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What Impacts Should We Expect from Tutoring at Scale? Exploring Meta-Analytic Generalizability

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U.S. public schools are engaged in an unprecedented effort to expand tutoring in the wake of the COVID-19 pandemic. Broad-based support for scaling tutoring emerged, in part, because of the large effects on student achievement found in prior meta-analyses. We conduct an expanded meta-analysis of 265 randomized controlled trials and explore how estimates change when we better align our sample with a policy-relevant target of inference: large-scale tutoring programs in the U.S. aiming to improve standardized test performance. Pooled effect sizes from studies with stronger target-equivalence remain meaningful but are only a third to a half as large as those from our full sample. This result is driven by stark declines in pooled effect sizes as program scale increases. We explore four hypotheses for this pattern and document how a bundled package of recommended design features serves to partially inoculate programs from these attenuated effects at scale.

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Exploring Meta-Analytic Generalizability

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Abstract

U.S. public schools are engaged in an unprecedented effort to expand tutoring in the wake of the COVID-19 pandemic. Broad-based support for scaling tutoring emerged, in part, because of the large effects on student achievement found in prior meta-analyses. We conduct an expanded meta-analysis of 265 randomized controlled trials and explore how estimates change when we better align our sample with a policy-relevant target of inference: large-scale tutoring programs in the U.S. aiming to improve standardized test performance. Pooled effect sizes from studies with stronger target-equivalence remain meaningful but are only a third to a half as large as those from our full sample. This result is driven by stark declines in pooled effect sizes as program scale increases. We explore four hypotheses for this pattern and document how a bundled package of recommended design features serves to partially inoculate programs from these attenuated effects at scale.

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Introduction

We are living in a rare moment where a collective effort is underway to change one of the core organizing principles of modern schooling. Historically, education – both formal and informal – was primarily an individualized endeavor with tutors and pupils or master craftsmen and apprentices working together, one-on-one. The rise of large-scale public education systems over the last two centuries evolved around a different organizing principle – one in which teachers became charged with the task of educating entire classrooms of students (Tyack, 1974). While teaching students in groups allowed these systems to expand access rapidly, it also created substantial challenges for educators to meet the full spectrum of students' individual needs.

The COVID-19 pandemic toppled the precarious balance teachers have long tried to achieve between whole-class instruction and differentiated instruction. The public health crisis caused widespread school closures as well as acute hardships for many families. In the U.S., researchers estimate that median student achievement fell 0.24 standard deviations (SD) in math and 0.13 SD in reading, with even larger declines among low-achieving students (Callen et al., 2024). The pandemic both exacerbated longstanding inequalities in student achievement and created a shared priority to accelerate learning. This crisis caused the historical pendulum to start swinging back towards individualized instruction as a means of meeting the needs of all students.

In the months following the pandemic, a rare consensus emerged among policymakers, researchers, and practitioners that tutoring had a critical role to play in addressing the educational harms caused by COVID-19. Integrating tutoring into the public education system at scale has become a primary policy response to pandemic-related learning disruptions. Unlike previous unfunded attempts to scale tutoring, such as President Clinton's America Reads initiative, the federal government and individual states catalyzed these efforts with substantial financial investments (NSSA, 2023). The federal Elementary and Secondary School Emergency Relief Fund (ESSER) provided $190 billion to public schools and required districts to spend a sizable fraction of this on student learning acceleration (including 20% of the third wave of ESSER funding) (Goldhaber & Falken, 2024). One estimate projects that states and districts have so far spent over $3 billion of this aid on tutoring (DiMarco & Jordan, 2022).

Efforts to scale tutoring after COVID-19's onset appear to have substantially expanded access to individualized instruction in U.S. public schools. The nationally representative School Pulse Survey found that by December of 2022, 37% of schools reported offering high-dosage tutoring, defined as "Tutoring that takes place for at least 30 minutes per session, one on one or in small group instruction, offered three or more times per week, is provided by educators or well-trained tutors, [and] aligns with an evidence-based core curriculum or program." This statistic increases to 59% when schools were asked if they offer more standard tutoring defined as a less intensive and structured approach to individualized instruction. At the same time, districts have yet to implement these programs at the scale or dosage many believe is required to support a full academic recovery (Goldhaber et al., 2022). Only 20% of schools that reported offering high-dosage tutoring (and 15% that offered standard tutoring) strongly agreed that they were able to effectively provide tutoring to all students in need.

Efforts to expand access to tutoring were, in many ways, evidence-based policy. Meta-analyses conducted by several independent research teams that reviewed randomized controlled trials (RCTs) of tutoring programs have all found large effects of tutoring on test-based measures of achievement in the range of 0.3 to 0.4 SD (Dietrichson et al., 2017; Fryer, 2017; Inns et al., 2019; Nickow et al., 2020, 2024; Pellegrini et al., 2021). These effects are roughly equal to an 11 to 15 percentile point increase, or the amount of learning in reading that upper elementary students in the U.S. typically make in an entire school year (Hill et al., 2008). These impressive findings played a central role in motivating calls by policymakers and researchers – including ourselves (Kraft & Falken, 2021; Robinson et al., 2021) – to advocate for scaling tutoring. Influential technologists such as Mark Zuckerberg and Sal Khan have extolled the moonshot-like potential of tutoring, evoking the eye-popping 2 SD effects found in small-scale studies conducted by University of Chicago doctoral students under the supervision of Benjamin Bloom in the 1980s. However, scholars have recently raised new critiques about Bloom's 2-sigma studies (Barnum, 2018; von Hippel, 2024) and the generalizability of pooled effect sizes generated from meta-analytic reviews (Dahabreh et al., 2020; Littell, 2024; Slough & Tyson, 2023).

In this paper, we conduct an expanded and updated meta-analysis of RCTs evaluating tutoring programs to explore the external validity of pooled effect size estimates. The common empirical focus on RCTs bolsters the internal validity of meta-analytic estimates, allowing researchers to draw credible inferences about the causal impacts of tutoring programs on student outcomes. However, despite combining findings from a wide variety of settings, samples, and treatments, meta-analytic reviews of experimental studies with small to medium non-probability samples do not necessarily produce estimates that generalize to broader efforts to scale tutoring (Littell, 2024). As many scholars have highlighted, strong internal validity does not beget broad external validity (Banerjee & Duflo, 2009; Esterling et al., 2024; Pritchett & Sandefur, 2015).

We seek to answer the question: What expectations should we have for tutoring effects on standardized test scores for large-scale programs implemented in the U.S.? We address this question by generating pooled effect sizes from a sample of 265 RCTs published between 1967 and 2023 and examining the sensitivity of our results to sample restrictions that better align our estimates with a specific, policy-relevant target of inference: large-scale tutoring programs in the U.S. aiming to improve achievement on standardized tests. Consistent with prior meta-analyses, we find a large, pooled effect size of 0.42 SD on student achievement across our full sample. These effects are driven, in part, by the strikingly large effects of literacy tutoring programs in elementary grades (0.46 - 0.48 SD), which constitute 73% of the estimates in our sample.

Our analyses reveal a stark pattern of declining effects of tutoring programs when taken to scale. When we restrict our sample to larger-scale tutoring programs implemented in the U.S. and evaluated based on third-party standardized assessments, our pooled estimates shrink to a third to a half the size of our unrestricted estimates. This difference is largely due to restricting the sample to large-scale programs. In our preferred analytic samples, we estimate a pooled effect size of 0.21 SD for programs serving 400 to 999 students and 0.16 SD for programs serving 1,000 students or more. We view these effects both as more plausible for large-scale programs and as still having considerable policy importance given their meaningful magnitude and strong external validity. Still, such effects for tutoring at scale are far from guaranteed. We observe considerable variation across individual tutoring program effects, and estimates from quasi-experimental studies of programs serving thousands of students are often even smaller.

We then explore several hypotheses that might explain the pattern of declining effects with scale – a widely documented phenomenon in the broader education research literature (Cheung & Slavin, 2016; Kraft, 2020, 2023). We find mixed evidence that the declining results are an artifact of selective reporting due to publication bias or *p*-hacking. It is possible, however, that the common practice to use the within-sample standard deviation to estimate effect sizes causes these estimates to be artificially larger for studies of smaller programs serving more targeted and homogenous populations. We also find evidence that some tutoring program design features differ systematically across program size, with increasing student-teacher ratios and declining dosage. We suspect another possible explanation for declining effects with scale is that tutor effects are heterogeneous across students, causing the marginal effect of tutoring to decline as programs expand to serve more students that stand to benefit somewhat less. Finally, we also find evidence from recent studies that implementation quality often declines at scale.

Encouragingly, we do find that a combination of tutoring program design features identified in the research literature as best practices somewhat buffers against the large decline in effects we find at scale. However, schools and districts are often motivated to expand tutoring while operating within budget constraints. This creates a tension between maintaining fidelity to best practices and supporting more students. We conclude by examining how common approaches to addressing scaling challenges, such as high program costs and limited tutor supply, might affect program efficacy at scale. We find generally inconclusive evidence about the ability to maintain program efficacy when tutoring is delivered online, when student-tutor ratios are increased, and when dosage is decreased. We find suggestive evidence that peer tutoring may offer a cost-effective model for scaling.

Our study makes several contributions to literature. We extend prior tutoring meta-analyses by compiling a sample of 265 RCTs, roughly three times the number of studies as the largest prior reviews. This large sample allows us to explore how our overall effect size estimates compare to those for sub-samples of studies that are more aligned to the target of inference used by many researchers and policymakers. Second, our study also serves as an applied example of why it is critical to attend to external validity when conducting meta-analytic reviews and engaging in evidence-based policymaking. Finally, our analyses generate important insights to inform ongoing efforts to take tutoring to scale within the U.S. public school systems in a sustainable way. Our findings provide stronger, more externally valid evidence to support investments in tutoring, while also recalibrating expectations towards more plausible gains for students.

Methods

Literature Search Procedures

We began by searching for articles in seven electronic databases, including Academic Search Premier, APA PsychInfo, AEA EconLit, ERIC, Google Scholar, Science Direct, and Web of Science. We also searched two working paper series, from the Brown University Annenberg Institute and the National Bureau of Economic Research, to ensure we captured studies not yet published in peer-reviewed outlets (Alexander, 2020; Pigott & Polanin, 2020). Searching this range of sources was essential to minimize the extent to which we were missing key research, especially work produced by scholars from historically marginalized groups (Boveda et al., 2023). Our search terms included keywords related to (a) tutoring (e.g., “tutor”), (b) educational contexts (e.g., “school”), and (c) impact evaluation research methods (e.g., “RCT”). We used Boolean operators between all terms, specifically “OR” between terms within each of these three keyword categories, many of which were synonyms, and “AND” between each of the three categories to maximize the relevance of search results without overlooking key studies. We identified 45 preexisting reviews and meta-analyses of tutoring-related interventions and scanned the reference lists of these for new studies. We supplemented this search by monitoring social media and email newsletters from research centers. We continued our literature search through the end of 2023, the cutoff date for studies we formally coded. Though we stopped coding new studies, we continued to track newly released studies and incorporate several into our narrative synthesis and discussion. Our search generated over 14,000 studies. After removing duplicates, we followed Pigott and Polanin (2020) and had two team members conduct an initial screening for relevance using titles and abstracts. This left 1,347 studies that we subjected to an in-depth inclusion review of the full texts, ultimately resulting in a final analytic sample of 265 studies.

Inclusion Criteria

To identify our analytic sample, we assessed studies against eight inclusion criteria: 1) human tutoring, 2) 1:1 or in small groups, 3) focused on academics, 4) measured effects on standardized tests in math or reading, 5) K-12 students, 6) in an OECD country, 7) RCT design, and 8) randomized more than 20 students or 4 classrooms. First, programs under study needed to meet a broad definition of tutoring: one non-parental individual providing academic support to a single student or small group of students. We excluded studies of individualized instruction provided by a book, computer program, or other curricular tool without the direct support of a human tutor. While we included studies of programs where the tutor was a teacher, paraprofessional, college student, volunteer, or peer, we excluded studies of parent tutoring programs because all relevant studies we identified evaluated models of parent training or professional development rather than direct parent-child instruction. Second, the tutoring intervention must have been implemented with either a 1:1 student-tutor ratio or in groups of 8 or fewer students.[1] Third, the tutoring content had to focus on academic subjects. This excluded, for example, studies of mentoring or socioemotional interventions without an academic component. Fourth, our focus on academic interventions also meant that studies needed to report effects on academic outcomes, specifically standardized tests and researcher-generated assessments measuring performance in either reading or math. We excluded studies where the only outcome was a non-test academic measure (e.g., GPA, attendance) because the sample of these studies

[1] We recognize not everyone would characterize instruction in groups of 5-8 as an authentic tutoring program. We include such programs because authors commonly applied this term and because it allowed us to cast a wide net.

was too small to facilitate broad comparisons. Fifth, the tutees had to K-12 students. This excluded studies of tutoring in early childhood settings, of college or graduate students, and of adults. Sixth, the intervention had to take place in a member country of the Organization for Economic Co-operation and Development (OECD) given our primary focus was on informing policy in the U.S. (a high-income nation). Seventh, we limited our sample to RCT designs to parallel prior reviews and given RCTs' relative advantage at isolating causal impacts. That said, we supplement our meta-analysis with a synthetic review of recent quasi-experimental studies, which helps us consider tutoring impacts at a scale not captured by most RCTs. Finally, the studies had to have a sample size of more than 20 students when randomization occurred at the student level, or more than four classrooms or schools when randomization was at the classroom or school level.

We also applied inclusion criteria to the effects reported and coded all qualifying estimates from each study. First, the effect estimates had to examine the same outcome as the subject of the tutoring (i.e., we dropped estimates of the impact of math tutoring on reading achievement). Second, we focused on treatment-control contrasts that isolated tutoring whenever possible, dropping estimates where the control condition involved tutoring-like programs and comparisons between treatment arms without a pure no-tutoring control group. However, we included studies in which we judged tutoring to be a key element of a larger set of interventions and reforms that together were evaluated against a business-as-usual control group. Finally, we prioritized estimates from reduced form models that capture the effect of offering tutoring. We view these intent-to-treat estimates as the relevant impact for the types of inferences policymakers often make about what the effect of a program will be as implemented at scale.

Coding Procedures

Our research team of 20 coders double coded each study in our sample. Coders were trained on a common set of studies until they achieved a consistently high agreement rate with master codes created by our most experienced coders. After coding each study independently, coders then met to reconcile any differences and arrive at a final set of codes. When a pair of coders felt that the reconciliation was not straightforward, they brought questions to the principal investigators for a final determination. The team kept a record of decision rules that resulted from these meetings to ensure consistency across coders and over time.

Our codebook included 128 codes that we grouped into five categories. Some codes varied at the study-level while others varied at the intervention- or estimate-level. The first group of codes catalogued study information such as publication type (e.g., peer-reviewed article, working paper) and publication year. The second group tracked information about the context in which the study occurred such as the country, school level, and participant demographics. The third set covered information about the intervention itself and the treatment/control contrast (e.g., student-tutor ratio, the dosage, tutor type). The fourth category was information on the methods used by the study's authors such as the level of assignment to treatment, whether standard errors were clustered at the appropriate level, and whether we had concerns about attrition or contamination of the randomization. The fifth set included information about the effects, including estimated effect sizes, standard errors, sample sizes, and outcome instruments.

We highlight one key code that we use throughout our analyses: the number of treated students. Prior meta-analytic reviews often explore how effect sizes vary by the total sample size of an evaluation. We take a somewhat different approach given our focus on identifying studies that are more closely aligned to a specific target of inference. We code the number of students randomly assigned to receive treatment as an estimate of the number of treated students.

Although we are conceptually interested in the actual number of students who participated in tutoring, this quantity was not consistently reported across studies. Thus, our code for the number of treated students serves as an upper bound of the actual size of the tutoring program.

Calculating Effect Sizes

Study authors reported treatment effects in a variety of ways. Whenever they were available, we defaulted to relying on standardized effect sizes generated from linear regressions estimating standardized mean differences between the treatment and control group, often controlling for baseline covariates. One advantage of model-based estimates is that the associated standard errors typically account for the ways data may be clustered, as recommended by Hedges (2007). When these estimates (and/or their associated standard errors) were unstandardized, we standardized them using unadjusted pre-treatment control group SD whenever possible (if unavailable, we used pooled SD). In other cases, we estimated a standardized effect size using the pre-post treatment means, SD, and sample sizes for the treatment and control group. For each estimate, we then calculated a Hedges' g effect size, correcting for upward bias present for small-sample studies (Borenstein et al., 2009) as follows:

$$g^* = \left(1 - \frac{3}{4(n_T + n_C) - 9}\right) g$$

Here, g^* is the corrected effect size estimate, n_T is the number of treated units (i.e., students or classrooms), n_C is the number of comparison units, and g is the uncorrected effect size estimate.

Meta-Analytic Approach

We generated our pooled standardized effect size estimates using robust variance estimation (RVE) meta-analytic methods (Hedges et al., 2010). Like other meta-analytic techniques, this approach up-weights effects estimated with greater precision, but RVE is unique in that it also accounts for the nesting of impact estimates within clusters. We often observe multiple estimates for a given study (for example, when there are multiple outcomes or interventions examined in a single study) and therefore model this most prevalent type of dependency, as recommended by Tanner-Smith and Tipton (2014). We fit the following model:

$$Y_{ij}^{k} = \beta_0^k + u_j^k + \varepsilon_{ij}^k$$

where Y_{ij}^{k} represents an impact estimate *i* on outcome *k* (either math, reading, or stacking subjects together in a single analysis) from study j. β_0^k is the overall weighted average impact of tutoring on outcome *k* and u_j^k is a study-level random effect. ε_{ij}^k is the residual of a specific effect size estimate from the average effect within its study. In addition to pooled effect estimates and associated standard errors, we also report prediction intervals for select estimates to describe the degree of heterogeneity in our sample and to illustrate the range of plausible effects policymakers might expect for an individual tutoring program (Borenstein et al., 2017).

Towards More Credible Estimates of Program Design Feature Effects

Researchers have typically explored the relative importance of various program features by comparing the pooled effect sizes of tutoring programs with different features. This approach is limited, however, because program features are often bundled and could be correlated with unobserved aspects of program quality (Tipton et al., 2023). We attempt to reduce these potential biases using meta-regressions to examine which moderators predict larger impact estimates, conditional on other study and program design features. We estimate the following model:

$$Y_{ij}^{k} = \beta_0^k + \Gamma X_{i/j} + u_j^k + \varepsilon_{ij}^k$$

Here, we include a vector of study and intervention features ($\Gamma X_{i/j}$). While this model does not allow us to isolate the causal impact of a particular intervention feature on student outcomes, it does allow us to tease apart which of the observable study and intervention characteristics are driving the largest differences in effect size estimates. When possible, we complement these analyses with results from multi-arm RCTs that randomly assign students to tutoring programs that differ only by a single design feature. These studies provide credible causal estimates of the effect of specific program design features but are often underpowered to detect small to medium differences in effects produced by modifying only one aspect of a tutoring program.

Target of Inference

Our aim is to draw inferences about tutoring programs that are most relevant for the target, context, outcomes, and scale of tutoring programs envisioned by U.S. policymakers. Specifically, we hope to inform the expectations of leaders who are seeking to address overall declines and growing gaps in academic outcomes post-COVID by integrating tutoring into the U.S. K-12 public school system. We imagine that because leaders are being held accountable for results on statewide standardized exams that assess a broad set of basic skills, policymakers will be more interested in tutoring impacts on standardized exams that measure general skills as opposed to assessments that measure narrower sets of skills or that are designed by researchers to align tightly with the focal content of the tutoring intervention. Furthermore, our U.S.-centric policy focus makes studies conducted in the U.S. likely to be the most relevant for our target of inference.

A central motivation for our work is to inform efforts to significantly expand access to tutoring programs. We therefore aim to draw inferences about reasonable expectations for the impacts of tutoring programs implemented at scale, as opposed to small-scale pilot programs. Throughout the paper we present estimates of pooled effects sizes across four bins of program size: 0-99, 100-399, 400-999, and 1,000 or more students. We can approximate the relevance of these bin sizes for tutoring programs through a back-of-the-envelope calculation.

Survey data from the "School Pulse Panel" (SPP) show that among districts offering high-dosage tutoring, approximately 28% of district students participate (NCES, 2024). This likely represents a lower bound estimate of the total need for high-dosage tutoring, given that 37% of 4th graders scored below basic on the 2022 National Assessment of Education Progress exam in reading and 38% of 8th graders scored below basic in math. Assuming districts target an average of 28% of students for tutoring, these bins partition public school districts at the 22nd, 57th, and 80th percentiles of district size.[2] Importantly, even though these bins capture a similar number of districts, the number of students in each bin is highly skewed towards the larger program sizes. Districts in the 0-99 program size bin serve 1% of all public school students, 100-399 8%, 400-999 15%, and 1,000+ 76%. Although only 20% of districts might intend to build tutoring programs that serve 1,000 students or more, roughly three-fourths of all public school students attend districts of this size, making it a policy-relevant focus of our analysis.

Findings

Characteristics of Included Studies

Our final analytic sample includes 265 RCTs that evaluate 340 distinct tutoring interventions. We present characteristics of these studies at the study/RCT-level in Table 1. Our sample skews towards recent research with almost two-thirds of included reports published in the years since 2009 and almost 80% in the last 20 years. Only five studies in our sample assess interventions implemented since the beginning of the pandemic, almost all of which provided remote tutoring, giving us limited power to disentangle virtual delivery from the pandemic context. Three-fourths of studies in our sample are peer-reviewed journal articles. The modal study examined a tutoring program in an urban, public school setting.

[2] Corresponding district size for the four bins is 1-357; 358-1,428; 1,429-3,571, and 3,572 or more students.

Our sample reflects substantial imbalance in the subject, grade-level, and size of tutoring programs evaluated in the literature, as illustrated by the evidence gap maps shown in Figures 1 and 2 (Polanin et al., 2023). Most of the studies assess literacy tutoring among early elementary school students (51%) and programs serving fewer than 100 students (59%). This concentration on small elementary reading programs is worth noting because if impacts differ across grade-levels, subjects, or with program scale, pooled results based on our full sample may not be immediately generalizable to other program types.

We provide further details on the characteristics of the programs evaluated in each of these studies in Table 2. Most interventions were delivered in-person (97%), at school (86%), during school hours (76%), using a 2:1 student-tutor ratio or less (62%), and with a provided curriculum (89%). Although individual tutoring was the modal approach (46%), student-tutor ratios varied widely across the sample. We observe greater variation in design choices across the features of tutor type, dosage, and whether students were pulled out of class for tutoring.

Full Sample Estimates of Tutoring Impacts

Similar to prior tutoring meta-analyses, we find notably large, pooled effect sizes across our full sample of studies. As shown in Table 3, we estimate that the average effect on student achievement of a broad variety of tutoring interventions subjected to rigorous evaluation via RCTs is 0.42 SD when stacking math and reading achievement impacts. The associated prediction interval ranges from -0.31 SD to 1.16 SD, illustrating the considerable heterogeneity of impacts we might expect across individual tutoring programs. This large average effect is driven, in part, by the pooled effects of literacy tutoring in lower and upper elementary grades of 0.46 and 0.48 SD, respectively, which make up a large portion of our sample (84%). That said, the pooled effects of tutoring on math achievement are still quite large (0.39 SD). We find inconsistent patterns in tutoring effect size across schooling levels by subject. Impacts of reading tutoring for elementary school students are substantially larger than for middle and high school students. In math, we find the largest effects at the high school level (0.55 SD) followed by upper elementary (0.44 SD). However, for both subjects, we only observe a small sample of high school program effects (13 estimates for math and 27 for reading). The magnitudes of effects are still moderate to large for the school levels and subjects with the smallest pooled effects (0.33 SD for lower elementary math and 0.16 SD for high school reading).

Sensitivity Analyses

We next explore whether our pooled estimates are robust to a variety of sensitivity checks in Table 4. First, we examine whether results differ for studies that may have lower internal validity due to quality concerns with the randomization design or empirical analyses. For example, some authors described their methods as an RCT but indicated or intimated that students, teachers, parents, or administrators had some influence over whether a student ended up in the treatment or control group. Another example is when a sizable number of students were excluded from the analytic sample because of non-compliance, attrition, a move, or some other reason. When we separately examine results based on studies for which we did not have quality concerns, results remain essentially unchanged. Our results are slightly sensitive to excluding outlier effects. When we omit the top and bottom 2.5% of effect size observations the pooled effect size estimate drops to 0.38 SD, a decline that is largely driven by a reduction in pooled reading effects from 0.44 to 0.37 SD.[3]

Finally, we examine whether estimates vary by the decade in which they were published as a rough proxy for study quality. Education research has taken major leaps in terms of

[3] The range of the lowest 2.5% of estimates was -1.70 to -0.34 SD. For the highest 2.5% of estimates, we observe a range of 2.24 to 8.06 SD.

methodological rigor and quality standards over the past three decades, particularly in applying causal inference methods (Angrist, 2004).[4] As shown in Table 4, we find substantial variation in the magnitude of the pooled impacts based on publication decade, with larger estimates prior to 2000 (0.45 SD) and between 2000 and 2009 (0.58 SD) than for those published between 2010 to 2019 (0.38 SD). We observe the smallest impacts for the most recent studies published in 2020 or after (0.27 SD), though even this is large in magnitude. We cannot definitively disentangle whether this variation in impacts is due to methodological changes, policy changes, or other study or program characteristic changes over time, but differences across decades remain even after we control for a host of study and program characteristics, as shown in Table 8.

What expectations should we have for tutoring effects at scale?

Evidence from our meta-analysis of experimental studies. In Table 5, we explore how our pooled effect size estimates change when we restrict our sample to more closely approximate our target of inference. Removing estimates that rely on assessments designed by the research team induces a modest 0.07 SD decline in our aggregate estimate. Removing studies conducted outside the U.S. only trivially reduces our pooled estimate by 0.03 SD. However, restricting the sample to studies that provided tutoring to incrementally larger groups of students profoundly changes the magnitude of our estimates. Using our full sample, we find that programs offering tutoring to fewer than 100 students have a pooled effect size of 0.55 SD, whereas programs tutoring between 100 and 399 students have a pooled effect size of 0.32 SD. As shown in Figure 3, this estimate continues to decline – almost linearly – as we further restrict the sample such that

[4] Another reason we chose to examine effects by decade is because the decades roughly correspond to major periods of education policy development, with the pre-2000 studies representing the period prior to universal test-based accountability, the 2000 to 2009 period representing the No Child Left Behind (NCLB) era, the time from 2010 to 2019 as representing the push for common core standards and expanded teacher-based accountability as part of Race to the Top and NCLB waivers. Finally, we view the 2020 to present period as representing the post-COVID era.

pooled effects for programs serving between 400 and 999 students and 1,000 or more students have an average effect of 0.25 SD and 0.14 SD, respectively.

When we apply both sample restrictions to best approximate our target of inference, we arrive at our preferred set of estimates presented in Panel D of Table 5. These impacts range from 0.21 SD to 0.16 SD for U.S. tutoring programs evaluated using standardized test outcomes and operating at a scale of 400 to 999 and 1,000 or more students. There are four important points to highlight about this preferred set of estimates. First, they are between a third and a half as large as the pooled estimate using our full meta-analytic sample, suggesting that inferences made using the broader sample are not well-calibrated to tutoring programs at scale. Second, effect sizes between 0.16 SD and 0.21 SD are of medium to large magnitude and still very impressive for large-scale education interventions (Kraft, 2020). Third, our pooled effect size estimate for programs serving 1,000 students or more is very imprecisely estimated given the limited number of RCTs of tutoring programs at this scale that meet our target-of-inference-aligned inclusion criteria. Fourth, the wide prediction intervals associated with these estimates suggest that we should expect tutoring program effects to vary considerably, with some individual programs producing quite small or even negative effects and others resulting in sizable gains. We further test the robustness of this pattern of results by omitting estimates that are outliers and those from studies published since 2010, as shown in Panels E-G of Table 5 and Figure 3. This reduces the magnitude of the pooled effect size estimates among small-scale studies, but the overall pattern of declines at scale remains unchanged.

Evidence from large-scale non-experimental studies. Meta-analytic reviews of the literature on tutoring frequently restrict their focus to studies that employ RCTs in an effort to ensure researchers are identifying the unbiased, causal effect of tutoring. This restriction strengthens the internal validity of the pooled effect sizes, but may also limit the external validity of these findings (Tipton & Olsen, 2018). Large-scale RCTs are expensive and often require the active consent of participants, making them financially and logistically challenging to conduct. Our meta-analytic sample contains only nine studies that evaluate programs serving at least 1,000 students. This sparse data makes it difficult to accurately project plausible effects from tutoring programs taken to scale in larger U.S. school districts given a lack of common support.

We attempt to further inform our understanding of the plausible effects of tutoring by turning to studies of large-scale programs (n treated ≥ 1,000) that employ quasi-experimental methods. Much of the literature evaluating large-scale programs focuses on after-school tutoring provided by private tutoring organizations and funded by two federal initiatives, 21st Century Learning Centers and Supplemental Educational Services (SES) under the No Child Left Behind Act. Studies of these initiatives often evaluate programs across large districts and entire states with thousands of treated students and find effects that are notably smaller than those we find with our full meta-analytic sample (Deke et al., 2012; Heinrich et al., 2010, 2014; James-Burdumy et al., 2005; Ross et al., 2008; Springer et al., 2014; Zimmer et al., 2009, 2010). These small to medium effects (frequently ≤ 0.10 SD) may be fully explained by poor attendance at these off-site afterschool programs and their design features such as large student-tutor ratios and rotating tutors. However, the scale of the programs may also have contributed to their underwhelming results by influencing program design choices and implementation quality (see Kraft & Falken, 2020 for a fuller discussion).

Three recent studies from the post-COVID era provide more relevant assessments of ongoing attempts to integrate tutoring in the U.S. public school system at scale. Carbonari, Dewey, et al. (2024) evaluate the efforts of four mid- to large-sized districts to support students' academic recovery in math during the 2021-22 academic year by providing tutoring and additional instructional time. Using a value-added framework, they find estimates that are uniformly smaller than 0.04 SD and often precisely estimated null effects. These small impacts should be interpreted in the context of the first year of these large-scale initiatives when student attendance and staffing remained critical challenges for most districts. However, this same team of researchers have expanded their analyses to evaluate the effect of tutoring and small-group instruction across eight districts during the 2022-23 academic year and found similar results (Carbonari, DeArmond, et al., 2024). They document statistically insignificant estimates of the average effects of tutoring and small-group instruction of 0.03 SD in math and 0.07 SD in reading when pooling across tutoring programs that jointly served over 12,000 students.

Kraft et al. (2024) study efforts to scale tutoring in Metro-Nashville Public Schools (MNPS) over the course of two and half years to serve over 4,000 students by the spring of 2023. In contrast to the districts studied by Carbonari and colleagues, MNPS was largely successful at engaging students to attend tutoring frequently and staffing their program at scale by hiring their own teachers as tutors. Using an event study design, they find medium effects of tutoring on standardized tests scores in reading (0.09 SD), but no effects on test scores in math, on average.

Two recent evaluations of public tutoring programs implemented across the United Kingdom (U.K.) and in Victoria, Australia also provide early evidence of post-pandemic tutoring impacts at large scales in high-income contexts. Both analyses used matching methods that included baseline tests scores to reweight regression analyses, comparing the test score gains of tutored students to comparison-group students in the third year of these tutoring programs. Government Social Research, an evaluation agency within the U.K. Civil Service, found small to medium effects of the U.K. National Tutoring Programme on math (0.06 SD) and English achievement (0.03 SD) among Key Stage 2 students (years 3-6), but no effects on the achievement of Key Stage 4 students (years 10-11) in either subject (math -0.01 SD; English -0.00 SD) (Moore et al., 2024). The Victorian Auditor-General's Office found no significant effects of the state-wide Tutor Learning Initiative on students' achievement gains in math and reading across students in years 3 through 10 (Victorian Auditor-General's Office, 2024). Together, these quasi-experimental studies of large-scale tutoring programs are consistent with the overall pattern of declining tutoring impacts as program size increases.

Why do tutoring effects decline at scale?

The phenomenon of interventions becoming less effective when they are delivered to more students is a well-documented pattern in education research (Cheung & Slavin, 2016; Kraft, 2020, 2023). Understanding why this pattern exists for tutoring programs is critical to informing efforts to expand access to tutoring and maintain its effectiveness at scale. We posit and test four primary hypotheses that might explain this pattern.

Hypothesis #1: Declining effects do not reflect a true phenomenon but are instead due to selective reporting, standardization techniques, and/or spillover. It is possible that the negative relationship between program effects and program size is a product of the research process rather than a real pattern of differential effects. First, such a pattern could be caused by selective reporting that is more acute among studies with smaller samples. Here we define selective reporting as the phenomenon where studies that produce statistically insignificant results are less likely to result in academic publications. This could occur through multiple mechanisms including researchers being less likely to write papers when they find null results, researchers making subjective modeling decisions that push preferred estimates over traditional significance thresholds (i.e., *p*-hacking), and journals being less likely to publish studies that find null results (i.e., publication bias). Of course, researchers could also be systematically designing studies of programs that are likely to have larger effects to also have smaller sample sizes given less statistical power is necessary to detect larger effects.

We explore potential bias in three ways given that no single test can definitely rule out publication bias (McShane et al., 2016). First, we produce funnel plots and conduct a trim and fill analysis (Duval & Tweedie, 2000) to assess the degree of symmetry of our point estimates around the meta-analytic mean. An imbalance in publications falling on either side of vertical line at the center of the full plot would suggest potential bias, and lead the studies being imputed to make the data more symmetric. We do this at both the individual effect-size level and at the study level by collapsing multiple effects sizes to account for the nested nature of the data. As shown in Figure 4 and Appendix Table B1, we find no evidence of publication bias in our full sample of studies using this method. We then repeat these analyses after sub-setting our data into studies with fewer than 100 treated students versus at least 100 treated students and find no evidence of differential publication bias among small-sample studies.

Second, we test for evidence of *p*-hacking bias by plotting the *p*-values from our sample of effect sizes and examine whether there is an excess mass of *p*-values just below conventional significance thresholds in these distributions, following the intuition of Brodeur et al. (2020).[5] A visual inspection of Figure 5 reveals that the distribution of *p*-values is smooth across critical values for traditional significance thresholds in the full sample and in subsamples of smaller and larger sample studies. We then formally test for differential bunching below each conventional statistical threshold using a randomization test. This approach examines whether within a given

[5] We conduct these analyses using *p*-values rather than test statistics (i.e. *Z*-scores) because of the many small-sample studies in our review. This allows us to look at a sharp discontinuity for significance which would not be possible using the test statistics where threshold values change relative to the sample size.

window around a cut-point the *p*-values are binomial-distributed with equal probability. In Table 6, we show that we find little evidence to suggest there is differential bunching of estimates with *p*-values just below the 0.05 and 0.10 significance thresholds, nor any evidence that *p*-hacking is more common among small-scale studies. Only one of the six tests we run in our full sample using three different bandwidths for each threshold is marginally significant.

Our final test of selective reporting is to compare pooled effect sizes between peer-reviewed publications and other types of studies, such as working papers or reports, that have not gone through the peer-review process. If selective reporting was occurring because journals have been less likely to publish non-significant findings, we would expect to see larger average estimates from peer-reviewed than non-peer-reviewed studies. In Table B2, we show that this is indeed the pattern we find. Specifically, we observe an average pooled effect from studies in peer-reviewed journals of 0.45 SD versus 0.22 SD for non-peer-reviewed reports. We also check for differences by publication type across subsamples of studies with different sample sizes. For both peer-reviewed and non-peer-reviewed studies, we observe smaller effects for larger-sample studies. However, for studies with at least 1,000 treated students, the effects are quite a bit smaller for non-peer-reviewed papers (0.09 SD) than for peer-reviewed studies (0.33 SD).

We interpret these results with caution, especially given peer-reviewed status is correlated with other factors such as publication date. Our sample of non-peer-reviewed works skews more recent, and we know that more recent studies have demonstrated smaller pooled effects. These results are therefore not proof positive of selective reporting but are consistent with that possibility. In sum, we find mixed evidence on whether selective reporting could explain the pattern of declining pooled effects for programs implemented at a greater scale.

Publication bias is an area of active ongoing research and new methods for testing and addressing bias with clustered data may help us shed more light on this issue in the future.

A second possible statistical explanation for the differential pattern of effect sizes across smaller and larger tutoring programs is due to the standardization process. Tutoring programs typically target students that fall within a specific range of the performance distribution. We find that 94% of the studies we include in our meta-analysis describe some type of efforts to target students, with 89% of studies evaluating programs that specifically targeted low-performing students. As Fitzgerald and Tipton (2024) document, this targeting results in samples recruited to participate in RCTs to be more homogenous than the population as a whole. Targeted sampling reduces the variation in achievement among the study sample, artificially inflating the magnitude of the effect sizes when researchers standardize their outcome measure using sample-based estimates of its standard deviation. It is possible, if not likely, that the overall effect sizes from meta-analyses of tutoring are somewhat inflated because of this practice. This may also help to explain the pattern of attenuated effects we find if smaller-scale tutoring programs are able to more precisely target students, resulting in even more homogenous participant populations compared to larger-scale programs. Said another way, the pattern of declining effects by program size might be less pronounced if all studies had used an estimate of the SD of their test score outcome derived from nationally representative populations.

Finally, the presence of peer spillover effects could contribute to a differential pattern of tutoring effects by program size. A large body of evidence documents peer effects in K-12 education settings (Barrios-Fernandez, 2023). If being in the same class or school as a student receiving tutoring has positive spillover effects on non-tutored students *and* the magnitude of these effects increases with the concentration of treated students in a class or school, then larger-scale tutoring programs could differentially attenuate the treatment-control contrast and contribute to the pattern of declining effects we find. However, it is not obvious that this would happen in practice given that the concentration of treated students per class or school could be similar across smaller and larger programs if larger programs simply serve more schools.

Hypothesis #2: Scaling causes programs to systematically alter key design features. A second potential explanation for declining effects with scale is that leaders systematically change the design of tutoring programs for larger versus smaller scale interventions. To assess the evidence for this hypothesis, we first explore how key program features, which past research has highlighted as important elements of effective tutoring programs, change as programs are taken to scale. Table 7 reveals two systematic differences in program design features when comparing smaller verses larger programs. First, larger programs are substantially less likely to tutor students individually. Programs serving over 400 students are roughly 10 percentage points less likely to rely on 1:1 student-tutor ratios than small programs that serve fewer than 100 students. Second, larger programs tend to deliver less dosage, primarily by shortening the number of weeks tutoring programs run. Here the relationship is not entirely monotonic, with the smallest tutoring programs offering moderate dosage, middle-sized tutoring programs with the highest total dosage and larger tutoring programs offering the least. For example, on average, programs that serve 100 to 399 students delivered 39 total tutoring hours while those serving greater than 1,000 delivered 27 total hours. Unexpectedly, we see that larger programs are even slightly more likely to use teachers and paraprofessionals as tutors and to provide a high degree of supervision and support to tutors – characteristics hypothesized to promote larger effects.

As another test, we examine whether the negative relationship between program effects and size is attenuated when we control for the full range of observable program characteristics in a meta-regression framework. We do this by comparing the results of two meta-regressions. The first model shown in Table 8 reports coefficients from binned sample size indicators which capture the clear negative relationship relative to the omitted category of studies evaluating small programs serving fewer than 100 students. We then add a large set of control variables including indicators for the intersection of schooling-level and subject, researcher generated assessments, studies from international contexts, publication decade, and our full set of tutoring program characteristics. We find that the strength of the negative relationship between program size and effects is reduced by roughly 30%, suggesting changing program characteristics do account for a portion of the overall negative relationship between program size and effects on achievement.

Notably, only a few study features and program characteristics appear systematically related to effect sizes when included in our fully controlled meta-analytic model. In addition to sample size, researcher-generated tests produce meaningfully larger effect sizes relative to third-party standardized tests (0.22 SD), tutoring outside of the school day has a strong negative association with effect sizes relative to during the school day (-0.19 SD), and an indicator for studies that did not report a specific tutor-student ratio – perhaps suggestive of larger and flexible ratios – also has a strong negative association compared to those with a 1:1 ratio (-0.19 SD). We similarly find a negative association with using a specified tutor type not in our major categories, relative to a teacher (-0.16 SD), although this group mostly consists of community members and/or volunteers. While we find significant positive associations with two bins of total dosage hours relative to receiving at least 60 hours of treatment, these results don't show a monotonic relationship between dosage and impacts, suggesting more systematic exploration is needed.

Hypothesis #3: Heterogeneous tutoring effects cause the marginal student to benefit less as tutoring programs expand. The attenuation of tutoring effects as program sizes increase may also be a product of the heterogeneous effects of tutoring across students. Prior research has found that tutoring may be more effective for students who are lower-performing prior to tutoring (Kraft, 2015; Robinson et al., 2024), Black students (Fryer & Howard-Noveck, 2020) and students from low-income families (Carlana & La Ferrara, 2024). It is plausible that smaller-scale tutoring programs appear more effective because they are better able to target students who stand to benefit the most, on average, from the programming. As tutoring programs scale, they may be expanding to serve students who will benefit less, on average.

We explore this by comparing weighted averages of student characteristics in our sample of RCTs, disaggregating by the size of the tutoring program, in Table 9. This comparison reveals a clear pattern where studies of smaller tutoring programs serve larger percentages of historically marginalized students. Students in smaller tutoring programs were 13 percentage points more likely to be English learners, 11 percentage points more likely to be receiving special education services, 8 percentage points more likely to be from low-income backgrounds, 4 percentage points more likely to be Hispanic, and 2 percentage points more likely to be Native American. These sizable differences in the characteristics of students served by smaller and larger tutoring programs are likely to attenuate the estimated effects of tutoring as program scale increases.

A related possibility that we cannot directly test with our data is that smaller programs treat student populations that are more homogenous. Homogeneity may make implementation easier because there is less of a need to tailor interventions to a variety of student achievement levels or other unique needs. Expanding tutoring programs might mean programming is provided to a more diverse group of students with a wider set of challenges, making it more difficult to produce large impacts among increasingly heterogenous groups.

Hypothesis #4: Implementation quality declines as tutoring programs scale. A final hypothesis for why we observe smaller impacts for larger programs is that the quality of program implementation declines as tutoring programs are brought to scale. Imagine, for example, two tutoring programs with the exact same intended program design features (e.g., high-dosage, 1:1 ratios, paraprofessional tutors), but one serves a small number of students at a single school and the other is brought to scale districtwide. Administrative needs are likely higher for the large-scale program. Small programs may be more likely to represent pilot efforts led by uniquely trail-blazing, motivated, and talented leaders, whereas administrators recruited to run large programs may not be as effective, on average. Implementation quality could also suffer if the effectiveness of the average tutor is lower for larger programs than for smaller programs. However, if tutoring screening tools are only weakly related to tutor performance, then tutor quality may not decline with scale (Davis et al., 2017). It may be more challenging to coordinate communication between tutors and teachers for large-scale programs. There may simply be less oversight with a greater number of tutoring sites, making it more difficult to ensure fidelity of implementation to program models for larger interventions. It may be that even if the intended program design remains constant as programs expand, the delivered dosage drops if student attendance suffers or time-on-task declines as programs expand. This would still be consistent with our pattern of results, given that our coding reflects intended measures of dosage rather than the actual number of total hours of tutoring treated students received.

Unfortunately, most tutoring RCTs do not directly measure implementation quality, limiting what we can say about this hypothesis using our meta-analytic dataset. However, survey data and several recent studies on post-COVID tutoring efforts do point to significant implementation challenges. To start, the majority of K-12 public school principals report experiencing barriers (e.g., funding, timing, or staffing challenges) that limited their ability to effectively provide tutoring on the nationally representative SPP survey (National Center for Education Statistics, 2024). The aforementioned "Road to Recovery" (R2R) evaluation of large-scale academic recovery efforts by Carbonari, Dewey, et al. (2024) documents how districts fell well short of leaders' intended expectations with regard to both the number of students served and the intensity of the interventions. Interviews with district leaders revealed challenges with engaging targeted students. This is consistent with SPP survey results showing that, among schools that provided tutoring, larger schools had somewhat lower student participation rates (National Center for Education Statistics, 2024). Research on an opt-in virtual tutoring program in the spring of 2021 further illustrates challenges related to low student participation, especially among struggling students who might benefit most from tutoring (Robinson et al., 2022).

Buy-in is also a problem identified amongst staff. Programs that appeared to successfully scale high-quality tutoring after the pandemic emphasized the importance of district-level leadership, goal setting, buy-in from school leaders and teachers, a willingness to rethink scheduling, the pursuit of multiple funding sources, and the ability to make difficult choices about spending trade-offs (Cohen, 2024). Leaders in the R2R districts highlighted staffing challenges related to pandemic surges, a tight labor market, and limited district capacity for recruitment and human resources management. These issues of staffing challenges and organizational capacity are echoed by findings from a qualitative study on programs in two urban districts (Makori et al., 2024). These implementation challenges do not appear to be solely a function of acute post-pandemic conditions, as the R2R team's follow up report examining academic recovery efforts during 2022-23 revealed that difficulties persisted (Carbonari, DeArmond, et al., 2024). Across the majority of interventions examined by the R2R study, including tutoring, fewer students participated and for less time than intended. Consistent with our meta-analytic findings, the rare R2R tutoring programs that generated positive impacts on test scores were those implemented on a small scale. Finally, leaders interviewed for the R2R report also pointed to their need to adapt tutoring program designs—sometimes departing from best practices—to align with federal, state, and local policies. This is likely to remain a challenge as schools and districts look to a range of federal, state, and local funding sources to support tutoring programs after the COVID-relief funding runs dry (Accelerate, 2023; Cohen, 2024).

How might policymakers approach the challenge of maintaining effectiveness at scale?

Like so many complex interventions, the efficacy of tutoring programs may lie in the combination of program design features rather than any single characteristic. Prior literature has focused on a bundle of program features that research suggests are associated with larger effects, aligned with what is sometimes described as "high-quality," "high-dosage," or "high-impact" programs (e.g., Robinson et al., 2021). This bundle of features includes in-person programming, delivered at school during school hours, with a student-tutor ratio of no more than 3:1, meeting at least 3 times per week, ensuring a high overall dosage (which we proxy for with at least 15 hours of total tutoring), and using a provided curriculum.[6]

When we test whether the combination of these features are greater than the sum of their parts, we find encouraging results. Specifically, the overall pattern of declining effect sizes persists among tutoring programs that utilize a bundled package of recommended design features but the attenuation at scale is less pronounced. When we isolate only individual features of this bundle, effects continue to erode to varying degrees as programs scale (Appendix Table B3).

[6] Sustained relationships between tutors and students and data-informed instruction based on formative assessments are also recommended. Unfortunately, most studies did not provide information to allow us to code these features.

That said, this erosion is milder for in-person programs providing at least 15 hours of tutoring. As shown in Figure 6, while the pooled effect among studies of programs serving between 100-399 students declines by 42% relative to programs serving 99 students or fewer in the full sample, it only declines by 18% in the restricted sample of studies with the bundled package of design features. The decline among programs serving 400-999 students is also slightly less pronounced, dropping 54% in the full sample and 44% in the bundled package sample. Perhaps most striking is that when we restrict our analysis to studies evaluating U.S. programs based on standardized test measures published after 2009 and omit outliers, we see no attenuation of program effects across sample size, at least for programs serving fewer than 1,000 students.[7]

What does the research suggest about modifying program design features to reduce costs and increase scalability?

Although the bundled package of program features appears to help sustain program effectiveness at scale, several aspects are costly and can be difficult to implement at scale. Here we explore the potential implications of modifying specific program features.

Moving Tutoring Online: Many districts and programs have adopted online tutoring to access a larger potential supply of tutors. How might this affect the efficacy of tutoring? When we limit our sample to the 59 estimates of tutoring delivered virtually (drawn from 6 unique studies), the pooled estimate reported in Table 11a is 0.07 SD.[8] This is substantially smaller than the unadjusted pooled estimate of in-person program impacts of 0.44 SD and even substantially smaller than our preferred pooled estimates of expected impacts for our target of inference, 0.16

[7] We are unable to estimate this number for programs serving more than 1,000 students because only one of the larger scale programs evaluated in our sample implemented the full bundle of recommended program characteristics.
[8] We observe the following virtual tutoring studies: Fesler et al. (2023), Gortazar et al. (2023), Kraft et al. (2022), Loeb et al. (2023), Roschelle et al. (2020), and Torgerson et al. (2016). We exclude Carlana and La Ferrara (2021) because their achievement outcome pools across math, Italian, and English.

to 0.21 SD (Table 5). However, results from our meta-analytic regressions presented in Table 8 suggest these smaller effects are likely driven by other program features. Conditional on our extensive set of codes for observable program features, we estimate a positive but statistically insignificant coefficient when comparing virtual tutoring programs to those in person.

Unfortunately, there are no studies to our knowledge that provide a direct causal comparison between virtual and in-person programs, which would be a major contribution to the field. Another limitation of the literature is that most studies of virtual tutoring in our sample were conducted in the post-COVID era, and some during the early part of the pandemic when challenges were most acute. These studies may not generalize well to non-pandemic times or to contexts that differ in important ways from those where leaders were willing to partner with researchers amid pandemic recovery. Two new studies of virtual tutoring released in 2024 find estimates that are similar to (Ready et al., 2024) or somewhat larger than (Carlana & La Ferrara, 2024) the magnitude of our pooled estimates for virtual programs. We read this evidence as suggestive that online tutoring has the potential to be an effective approach to addressing scaling challenges when accompanied by other effective program design characteristics.

Increasing Student-Tutor Ratios: The cost of tutoring is driven largely by tutor compensation. Many districts and tutoring organizations have chosen to increase student-tutor ratios as a means of expanding access while managing costs. In Table 11a, we report pooled effect estimates by student-tutor ratio and find somewhat larger impacts, on average, for programs with lower ratios. Using the full sample, we estimate pooled effects of 0.43, 0.41, 0.30, and 0.34 SD for 1:1, 2:1, 3:1, and 4:1 programs, respectively. The effects for programs with 5 or more students per tutor are substantially larger with the full sample (0.91 SD), but this result is not robust to excluding studies outside of our target of inference. The overall pattern of declining effects persists when we focus on U.S. tutoring programs evaluated using standardized tests and when we further restrict to those studies for which we have more confidence in the quality of research methods. Importantly, even programs with ratios of 5 or more generate meaningful impacts, on average, between 0.24 SD and 0.29 SD. When we examine student-tutor ratios in a meta-regression framework, we find a pattern of larger effects for smaller student-tutor ratios, although none of the point estimates are statistically significant.

Evidence from the ten studies that experimentally vary student-tutor ratios, summarized in Table 12a, provide a range of contrasts from 1:1 versus 2:1 ratios (Carlana & La Ferrara, 2024; Loeb et al., 2023; Vadasy & Sanders, 2008) to 4:1 to 13:1 ratios (Vaughn et al., 2010). Most examine interventions with elementary students (Clarke et al., 2017, 2020, 2023; Doabler et al., 2019; Loeb et al., 2023; Schwartz et al., 2012; Vadasy & Sanders, 2008) except for three with middle schoolers (Carlana & La Ferrara, 2024; Kraft & Lovison, 2024; Vaughn et al., 2010). The effect size differences most often favor smaller ratios but are not always large in magnitude and do not typically achieve statistical significance. However, many of these studies are underpowered to detect small differences in effects between treatment arms. In short, the existing research suggests that lower ratios produce larger effects, but it is possible to deliver tutoring in pairs or small groups and maintain meaningful effects.

Using Peer Tutors: An alternative approach to scaling tutoring on a fixed budget is to enlist K-12 students as peer tutors. We find that pooled effect sizes for peer tutoring in our full sample are an impressive 0.36 SD, shown in Table 11b. Our meta-analytic regression (Table 8) suggests peer tutoring is equally as effective as tutoring by teachers, conditional on other program and study characteristics, with a non-significant difference of just 0.06 SD in favor of teachers relative to peer tutors. We know of only one study that randomizes students to different tutor types. Mathes et al., (2003) uses a partially matched and partially randomized design to compare teachers who implemented small group (4-5:1) instruction verses overseeing pairs of students who used Peer Assisted Learning Strategies (PALS). They find effect sizes of 0.70 SD for teacher directed small-group instruction and 0.55 SD for peer-assisted instruction. We see the limited evidence for scaling with peer tutoring as encouraging but incomplete.

Decreasing Dosage: A fourth common approach to scaling tutoring while controlling costs is to reduce overall dosage. Pooled effect estimates presented in Table 11b do not reveal a clear monotonic trend between dosage hours and program impacts, particularly in our more restricted samples. Across the full sample as well as the more restricted, generalizable samples, programs offering over 60 hours of tutoring consistently have the smallest impacts. In our preferred policy-relevant subsample, the greatest magnitude of effect is for programs providing 15-29 hours of tutoring (0.41 SD). However, these pooled estimates may be inflated or deflated if other study characteristics are correlated with certain amounts of dosage. When employing meta-regression to control for a variety of program features and study characteristics, we do not find consistent statistically significant differences based on the total hours of dosage, as shown in Table 8. If anything, the pattern of results is counter-intuitive with programs offering higher dosage showing smaller effects. Here selection bias still poses a potential challenge if, for example, more intensive programs target particularly struggling students.

Evidence from four studies that randomly assigned students to different doses of tutoring to isolate the causal impact of dosage suggests some benefits of greater dosage. As shown in Table 12b, three of these studies evaluate elementary school programs (Al Otaiba et al., 2005; Begeny, 2011; Wanzek & Vaughn, 2008) and one middle school program (Carlana & La Ferrara, 2021). These studies provide a range of contrasts, for example, comparing four versus nine total hours of tutoring (Begeny, 2011) to comparing 36 hours versus 72 hours (Al Otaiba et al., 2005). More often than not, these studies show greater effect sizes for programs providing higher than lower dosages. In short, studies that experimentally vary dosage suggest that decreasing dosage may attenuate effects. However, we still have much to learn about dosage effects related to days per week, hours per session, hours per week, total weeks, and whether the ideal design varies for different age groups, subjects, or by other factors.

Discussion

Evidence-based policymaking has increasingly become the standard in education, particularly as practitioners look to implement proven approaches to accelerate students' academic growth after the substantial disruptions caused by the COVID-19 pandemic. While this trend is encouraging, it places increased importance on the external validity of research. Even well designed and implemented RCTs – the gold standard approach to causal inference – offer incomplete information to policymakers and practitioners if the evidence they produce is at arm's length from the realities of implementing education policies and practice at scale. Meta-analyses that pool evidence across multiple studies seemingly offer evidence with strong external validity, but aggregating across multiple studies with limited generalizability does not make the results valid for a very different target of inference.

Our study illustrates the importance of carefully considering the alignment between the research evidence and the policy target of inference. We find that attempts to better harmonize our meta-analytic sample of 265 studies to the target of inference used by most policymakers – large-scale tutoring programs in the U.S. aiming to increase student performance on standardized tests – substantially reduces the pooled effect sizes we find. This attenuation is largely driven by the declining impacts of tutoring programs as they scale – a pattern that is common across the education research literature.

This pattern of declining effects at scale often leads to a circular argument that, “the program works when implemented with fidelity, it just wasn’t implemented correctly when taken to scale.” Alternatively, one might ask, “If implementation becomes systematically more difficult at scale, then does a program really work?” We see four possible responses to this challenge: 1) start small, learn, iterate, and engage in the hard but critical work to scale vertically (i.e. expanding program size) over time while maintaining program fidelity, 2) redesign the program to be easier to implement at scale, 3) adopt a more flexible approach to scaling that allows for localized adaptation, and/or 4) decide that a program is best delivered in a small-scale format and focus on horizontal scaling (replicating small programs).

To be clear, we view the more target-equivalent estimates of the effects of tutoring we find as still meaningful and policy-relevant (Kraft, 2020). We continue to see tutoring as one of the most promising evidence-based approaches to accelerating student achievement. If districts could leverage tutoring at scale for those students whose learning was most negatively affected by the pandemic and produce effects similar to our policy-relevant estimates, it would be a huge success. In fact, several recent experimental studies of tutoring programs implemented post-COVID at a medium scale (Carlana & La Ferrara, 2024; Cortes et al., 2024; Gortazar et al., 2024) and at a large scale (Robinson et al., 2024) find effects on par with those from our target-aligned pooled effect sizes.

That said, we also think it is equally important for policymakers and practitioners alike to have more grounded expectations about what tutoring can accomplish. Several other recent studies using both experimental and quasi-experimental methods suggest early attempts to scale tutoring in the U.S. have produced quite small effects (Carbonari, DeArmond, et al., 2024; Carbonari, Dewey, et al., 2024; Kraft et al., 2024; Ready et al., 2024). Outsized expectations can lead policymakers and practitioners to become disillusioned when they fail to realize the eye-popping effect sizes of small-scale, boutique tutoring programs implemented under favorable circumstances among students who often opt into participating, particularly when meta-analytic estimates mask those contextual factors. Unrealistic expectations can also lead policymakers to mistakenly rely on a single or limited set of interventions when multiple interrelated programs may be needed to achieve their goals. Contextualizing tutoring program effects relative to their costs will also be critical for identifying sustainable models (Kohlmoos & Steinberg, 2024).

New technology may also present opportunities to scale tutoring with greater fidelity while maintaining program effects and reducing per-pupil costs. Recent studies suggest that computer-assisted learning programs paired with tutoring (Bhatt et al., 2024) or integrated into core academic classes (Oreopoulos et al., 2024) can support effective instruction, potentially reducing common obstacles to scaling tutoring. There is growing interest in the potential of generative artificial intelligence to offer effective tutoring at scale, although early programs appear to fall well short of this goal (Barnum, 2024). We remain optimistic about the potential of these new technologies but emphasize that the benefits of human tutoring likely extend far beyond student performance on standardized tests, to say nothing about the value of tutoring for the tutor. Human tutoring offers the opportunity for authentic personal connections and social interactions that can contribute to student development; it also creates volunteer and employment opportunities and valuable experiences for those interested in pursuing a career in education.

Conclusion

Efforts to integrate tutoring at scale into the U.S. K-12 public education system are at a critical juncture. New evidence documenting the mixed results of early efforts to expand access to tutoring during the 2021-22 and 2022-23 academic years is emerging right as large-scale federal funding to support tutoring is ending. With this paper, we aim to inform ongoing efforts to refine tutoring programs when implemented at scale and better calibrate expectations for what these programs are capable of accomplishing. Our findings highlight the importance of conducting research that considers both internal and external validity to best inform policy and practice.

Our analyses suggest that a bundled package of program features hypothesized to promote effective tutoring does guard against some of the attenuation that occurs as programs expand. It remains an open question whether adapting individual features of this bundle – such as moving tutoring online, increasing student-tutor ratios, using peer tutors, or decreasing dosage – can be done without compromising effectiveness. Such changes may attenuate effects but still be an equally, if not more, cost-effective way to deliver tutoring at scale. Our hope is that as policymakers experiment with new tutoring models, they will partner with researchers to learn about the impacts of these adaptations. Continued efforts to integrate individualized instruction into the U.S. K-12 education system would benefit from a decades-long approach that focuses first on establishing effectiveness and then on scaling, rather than the other way around.

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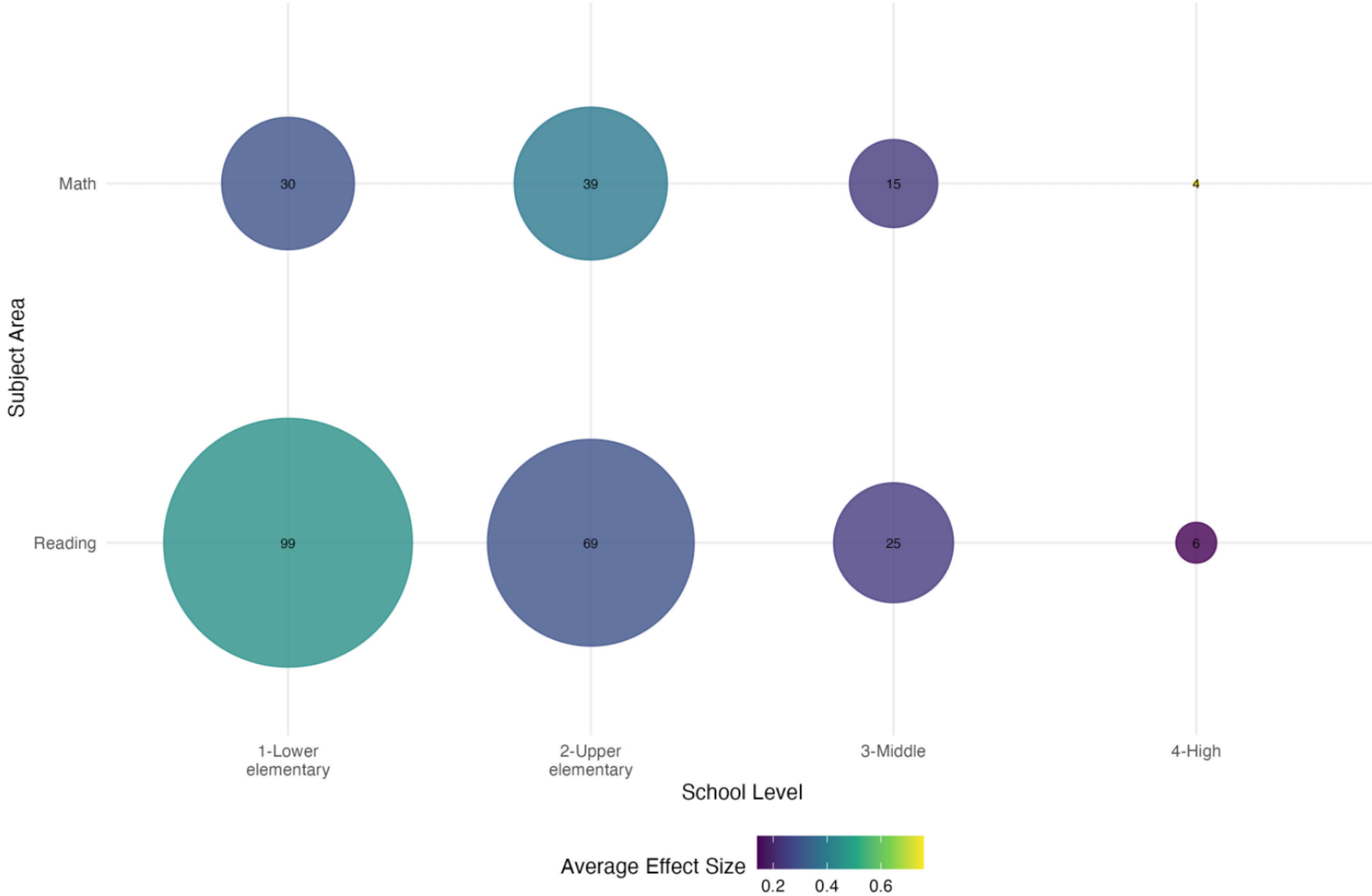
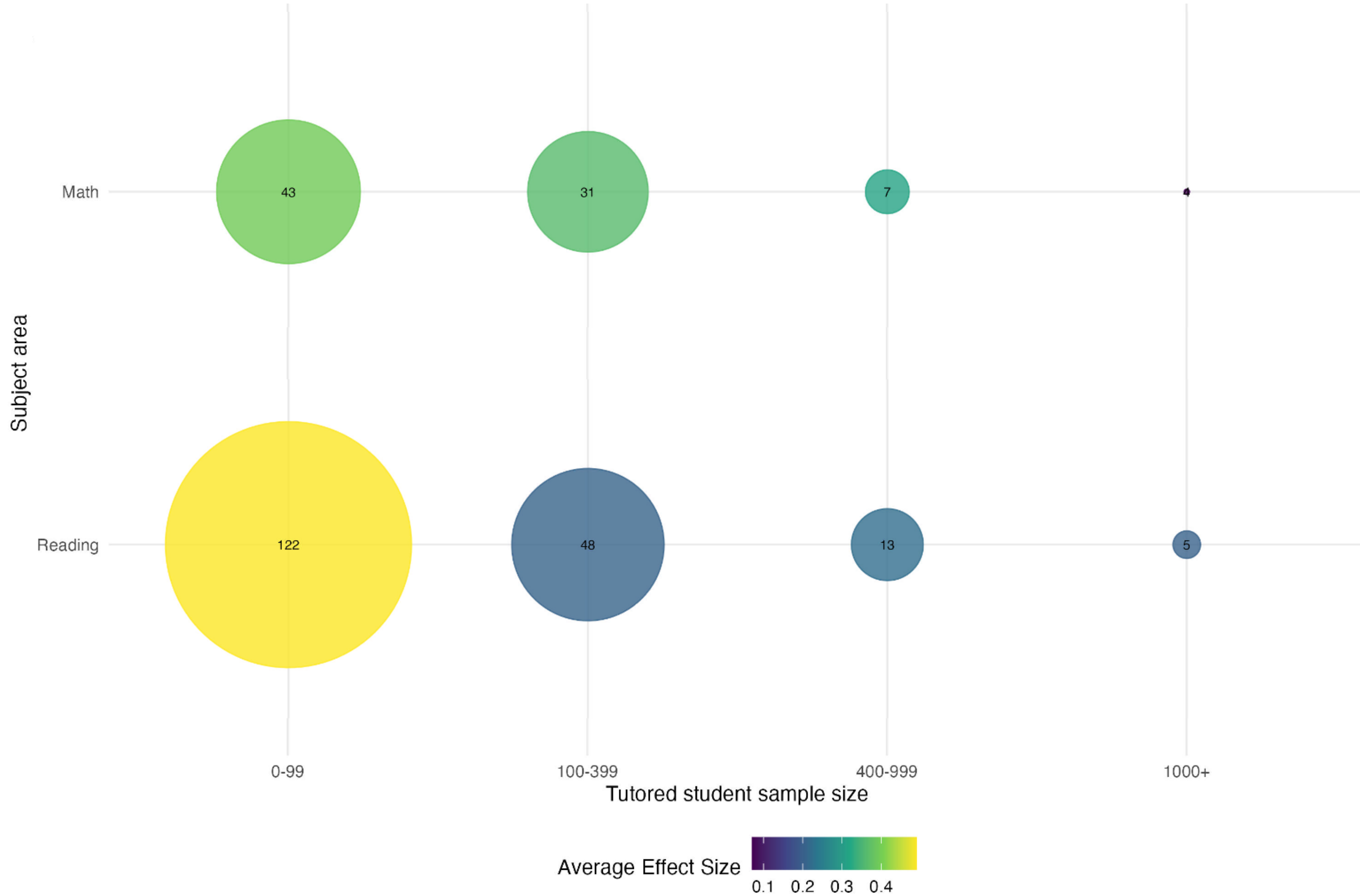
Figures and Tables

Figure 1. Evidence gap map by school level and subject area



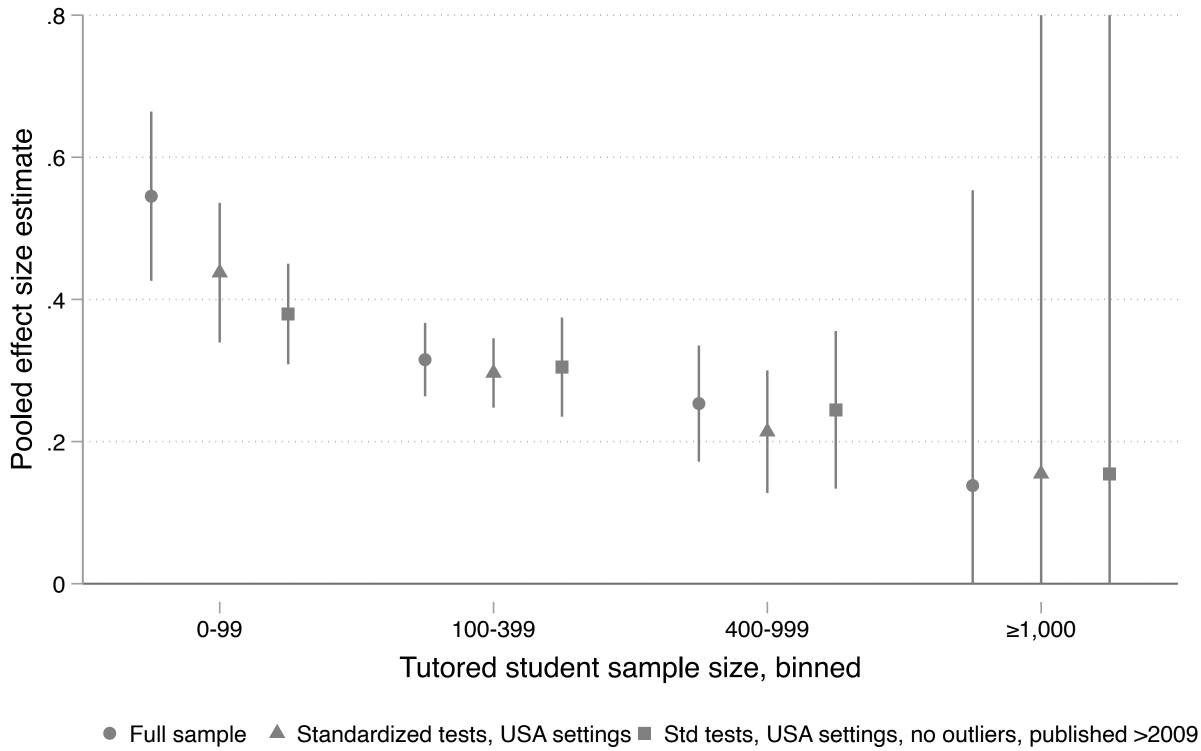
Notes: Each circle is scaled to illustrate the number of unique studies with estimates in that cross section of features. The color of each circle presents the average pooled effect size amongst those estimates. Circles are all labeled with the number of studies they represent. Note that for this figure we have established mutually exclusive categories for school level, where we round up to the higher school level observed if studies treat students are more than one level.

Figure 2. Evidence gap map by tutored student sample size and subject area



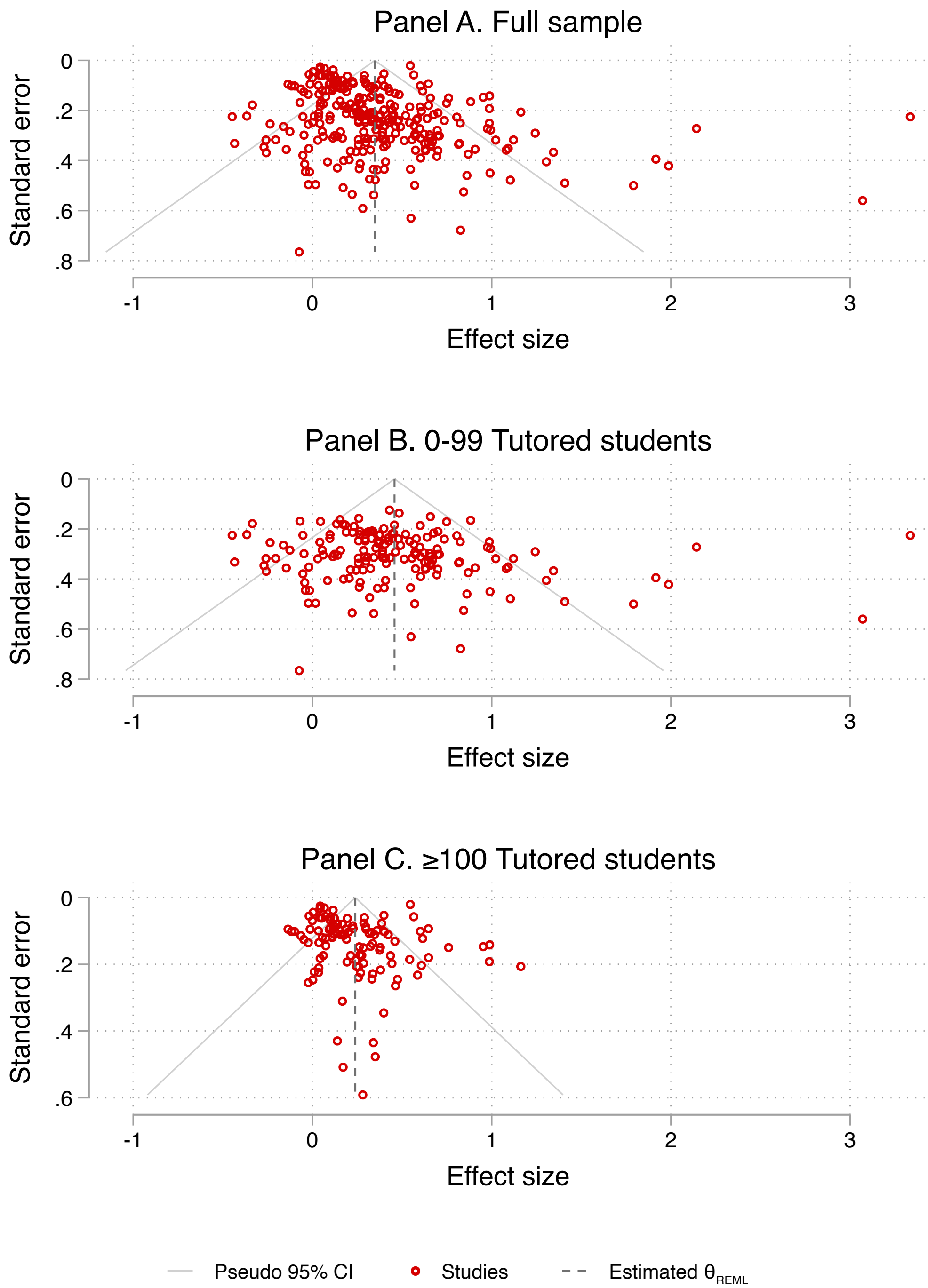
Notes: Each circle is scaled to illustrate the number of unique studies with estimates in that cross section of features. The color of each circle presents the average pooled effect size amongst those estimates. Circles are all labeled with the number of studies they represent.

Figure 3. Pooled estimated impacts of tutoring across program size and study characteristics



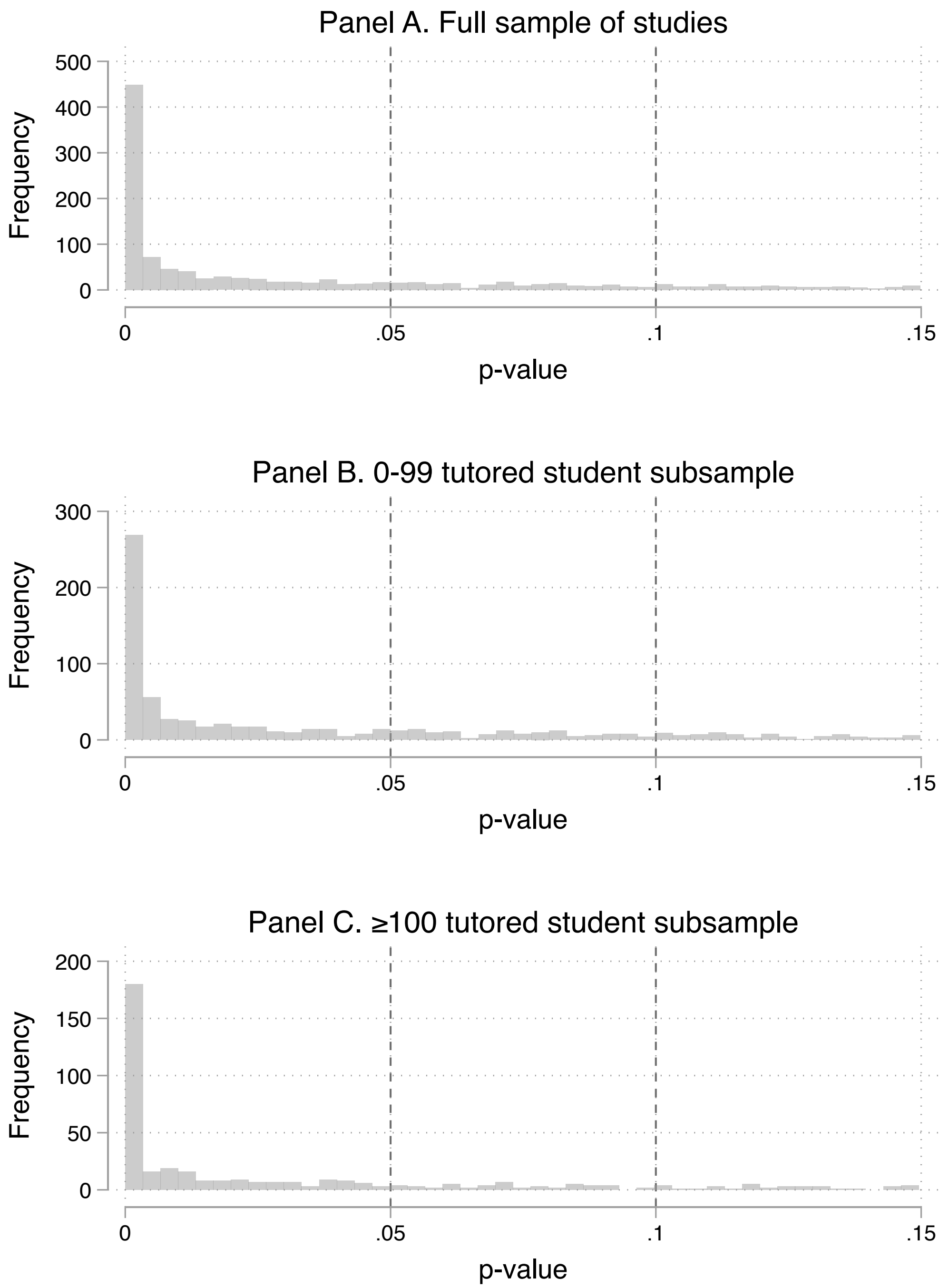
Notes. Each point represents a pooled effect size estimate for the subsample of studies triangulated by the tutored student sample size and restrictions indicated; bars represent 95% confidence intervals for these estimates. All estimates for studies with tutored student samples at or above 1,000 students are not statistically distinguishable from zero. All estimates pool impacts across math and reading.

Figure 4. Funnel plots displaying trim and fill results at the study-level



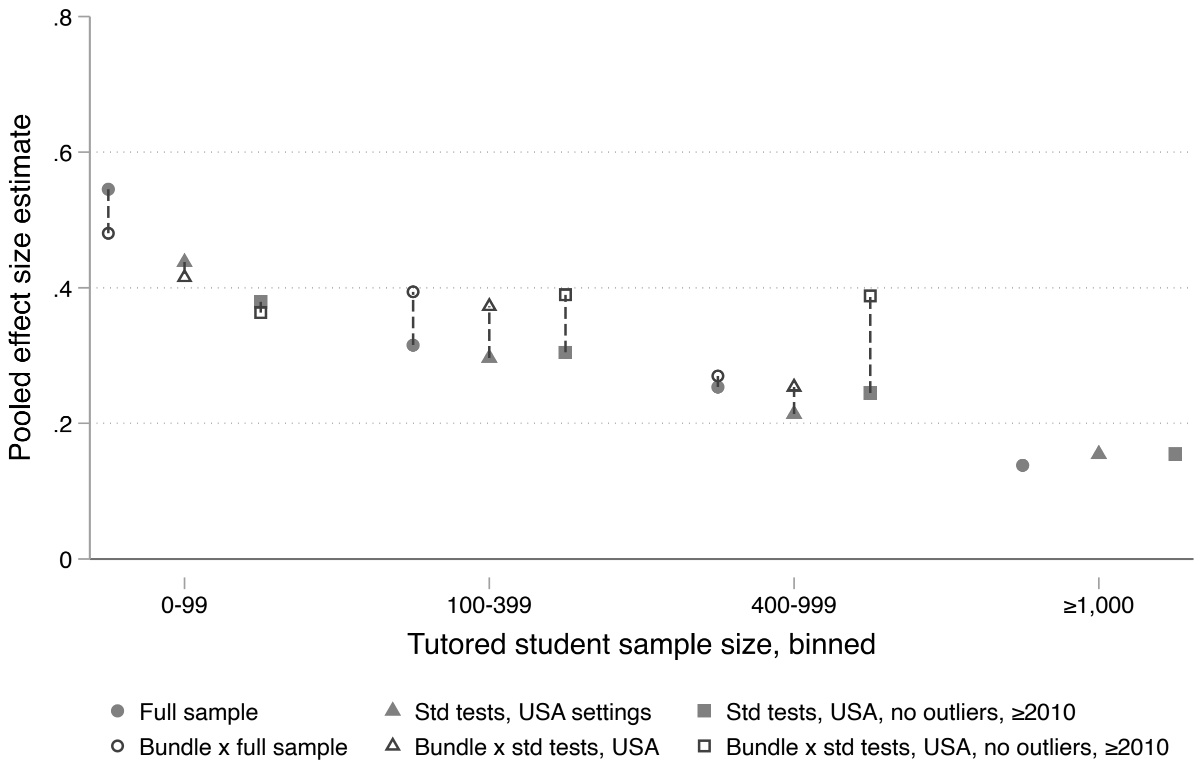
Notes: Each panel presents the average effects of each study-by-subject in the sample indicated. Panel A presents results for all studies and subjects, Panel B limits to studies of programs treating fewer than 100 students, and Panel C limits to studies of programs treating at least 100 students.

Figure 5. Density of effect estimate *p*-values across significance thresholds



Notes: Each panel presents a histogram of estimated effects' *p*-values, constrained to those <0.15, stacked across math and reading. Mass points to the left of each vertical, dashed line would suggest manipulation of results to attain significance or asymmetric reporting of results according to their significance. Panel A shows this distribution for the full sample of *p*-values below 0.15, Panels B and C disaggregate estimates according to the study's treated student sample size.

Figure 6. Gaps between pooled estimates for all studies compared to those using a bundle of tutoring best practices



Notes: Each point represents a pooled effect size estimate for the subsample of studies triangulated by the tutored student sample size and sample restrictions indicated. Dashed lines demarcate the difference between estimates using the full sample of studies indicated (solid markers) and estimates for the subsample of studies in this same group which leverage a bundle of best practices identified in the tutoring literature (hollow markers). Tutoring programs in the bundle subgroups share the following characteristics: in-person instruction, instruction at school, instruction during school, no greater than three-to-one student-to-tutor ratio, curriculum provided to tutors, at least 3 sessions per week, and at least 15 hours of total instruction dosage planned. All estimates pool across math and reading.

Table 1: Study characteristics

	Sample mean (%)	n
Publication date		
Published before 1980	3.77	10
Published in the 1980s	1.89	5
Published in the 1990s	7.92	21
Published in the 2000s	24.15	64
Published in the 2010s	50.57	134
Published in the 2020s	11.7	31
Publication type		
Peer-reviewed journal article	76.98	204
Research firm report	8.3	22
University-based research center report	1.89	5
Working paper	1.51	4
Dissertation	8.3	22
Other publication type	3.02	8
***Setting grade level**		
Lower elementary (K-2)	62.64	166
Upper elementary (3-5)	41.89	111
Middle (6-8)	12.45	33
High (9-12)	3.4	9
Setting urbanicity		
Urban setting	39.62	105
Suburban setting	4.91	13
Rural setting	4.91	13
Multiple urbanicities studied	18.49	49
Urbanicity unknown	32.08	85
Setting country		
USA	80.75	214
International / OECD Country	19.25	51
Treated student sample		
0-99 treated sample	59.25	157
100-399 treated sample	29.81	79
400-999 treated sample	7.55	20
≥1000 treated sample	3.4	9
Tutoring subject		
English as a second language	1.89	5
Math	27.55	73
Reading	64.53	171
Multiple subjects	6.04	16
N studies	265	

* Setting grade level categories are not mutually exclusive

Table 2: Intervention characteristics

	Sample mean (SD)	n
Virtual / in-person delivery		
Tutoring online	3.24	11
Tutoring in-person	96.76	329
Where tutoring happens		
Tutoring at school	85.59	291
Tutoring at home	1.18	4
Tutoring in multiple locations / other	2.65	9
Tutoring location unknown	10.59	36
When tutoring happens		
Tutoring during school	76.18	259
Tutoring after school	6.47	22
Tutoring during vacation	0.29	1
Multiple time windows / other	4.41	15
Timing unknown	12.65	43
Student-tutor ratio		
1:1 student-tutor ratio	45.88	156
2:1 student-tutor ratio	16.18	55
3:1 student-tutor ratio	13.24	45
4:1 student-tutor ratio	14.12	48
≥5:1 student-tutor ratio	7.94	27
Ratio unknown	2.65	9
Tutor type		
Tutored by teacher	17.65	60
Tutored by paraprofessional	17.06	58
Tutored by peer	9.41	32
Tutored by college / graduate student	16.18	55
Other tutor type	12.06	41
Tutor type unknown	27.65	94
***Dosage (units specified)**		
Sessions per week	3.39 (1.29)	
Hours per session	0.61 (0.38)	
Hours per week	2.01 (1.54)	
Weeks per year	16.32 (9.15)	
Hours total dosage	33.51 (31.60)	
Curriculum provided		
Yes	89.12	303
No	10.59	36
Unknown	0.29	1
N interventions	360	

* Dosage metrics are not binary variables and are not mutually exclusive. Standard deviations are reported in parentheses, where applicable. All other sets of variables are percents.

Table 3. Estimates pooled by grade level and tested subject

	Lower elementary (1)	Upper elementary (2)	Middle school (3)	High school (4)	Pooled grades (5)
Math	0.333***	0.441***	0.377***	0.550*	0.385***
	(0.036)	(0.053)	(0.075)	(0.326)	(0.032)
	[0.045, 0.620]	[0.150, 0.732]	[-0.154, 0.907]	[-0.543, 0.751]	[0.018, 0.0751]
	229	268	46	13	507
Reading	0.462***	0.480***	0.362***	0.158	0.436***
	(0.057)	(0.103)	(0.111)	(0.131)	(0.046)
	[-0.392, 1.317]	[-0.426, 1.386]	[-0.245, 0.970]	[-0.313, 0.629]	[-0.319, 1.192]
	1,263	608	127	27	1,716
Pooled subjects	0.438***	0.469***	0.360***	0.269**	0.423***
	(0.047)	(0.075)	(0.076)	(0.135)	(0.036)
	[-0.367, 1.243]	[-0.435, 1.373]	[-0.188, 0.908]	[-0.115, 0.652]	[-0.314, 1.161]
	1,492	876	173	40	2,223

Notes: *** p<0.10; ** p<0.05, * p<0.01. Prediction intervals are included for each estimate in brackets; robust standard errors are reported in parentheses. Estimates may be included in more than one group if they treat students in multiple grade levels. Lower elementary indicates treatment in grades K-2; upper elementary indicates treatment in grades 3-5; middle school indicates grades 6-8; high school indicates grades 9-12. Pooled estimates combine impact estimates for both math and reading subject tests.

Table 4. Estimates by RCT quality concerns, stacked subjects

Lower elementary (1)	Upper elementary (2)	Middle school (3)	High school (4)	Pooled grades (5)
Panel A. Studies with no RCT quality concerns				
0.437***	0.465***	0.259***	0.184**	0.415***
(0.054)	(0.081)	(0.046)	(0.090)	(0.040)
1,225	807	146	36	1,875
Panel B. Studies with an RCT quality concern				
0.436***	0.505***	1.019**		0.461***
(0.085)	(0.073)	(0.464)		(0.073)
267	69	27		348
Panel C. Omitting top and bottom 2.5% of effect size observations				
0.384***	0.376***	0.293***	0.287**	0.379***
(0.027)	(0.030)	(0.044)	(0.143)	(0.021)
1,430	815	164	39	2,113
Panel D. Studies published prior to 2000				
0.473***	0.315***	0.557***	1.198	0.449***
(0.104)	(0.115)	(0.189)	(0.779)	(0.083)
191	67	22	5	259
Panel E. Studies published between 2000 and 2009				
0.573***	0.727***	0.956**		0.584***
(0.125)	(0.233)	(0.407)		(0.103)
503	252	37		654
Panel F. Studies published between 2010 and 2019				
0.383***	0.375***	0.223***	0.178	0.376***
(0.034)	(0.046)	(0.033)	(0.122)	(0.033)
681	489	96	30	1,118
Panel G. Studies published in 2020 and following				
0.233*	0.376***	0.189**	0.116***	0.272***
(0.125)	(0.091)	(0.079)	(0.004)	(0.085)
117	68	18	5	192

Notes: *** $p<0.10$; ** $p<0.05$, * $p<0.01$. Estimates may be included in more than one group if they treat students in multiple grade levels. All cells pool across both math and reading. Cells left blank contain too few or no estimates. Panel A and B split up the entire sample by whether we identified any concerns with the quality of the RCT. Panel C omits the top and bottom 2.5% of observations by effect size magnitude.

Table 5. Pooled effect size estimates overall and by treated student sample size

No sample size restriction (1)	0-99 treated students (2)	100-399 treated students (3)	400-999 treated students (4)	≥1,000 treated students (5)
Panel A. Full analytic sample with no restrictions				
0.423***	0.545***	0.315***	0.253***	0.138
(0.036)	(0.060)	(0.026)	(0.037)	(0.103)
[-0.314, 1.161]	[-0.689, 1.780]	[-0.103, 0.733]	[0.095, 0.411]	[-0.362, 0.638]
2,223	1,403	640	112	68
Panel B. Standardized tests only				
0.352***	0.459***	0.279***	0.219***	0.138
(0.028)	(0.048)	(0.024)	(0.034)	(0.106)
[-0.228, 0.932]	[-0.359, 1.277]	[-0.148, 0.706]	[0.061, 0.377]	[-0.365, 0.642]
1,810	1,086	560	97	67
Panel C. USA settings only				
0.390***	0.463***	0.340***	0.254***	0.154
(0.027)	(0.041)	(0.027)	(0.041)	(0.148)
[-0.228. 0.932]	[-0.348, 1.275]	[-0.030, 0.709]	[0.071, 0.437]	[-0.463, 0.771]
1,829	1,152	528	90	59
Panel D. Standardized tests only, USA settings only				
0.351***	0.438***	0.297***	0.214***	0.155
(0.029)	(0.049)	(0.024)	(0.037)	(0.154)
[-0.245, 0.947]	[-0.347, 1.222]	[-0.066. 0.660]	[0.055, 0.373]	[-0.468, 0.778]
1,484	895	456	75	58
Panel E. Omitting the top and bottom 2.5% of effect sizes				
0.379***	0.464***	0.307***	0.253***	0.138
(0.021)	(0.028)	(0.025)	(0.037)	(0.103)
[-0.193, 0.951]	[-0.044, 0.973]	[-0.078, 0.692]	[0.095, 0.411]	[-0.362, 0.638]
2,113	1,306	627	112	68
Panel F. Studies published in or since 2010				
0.357***	0.433***	0.328***	0.289***	0.138
(0.031)	(0.053)	(0.035)	(0.039)	(0.103)
[-0.263, 0.976]	[-0.337, 1.204]	[-0.169, 0.825]	[0.202, 0.375]	[-0.362, 0.638]
1,310	725	430	87	68
Panel G. Standardized tests only, USA settings only, omitting the top and bottom 2.5% of effect sizes, studies published in or since 2010				
0.320***	0.379***	0.305***	0.245***	0.155
(0.030)	(0.034)	(0.034)	(0.045)	(0.154)
[-0.054, 0.694]	[0.058, 0.701]	[-0.054, 0.663]	[0.095, 0.394]	[-0.468, 0.778]
840	462	268	52	58

Notes: *** p<0.10; ** p<0.05, * p<0.01. Prediction intervals are included for each estimate in brackets; robust standard errors are reported in parentheses. Each cell presents the Hedges' g estimate stacking both math and reading. Model (1) offers the pooled average impact of tutoring across the entire subsample indicated in each panel. Models (2) through (5) disaggregate the estimate in Model (1) by the tutored student sample size of each study. Panel D combines the sample restrictions in Panels B and C; Panel G combines the sample restrictions in Panels D, E, and F.

Table 6. Tests for significant differences in estimate mass across p-value thresholds

	p-value threshold = 0.10			*p*-value threshold = 0.05		
Bandwidth	± 0.02	± 0.01	± 0.005	± 0.02	± 0.01	± 0.005
Panel A. Full sample						
N estimates within bandwidth	117	54	32	180	91	47
% significant estimates	0.52	0.50	0.41	0.56	0.48	0.45
One-sided *p*-value	0.36	0.55	0.89	0.06	0.66	0.81
Panel B. Studies with 0-99 treated students						
N estimates within bandwidth	85	42	25	123	64	35
% significant estimates	0.52	0.50	0.44	0.53	0.42	0.43
One-sided *p*-value	0.41	0.56	0.79	0.29	0.92	0.84
Panel C. Studies with 100+ treated students						
N estimates within bandwidth	32	12	7	57	27	12
% significant estimates	0.53	0.50	0.29	0.63	0.63	0.50
One-sided *p*-value	0.43	0.61	0.94	0.03	0.12	0.61

Notes: Here we present the likelihood of observing the number of significant *p*-values in our data at the 5% and 10% significance levels within the bandwidths 0.02, 0.01, and 0.005 around those thresholds. For each of these combinations, we isolate the subsample within the indicated bandwidth around the indicated threshold ("N estimates within bandwidth"), present the share of significant estimate *p*-values in that range ("Share significant estimates"), calculate the likelihood of having at least that many significant estimates assuming a binomial distribution ("One-sided *p*-value"). We repeat this exercise for our full sample of estimates in Panel A, and disaggregated according to treated student sample size in Panels B and C. All estimates pool across both math and reading subject areas. All cells pool across math and reading.

Table 7. Intervention characteristics across treated student sample size

	Treated student sample size			
	0 to 99	100 to 399	400 to 999	≥1,000
Virtual / in-person delivery				
Tutoring online	0.52	6.31	0.00	23.08
Tutoring in-person	99.48	93.69	100.00	76.92
Where tutoring happens				
Tutoring at school	86.01	81.08	95.65	100.00
Tutoring at home	0.52	2.70	0.00	0.00
Tutoring in multiple locations / other	2.59	3.60	0.00	0.00
Tutoring location unknown	10.88	12.61	4.35	0.00
When tutoring happens				
Tutoring during school	76.68	72.07	86.96	84.62
Tutoring after school	6.22	9.01	0.00	0.00
Tutoring during vacation	0.52	0.00	0.00	0.00
Multiple time windows / other	3.11	6.31	8.70	0.00
Timing unknown	13.47	12.61	4.35	15.38
Student-tutor ratio				
1:1 student-tutor ratio	49.22	42.34	39.13	38.46
2:1 student-tutor ratio	11.92	21.62	13.04	38.46
3:1 student-tutor ratio	12.44	15.32	17.39	0.00
4:1 student-tutor ratio	13.99	16.22	13.04	0.00
≥5:1 student-tutor ratio	9.84	2.70	8.70	23.08
Ratio unknown	2.59	1.80	8.70	0.00
Tutor type				
Tutored by teacher	15.03	19.82	21.74	30.77
Tutored by paraprofessional	15.03	18.02	26.09	23.08
Tutored by peer	13.99	1.80	4.35	15.38
Tutored by college / grad student	19.17	14.41	4.35	7.69
Other tutor type	7.25	16.22	26.09	23.08
Tutor type unknown	29.53	29.73	17.39	0.00
Dosage				
Sessions per week	3.31	3.45	3.55	3.58
	(1.35)	(1.09)	(1.58)	(1.68)
Hours per session	0.61	0.60	0.53	0.72
	(0.44)	(0.29)	(0.23)	(0.39)
Hours per week	1.98	2.03	1.90	2.30
	(1.82)	(1.06)	(1.15)	(1.33)
Weeks per year	15.02	18.61	18.48	13.71
	(9.74)	(7.90)	(8.26)	(3.30)
Hours total dosage	30.32	39.17	36.85	27.00
	(31.68)	(31.00)	(35.10)	(11.84)
N interventions	193	111	23	13

Notes: Except for dosage variables, all measures are percents scaled 0 to 100. Dosage variable units are indicated, with standard deviations presented in parentheses.

Table 8. Meta-regression controlling for study and intervention features

	(1)		(2)	
100-399 treated sample (ref. 0-99)	-0.203***	(0.062)	-0.140**	(0.058)
400-999 treated sample (ref. 0-99)	-0.272***	(0.067)	-0.164**	(0.069)
≥1,000 treated sample (ref. 0-99)	-0.386***	(0.117)	-0.284*	(0.161)
Published in 2000s (ref. pre-2000)			0.152	(0.157)
Published in 2010s (ref. pre-2000)			0.004	(0.131)
Published in 2020s (ref. pre-2000)			0.054	(0.151)
Flag for poor RCT quality			0.005	(0.076)
Lower elementary math (ref. LE reading)			-0.150*	(0.084)
Upper elementary reading (ref. LE reading)			-0.029	(0.065)
Upper elementary math (ref. LE reading)			-0.122	(0.104)
Middle school reading (ref. LE reading)			-0.073	(0.140)
Middle school math (ref. LE reading)			-0.096	(0.138)
High school reading (ref. LE reading)			-0.224	(0.150)
High school math (ref. LE reading)			0.295	(0.267)
Researcher-generated assessment (ref. standardized test)			0.164*	0.224**
OECD country (ref. USA)			0.135	(0.142)
Tutoring delivered online (ref. in-person)			0.034	(0.171)
Tutoring at multiple locations / other (ref. at school)			-0.148	-0.016
Tutoring location missing (ref. at school)			0.069	(0.189)
Curriculum not provided			0.135	(0.148)
Tutoring outside of school hours (ref. during school)			-0.165***	-0.192***
Tutoring timing missing (ref. during school)			0.16	(0.151)
2:1 student-tutor ratio (ref. 1:1)			0.053	(0.063)
3:1 student-tutor ratio (ref. 1:1)			-0.058	(0.087)
4:1 student-tutor ratio (ref. 1:1)			-0.059	(0.077)
≥5:1 student-tutor ratio (ref. 1:1)			0.237	(0.251)
Ratio missing (ref. 1:1)			-0.186*	(0.102)
Tutored by paraprofessional (ref. teacher)			-0.042	(0.088)
Tutored by K-12 peer (ref. teacher)			-0.056	(0.125)
Tutored by college / graduate student (ref. teacher)			0.071	-0.081
Other tutor type (ref. teacher)			-0.162*	(0.088)
Tutor type missing (ref. teacher)			0.084	(0.091)
Total dosage 0-14 hours (ref. ≥60 hours)			0.17	(0.117)
Total dosage 15-29 hours (ref. ≥60 hours)			0.206**	(0.092)
Total dosage 30-44 hours (ref. ≥60 hours)			0.15	(0.106)
Total dosage 45-59 hours (ref. ≥60 hours)			0.213**	(0.083)
Total dosage missing (ref. ≥60 hours)			0.057	(0.089)
Constant	0.523***	(0.056)	0.274	(0.167)
Observations	2,223		2,223	

Notes: *** p<0.10; ** p<0.05, * p<0.01. Standard errors are presented in parentheses.

Table 9. Student sample characteristics by size of tutored student sample

	Treated student sample size	
	0 to 99	≥100
Student Demographics		
% Asian	2.48	2.40
% Black	31.04	33.52
% Hispanic / Latinx	29.41	24.98
% Native American	2.41	0.52
% Multiracial	0.59	2.26
% White	28.71	35.59
% Other	6.23	7.29
% free/reduced-price lunch	72.83	65.17
% special education	28.80	17.75
% English language learners	30.70	17.15
Program Targeted to Certain Students		
Any targeting	92.75	95.92
Targets low performers	88.60	85.71
Targets ELLs	8.29	0.68
Targets underserved students	19.17	21.77
Targets socioemotional problems	3.11	3.40
N interventions	193	147

Notes: Means are taken at the intervention level. All variables presented range from 0 to 100.

Table 10. Estimates for programs that combine best practices, stacked subjects

No sample size restriction	0-99 treated students	100-399 treated students	400-999 treated students
(1)	(2)	(3)	(4)
Panel A. Subsample of programs in-person, at school, during school, with ratio no more than 3:1, provided curricula, meeting ≥3 times per week, ≥15 hours of dosage			
0.433***	0.480***	0.394***	0.270***
(0.039)	(0.059)	(0.046)	(0.068)
732	474	216	42
Panel B. Subsample of programs using standardized tests, USA settings, in-person, at school, during school, with ratio no more than 3:1, provided curricula, meeting ≥3 times per week, ≥15 hours of dosage			
0.385***	0.415***	0.373***	0.254***
(0.036)	(0.053)	(0.049)	(0.073)
519	336	147	36
Panel C. Subsample of programs standardized tests, USA settings, no outliers, published since 2010, in-person, at school, during school, with ratio no more than 3:1, provided curricula, meeting ≥3 times per week, ≥15 hours of dosage			
0.375***	0.363***	0.390***	0.388***
(0.037)	(0.059)	(0.056)	(0.066)
299	180	103	16

Notes: *** p<0.10; ** p<0.05, * p<0.01. All cells stack estimates for math and reading. Column (1) presents the pooled meta-analytic estimated effect for the subsample of studies described in each panel. Columns (2) through (4) disaggregate the estimate in Column (1) according to the tutored student sample size. Panel A limits to the described subsample of programs sharing a set of best practices in their designs. Panel B restricts to studies with standardized test outcome measures and conducted in USA settings. Panel C additionally drops the top and bottom 2.5% of effect sizes from the whole sample ("no outliers"), and excludes studies published prior to 2010.

Table 11a. Average effect sizes across different program design features, stacked subjects

Delivery mode		Student-tutor ratio				
In-person	Virtual	1:1 ratio	2:1 ratio	3:1 ratio	4:1 ratio	≥5:1 ratio
(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Full analytic sample with no restrictions						
0.438***	0.065**	0.432***	0.406***	0.299***	0.341***	0.909**
(0.036)	(0.028)	(0.040)	(0.052)	(0.045)	(0.068)	(0.364)
2,164	59	1,061	290	314	367	154
Panel B. Standardized tests only, USA settings only						
0.368***	0.052***	0.465***	0.298***	0.263***	0.248***	0.244***
(0.028)	(0.005)	(0.054)	(0.056)	(0.040)	(0.041)	(0.083)
1,438	46	685	189	246	282	62
Panel C. Standardized tests only, USA settings only, omitting the top and bottom 2.5% of effect sizes, studies published in or since 2010						
0.345***	0.052***	0.374***	0.268***	0.292***	0.286***	0.292***
(0.023)	(0.005)	(0.053)	(0.094)	(0.039)	(0.042)	(0.076)
794	46	319	87	204	173	42

Notes: *** $p<0.10$; ** $p<0.05$, * $p<0.01$. Each column isolates a subsample of effects according to tutoring program characteristics. All estimates stack math and reading.

Table 11b. Average effect sizes across different program design features, stacked subjects

Tutor type				Total dosage of tutoring hours				
Teacher	Paraeducator	College / graduate student	K-12 peer	0-14 hours	15-29 hours	30-44 hours	45-59 hours	≥60 hours
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Full analytic sample with no restrictions								
0.381***	0.428***	0.397***	0.358***	0.541***	0.484***	0.465***	0.422***	0.256***
(0.055)	(0.042)	(0.049)	(0.092)	(0.133)	(0.069)	(0.067)	(0.074)	(0.044)
496	363	328	122	481	520	399	158	342
Panel B. Standardized tests only, USA settings only								
0.309***	0.396***	0.358***	0.429***	0.313***	0.507***	0.348***	0.430***	0.241***
(0.044)	(0.046)	(0.060)	(0.127)	(0.042)	(0.102)	(0.050)	(0.091)	(0.049)
388	229	194	93	281	323	236	109	291
Panel C. Standardized tests only, USA settings only, omitting the top and bottom 2.5% of effect sizes, studies published in or since 2010								
0.338***	0.355***	0.375***	0.109	0.308***	0.412***	0.305***	0.398***	0.220***
(0.046)	(0.040)	(0.075)	(0.189)	(0.045)	(0.042)	(0.044)	(0.109)	(0.066)
200	140	133	19	208	207	116	62	124

Notes: *** p<0.10; ** p<0.05, * p<0.01. Each column isolates a subsample of effects according to tutoring program characteristics. All estimates stack math and reading.

Table 12a. Multi-arm studies experimentally comparing different student-tutor ratios

Citation	N Treated Students	Subject	Small Ratio	Large Ratio	Students per tutor													Diff. (Small - Big)
					1	2	3	4	5	6	7	8	9	10	11	12	13	
Carlana & La Ferrara, 2024	607	Multiple	1:1	2:1														0.09
Clarke et al., 2017	415	Math	2:1	5:1														0.09
																		0.52
																		0.14
																		0.25
																		0.76
Clarke et al., 2020	880	Math	2:1	5:1														-0.02
																		-0.03
																		-0.03
																		0.01
																		-0.03
																		0.12
Clarke et al., 2023	322	Math	2:1	5:1														0.07
																		0.20
																		0.77
Doabler et al., 2019	465	Math	2:1	5:1														-0.01
																		0.04
																		0.00
																		0.10
																		-0.01
Kraft & Lovison, 2024	180	Math	1:1	3:1														0.14
Loeb et al., 2023	1,080	Reading	1:1	2:1														0.06
																		0.03

Table 12a. Continued

Citation	N Treated Students	Subject	Small Ratio	Large Ratio	Students per tutor 1	2	3	4	5	6	7	8	9	10	11	12	13	Diff. (Small - Big)
Schwartz et al., 2012	170	Reading	1:1	3:1														0.63
																		0.35
																		0.41
																		0.23
																		0.41
																		0.30
																		0.36
Vadasy & Sanders, 2008	54	Reading	1:1	2:1														-0.09
																		-0.22
																		-0.37
																		-0.12
																		-0.06
																		-0.22
																		-0.08
Vaughn et al., 2010	514	Reading	4:1	13:1														-0.10
																		-0.01
																		0.07
																		0.11
																		-0.08
																		0.26
																		0.08
																		0.22
																		0.13
																		0.11
																		0.06
																		0.23

Notes: All studies examine elementary programs except for three that study middle school programs: Carlana & La Ferrara (2024), Kraft & Lovison (2024) & Vaughn et al. (2010)

Table 12b. Multi-arm studies experimentally comparing different dosages of tutoring

Citation	N of Treated Students	Amount of Low Dosage	Amount of High Dosage	Total hours of tutoring												Effect Size Diff. (High - Low)
				2	8	14	20	26	32	38	44	50	56	62	68	
Al Otaiba et al., 2005	49	Two 20 min. sessions / week	Four 20 min. sessions / week													0.01 0.01 -0.12 0.18 0.13 0.15 0.18
Begeny, 2011	58	1.5 nine min. sessions / week	Three nine min. sessions / week													0.10 0.29
Carlana & La Ferrara, 2021	530	Three hours / week	Six hours / week													0.22
Wanzek & Vaughn, 2008	35	Five 30 min. sessions / week	Ten 30 min. sessions / week													-0.4 0.54 0.39 -0.76

Note: There is more than one effect in each study because authors report effects on multiple reading outcomes or assessments. All studies examine tutoring in reading subjects except for Carlana & La Ferrera (2021) which focused on multiple subjects.

Appendix A. Included Studies

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Appendix B. Additional Tables and Figures

Table B1. Trim and fill estimates, stacked subjects

Effect-level estimates		Study-by-subject-level estimates	
Observed	Observed and imputed	Observed	Observed and imputed
(1)	(2)	(3)	(4)
Panel A. Full sample			
0.444***	0.444***	0.348***	0.348***
(0.014)	(0.014)	(0.023)	(0.023)
2,223	2,223	277	277
Panel B. Studies with 0-99 treated students			
0.550***	0.550***	0.458***	0.458***
(0.021)	(0.021)	(0.039)	(0.039)
1,403	1,403	166	166
Panel C. Studies with ≥100 students			
0.289***	0.289***	0.239***	0.239***
(0.013)	(0.013)	(0.022)	(0.022)
820	820	111	111

Notes: *** *p*<0.10; ** *p*<0.05, * *p*<0.01. All estimates stack math and reading. Columns (1) and (2) report trim and fill estimate results on effect size level observations, while columns (3) and (4) report the same for observations collapsed to the study by subject area level. While Panel A conducts this estimation on the full sample, Panels B and C subdivide the sample into studies of programs serving less than and at least 100 tutored students, respectively. Columns (2) and (4) impute values if there are imbalances in the distribution of effects reported in columns (1) and (3), respectively. Note that if the distribution of effects was imbalanced around the average pooled estimate, the samples in columns (2) and (4) would include additional, imputed values that fill in the "gaps" of those distributions; observing no differences in these samples is evidence results are not meaningfully impacted by publication bias.

Table B2. Estimates pooled by publication type

No sample size restriction	0-99 treated students	100-399 treated students	400-999 treated students	≥1000 treated students
(1)	(2)	(3)	(4)	(5)
Panel A. Subsample of studies published in peer-reviewed journals				
0.454***	0.543***	0.340***	0.279***	0.325*
(0.040)	(0.064)	(0.029)	(0.048)	(0.193)
1940	1288	570	67	15
Panel B. Subsample of studies not published in peer-reviewed journals				
0.220***	0.572***	0.150***	0.207***	0.085
(0.056)	(0.133)	(0.029)	(0.052)	(0.064)
283	115	70	45	53

Notes: *** *p*<0.10; ** *p*<0.05, * *p*<0.01. Column (1) present the pooled meta-analytic average effect size estimate for the subsample of effects described by each panel. This estimate is disaggregated across treated student sample size in columns (2) through (5). Each panel isolates a single feature of tutoring program design that has been associated with elevated impacts in prior research.

Table B3. Estimates pooled by best practices, stacked subjects

No sample size restriction (1)	0-99 treated students (2)	100-399 treated students (3)	400-999 treated students (4)	≥1000 treated students (5)
Panel A. Subsample of tutoring programs conducted in-person				
0.438***	0.545***	0.320***	0.253***	0.276**
(0.036)	(0.060)	(0.026)	(0.037)	(0.118)
2,164	1,396	630	112	26
Panel B. Subsample of tutoring programs conducted at school				
0.404***	0.504***	0.318***	0.260***	0.138
(0.030)	(0.045)	(0.030)	(0.038)	(0.103)
1,885	1,236	473	108	68
Panel C. Subsample of tutoring programs conducted during the school day				
0.388***	0.473***	0.324***	0.272***	0.147
(0.027)	(0.039)	(0.028)	(0.040)	(0.125)
1,748	1,086	496	103	63
Panel D. Subsample of tutoring programs with student-tutor ratios of no more than 3:1				
0.400***	0.504***	0.327***	0.245***	0.147
(0.030)	(0.047)	(0.028)	(0.042)	(0.129)
1,665	999	531	73	62
Panel E. Subsample of tutoring programs delivering ≥15 total hours of tutoring				
0.423***	0.523***	0.332***	0.254***	0.306***
(0.039)	(0.070)	(0.032)	(0.042)	(0.115)
1,572	1,003	447	92	30
Panel F. Subsample of tutoring programs using curricula for lessons				
0.410***	0.518***	0.330***	0.260***	0.055***
(0.036)	(0.059)	(0.027)	(0.038)	(0.008)
2,011	1,245	602	109	55
Panel G. Subsample of tutoring programs with ≥3 sessions per week				
0.411***	0.509***	0.348***	0.260***	0.150
(0.030)	(0.049)	(0.026)	(0.045)	(0.135)
1,805	1,086	573	85	61

Notes: *** p<0.10; ** p<0.05, * p<0.01. Column (1) present the pooled meta-analytic average effect size estimate for the subsample of effects described by each panel. This estimate is disaggregated across treated student sample size in columns (2) through (5). Each panel isolates a single feature of tutoring program design that has been associated with elevated impacts in prior research.