	(.21)
combination	-0.04	-0.04
	(.14)	(.10)
training time (hours)	0.02	0.02
	(.03)	(.04)
passive control	-0.22	-0.22
	(.15)	(.25)
QE(df=27)	36.25	
QM(df=5)	7.38	
k	33	

Note. Models follow main specification: far transfer to symbolic math for treatment and control with no symbolic math controls. Reference group is (adult+approximate arithmetic+0 training time+active control). SE in parentheses. b = Hedges' *g*; RVEb = robust variance estimation; QE = test for residual heterogeneity; QM = test of moderator; k = sample size. ' p<.1 * p<.05 ** p<.01 *** p<.001

Table 5

Individual Moderator Analysis of ANS Training Effect on Symbolic Math

	b	RVEb	QE	QM	k
Age group			39.01	5.33*	33
intercept (adult)	0.28** (.10)	0.28** (.03)			
child	-0.26* (.11)	-0.26* (.07)			
Training type			44.31*	0.48	33
intercept (approximate arithmetic)	0.16' (.09)	0.16 (.12)			
approximate comparison	-0.07 (.16)	-0.07 (.19)			
combination	-0.10 (.16)	-0.10 (.13)			
Training time			43.64'	0.57	33
additional hours	0.02 (.03)	0.02 (.03)			
Control group			41.56'	2.32	33
intercept (active control)	0.13* (.06)	0.13* (.04)			
passive control	-0.21 (.14)	-0.21 (.33)			

Note. Models follow main specification: far transfer to symbolic math for treatment and control with no symbolic math controls. Model names in bold. SE in parentheses. b = Hedges' g; RVEb = robust variance estimation; QE = test for residual heterogeneity; QM = test of moderator; k = sample size. ' p<.1 * p<.05 ** p<.01 *** p<.001

Table 6

ANS Training to Other ANS Task Transfer Effect

	k	b	Q
ANS	19	0.16 (.16)	37.32**
RVEb	19	0.16 (.16)	
Moderated by transfer distance			
	b	RVEb	
intercept	0.12 (.20)	0.12 (.14)	
near transfer	0.08 (.22)	0.08 (.15)	
QE(df=17)	37.32**		
QM(df=1)	0.12		
k	19		

Note. ANS model specification examines the effect of trained ANS tasks on other untrained ANS transfer tasks. Reference group for intercept within moderator model is far transfer. SE in parentheses. b = Hedges' g; RVEb = robust variance estimation; QE = test for residual heterogeneity; QM = test of moderator; k = sample size. ' p<.1 * p<.05 ** p<.01 *** p<.001

Figure 1

Flowchart of literature search and screening procedure

Note. The exclusion criteria for the pre-screened records: not clear pre-post (i.e. studies performed additional testing with more than one pre-post performance assessment), impure training or transfer task (e.g. ANS training combined with symbolic math training or transfer task tested both symbolic and non-symbolic knowledge), lack of clear control group (e.g. when a control group had another form of training, such as symbolic math or general knowledge, in addition to ANS), no symbolic math transfer task (e.g. when no symbolic math tasks were involved in the post-tests). Two extra included studies are Libertus et al. (2020) and Ferres-Forga & Halberda (2020).

Figure 2

Funnel plot of effect sizes (Hedge's g) of ANS training to symbolic math

Note. The effect sizes in this model are based on the main specification: ANS training vs. control groups (excluding symbolic math control).

Figure 3
The distribution of p-values for studies investigating whether the experimental manipulation influenced mathematics performance

Figure 4

Two Interpretations of Meta-Analytic Impacts

Note. In both cases, the impact of ANS training on the trained non-symbolic task is 1, the impact on the transferred non-symbolic task is .16, and the impact on the symbolic transfer task is .11. However, Panel A implies a much smaller impact of ANS training on symbolic math skills than Panel B.

Appendix

Table S1

Moderation Models of Pre-post Gains in ANS

	b	RVEb	QE	QM	k
No change			39.57*	0.59	26
Intercept	0.18'	0.18			
	(.10)	(.13)			
ANS acuity	-0.01	-0.01			
	(.02)	(.03)			
Imputed with 2nd highest value			31.86	6.60*	26
Intercept	-0.09	-0.09			
	(.09)	(.08)			
ANS acuity	0.13*	0.13*			
	(.05)	(.03)			
Treated as missing			25.80	10.50**	24
Intercept	-0.11	-0.11			
	(.09)	(.06)			
ANS acuity	0.17**	0.17*			
	(.05)	(.04)			

Note. Models follow main specification: far transfer to symbolic math for treatment and control with no symbolic math controls. Model names in bold. SE in parentheses. b = Hedges' *g*; RVEb = robust variance estimation; QE = test for residual heterogeneity; QM = test of moderator; k = sample size. Hedges' *g* in column 1 are not in relation to a reference group so can be interpreted as effect size; except for training time (relates to 0 minutes training). ' $p < .1$ * $p < .05$ ** $p < .01$ *** $p < .001$