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**From problem-solving to sensemaking: A comparative meta-analysis of
preparatory approaches for future learning**

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Author Note

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Abstract

Against the backdrop of a growing body of research showing the effectiveness of problem-solving activities followed by instruction (PS-I), we report a meta-analysis of the effectiveness of three broad categories of preparatory activities on future learning from instruction: (a) problem-solving followed by instruction (PS-I), (b) scaffolded problem-solving followed by instruction (+PS-I), or (c) an alternative sensemaking activity followed by instruction (!PS-I)? We examined 118 experimental comparisons spanning 33 articles that compared PS-I with +PS-I and !PS-I designs. Although scaffolding was descriptively associated with a small effect size, there was no significant difference relative to PS-I (Hedge's g -0.08 [95% CI -0.20, 0.04]). Additionally, PS-I exhibited a non-significant moderate effect (Hedge's g 0.22 [95% CI -0.06, 0.51]) compared to !PS-I. Bayesian analyses strongly favored the null hypothesis for the comparison of PS-I with +PS-I (suggesting a 99% probability of the difference in effect between these designs being less than 0.2), while it suggested a 40.37% probability of at least a moderate effect favoring PS-I relative to !PS-I. Further, the estimation of true effect sizes after accounting for the publication bias suggested moderate effect sizes in favor of PS-I, when considering both comparison conditions +PS-I (Hedge's g 0.55) and !PS-I (Hedge's g 0.64).

Keywords: Learning through Problem-Solving, Productive Failure, Scaffolding, Sensemaking, Preparation for Future Learning.

1. Introduction

Designing learning experiences that prepare students for future learning [1] is an important, yet under-appreciated educational goal. The intellectual climate around education at the beginning of the nineteenth and twentieth centuries, with its emphasis on conformity and templates of successful learning, still forms the basis of how the majority of formal classroom instruction is designed. Routines of instruction followed by problem-solving practice (I-PS) are commonplace in educational practice. I-PS rests on the assumption that all students can learn new materials when they have mastered prerequisite knowledge and skills and the instruction is unambiguous [2, 3].

There has been a growing interest in alternative pedagogies that flip this traditional I-PS design sequence [4], and assume students' ability to learn to be a strong function of their ability to actively construct understandings [5]. Such problem-solving followed by instruction (PS-I) routines exploit the hidden efficacy in students' movement through phases of "construction, unconstruction, and reconstruction" [6, p. 429], even as their knowledge of a targeted concept is still in development. PS-I intentionally (a) activates relevant prior knowledge, (b) enhances awareness of the problem situation and students' knowledge gaps, (c) focuses attention on search for critical task features, and (e) elicits curiosity to know the canonical answer [4, 7].

We recently conducted a meta-analysis of 53 studies with 166 experimental comparisons to investigate the relative efficacy of I-PS and PS-I routines [8]. Our results suggested a significant moderate effect size in favor of PS-I (Hedge's g 0.36 [95% CI 0.20, 0.51]) for learning outcomes of conceptual understanding and transfer. These effects were even stronger (Hedge's g ranging between 0.37 to 0.58) when PS-I was implemented with high fidelity to the principles of Productive Failure (PF; [7, 9, 10]), a variant of PS-I that intentionally designs for and uses failure in preparatory problem-solving for learning. Given a

seemingly robust trend of the superiority of PS-I designs across several learning domains and student populations, we sought to assess the generalizability of this evidence and asked

- a. Might there be ways to further improve the effectiveness of preparatory problem-solving, e.g., by explicit forms of scaffolding (+PS-I)?
- b. Might we consider alternative sensemaking activities (!PS-I) that perform at par (or even out-perform) preparatory problem-solving, e.g., studying worked examples [11], evaluating pre-designed solutions [12], ranking contrasting cases [13], problem-posing [14]?

Against the backdrop of these two overarching research goals, we attempt to compare PS-I with +PS-I and !PS-I in the current article. Consequently, the current work does not (and cannot) say anything about how PS-I fares relative to I-PS.

2. Background

2.1 Scaffolding PS-I (+PS-I)

Scaffolding (+PS-I), or administering just-in-time supports to allow students to make meaningful progress in a problem-solving task [15], is one way in which preparatory problem-solving quality can be positively influenced. The idea here is that if the PS-I design is effective even without any explicit scaffolding in the PS phase, then might not scaffolding the PS phase enhance the overall effectiveness of PS-I? Scaffolding preparatory problem-solving can be expected to increase not only the diversity of generated solution approaches [16, 17] but also students' engagement in critical learning activities like exploratory analysis, peer interaction, self-explanation, evaluation, etc [18]. This might aid students in curtailing non-promising solution pathways early and gaining insight into whether the current solution approach towards the problem is reasonable. Taken together, scaffolded problem-solving can be regarded as a more substantial preparation for follow up instruction because of deliberate student involvement in solution creation and revision, affording greater prior knowledge activation.

However, empirical results from the PS-I literature suggest that the quality of student-generated solutions largely depends on the interaction between student attitudes towards inquiry [19] and individual differences in learning from success and failure [20]. Further, explicit scaffolding might impose external learning goals during problem-solving that are psychologically very different than self-directed learning [21]. During problem-solving prior to formal instruction, learning goals are likely to be internally generated. Therefore, students may presumably not be in a context where they can be instructed – they can only be provided with design environments to support their efforts to learn. We will now briefly describe the rationale behind commonly studied forms of scaffolding for which we have empirical evidence in this meta-analysis. As an illustrative and descriptive taxonomy, we will draw from seminal work on structuring versus problematizing student work [22]. Structuring scaffolds reduce degrees of freedom to lower task complexity and make problem-solving tractable. Problematizing scaffolds increase degrees of freedom to challenge students' understanding by explicitly highlighting task discrepancies.

2.1.1 Structuring (*Cognitive support*)

Explicit cognitive scaffolding during preparatory problem-solving is likely to increase success-likelihood. For instance, [23] demonstrated in a PS-I study that scaffolding students to detect a high number of relevant similarities and differences in contrasting cases during the problem-solving phase can lead to focused elaboration regarding deep features of the problem post-instruction. This was associated with improvements in conceptual understanding. Similar findings have been echoed in prior PS-I work on the topic of density [24]. Here, students who recalled the deep structure of ratio from their invention activity were the ones who ultimately benefited from activating their prior knowledge on assessments of transfer. Cognitive scaffolding might then be conceived as one means to help students activate prior knowledge before receiving instruction [20].

However, it is plausible that scaffolded problem-solving experiences are insufficiently disruptive to challenge existing thought processes. This can, in turn, impede students' ability to handle being wrong [25, 26], and result in problem-solving behaviors consistent with the conventional wisdom that *if it ain't broke don't fix it*. Alternatively, it can also be posited that without possessing formal operators of a domain, sensemaking with cognitive scaffolding can be challenging and may inhibit fuller exploration of the problem and solution spaces. These tradeoffs may reduce the differential advantage of +PS-I relative to PS-I. In the empirical studies included in the current meta-analysis, cognitive support was instantiated via forms such as hints on critical task features, task guidance to reiterate goals and constraints of the task, subgoal labeling as well as strategy/outcome feedback during the process of problem-solving.

2.1.2 Structuring (*Metacognitive support*)

Explicit metacognitive scaffolding during preparatory problem-solving is likely to engage students in structured or systematic problem-solving by evaluating and revising solution methods and appraising sources of error [27]. This might increase the likelihood of students activating and noticing critical task features. However, engagement in productive inquiry behaviors may not always translate to better posttest outcomes and sufficient practice opportunities may be needed to internalize these behaviors. Further, the time spent on attending to scaffolding prompts takes valuable time away from solution iteration in the form of more attempts, which might curb student agency and mitigate the positive benefits of engaging in metacognitive learning activities.

For instance, in +PS-I research conducted by [28] in physics, task failure in the form of circuit explosion (entire electrical circuit goes up in flames and a restart is required) was found to be negatively related to learning outcomes. Behavioral logs showed that students were not able to meaningfully grapple with the task complexity and lacked an understanding of basic task elements. Further, prompts for metacognitive reflection did not help them address these

recurring failures. Taken together, by being exposed to structuring scaffolds during initial exploration in the form of explicit cognitive or metacognitive support, there is a risk of students failing to develop appreciation of factors that can impede long-term learning [29, 30].

2.1.3 Problematising

In contrast to structuring, problematising scaffolds during preparatory problem-solving serve the function of making students grapple with potentially overlooked aspects of the problem. By using strategies such as eliciting articulation with constrained options and asking students to explicitly connect their thinking to key disciplinary ideas, students are nudged towards identifying discrepancies among problem features [31]. This can in turn increase the likelihood that students avoid premature termination of solution approaches even if they may initially seem non-promising. For instance, [31] used feature contrast, ranking activities and self-explanation activities to problematize students' intuitive notions of crowdedness in an invention task on density. Although explicit problematising can meaningfully challenge student reasoning, implementing it within an already ill-structured preparatory problem-solving phase might sound like overkill, especially for students who are not able to generate a high number of diverse solutions necessary to be optimally prepared for learning from instruction. To an extent, this tradeoff can be expected to mitigate +PS-I's effectiveness relative to PS-I.

2.2 Alternative Preparatory Sensemaking Activities (!PS-I)

Sensemaking refers to “the basic operations of science inquiry such as generating hypotheses, designing comparisons, collecting observations, analyzing data, and constructing interpretations” [32, p. 344]. Sensemaking opportunities can be instantiated through different learning designs that allow students to identify, construct, manipulate and coordinate among representations, albeit to different degrees. Such learning designs, including but not limited to preparatory problem-solving, hold great potential in triggering students' readiness to learn a targeted concept in the current situation as well as in facilitating accelerated learning in novel future situations [1]. Learning opportunities afforded by preparatory sensemaking reflect “an

interactional phenomenon, which extends beyond a unidirectional presentation of information” [33, p. 76], and position students relative to the presented task “in ways that help them realize options for understanding and designing solutions to a meaningful problem” [34, p. 140].

When considering preparatory sensemaking activities, however, several common challenges stemming from the gap between students’ intuitive thinking and disciplinary formalisms before having learned a concept, their failure to notice and encode critical features of a concept, etc emerge [4, 7, 32]. Each alternative form of preparation involves consideration of how the associated tradeoffs relative to preparatory problem-solving might differentially impact learning outcomes. For example, prior literature has systematically examined conditions constraining the effectiveness of presenting students with partially or fully-solved worked examples [35], or asking them to explicitly generate explanations [36]. We will now briefly describe the rationale behind commonly studied preparatory sensemaking activities for which we have empirical evidence in this meta-analysis.

2.2.1 *Worked Examples*

Worked examples are typically designed to occur after formal instruction as a substitute for solving multiple practice problems. Their design rationale draws on the long-standing pedagogical philosophy of reducing the cognitive load for novices by modeling how experts solve a problem [2, 11]. In this meta-analysis, however, we refer only to empirical studies that use worked examples as a sensemaking activity prior to formal instruction. By providing concrete stepwise illustrations and explanations of interrelated problem steps, students are expected to profit from (a) lower failure likelihood and susceptibility to negative affective experiences, including knowledge insufficiency and lack of confidence [37], (b) reduced stickiness to suboptimal ways of thinking and reasoning [38, 39], and (c) high valuation of self-generated solutions [40]. Further, devoting the majority of the time to study a fully (or, partially) worked-out example alleviates the need to engage in effortful retrieval [41] to resolve

problem-solving uncertainty and reduces the burden on the limited working memory capacity. However, in the absence of opportunities for elaborative processing [36], that is, showing sufficient depth of understanding about the targeted topic by being able to generate appropriate inferences and responding to questions related to it, preparation for learning from follow up instruction may be limited. In contrast, preparatory problem-solving, which triggers awareness of knowledge gaps [4] because of having to generate inferences about causes behind outcomes, may provide a relatively better springboard for knowledge integration and assembly.

2.2.2 Evaluating Pre-designed Solutions

Evaluating pre-designed solutions prior to instruction can be conceived as a vicarious failure experience for students. This is because pre-designed solutions are typically chosen such that one or more critical features of the targeted concept is missing. For instance, in their study targeting the math concept of variability, [42] gave students a measure of spread that failed to use all data points in the presented problem ($range = max - min$) and asked students to explain their rationale by testing against multiple datasets. Asking students to merely evaluate a given erroneous or suboptimal solution may be posited to take up fewer cognitive resources [12, 43], which might free up resources for solution schema acquisition when exposed to instruction later. Although such a preparatory activity may be less time-consuming and relatively less frustrating than self-generating solutions, students may miss opportunities to engage in deeper comparisons between solutions that can increase the likelihood of attending to critical problem features. These tradeoffs have been exemplified in empirical work comparing evaluation of pre-designed solutions with actually engaging in problem-solving prior to instruction [12, 43].

2.2.3 Ranking Contrasting Cases

The use of ranking activities with contrasting cases offers a strong alternative to preparatory problem-solving. Reasoning about a problem-solving task by comparing different cases can support incremental (progressive) noticing of critical features of a targeted concept. Further, when ranking cases intuitively according to the targeted concept (e.g., variability),

mismatches in ranking can serve as iterative debugging opportunities to refine one's intuition by incorporating previously overlooked information [13]. However, rather than picking up the invariant of a critical feature across different sets of contrasting cases, students may merely notice what they already know – this can lead to a comparison of familiar features to align one's own ranking with the canonical ranking of the presented cases [13, 24]. Alternatively, students' unwillingness to engage meaningfully with critical task features to formulate a ranking (despite noticing them) may also limit the learning benefits of ranking contrasting cases prior to instruction [44]. These tradeoffs call for evaluating the relative effectiveness of ranking contrasting cases versus generative problem-solving as a preparatory activity.

2.2.4 Others (Text Comprehension, Problem Posing, Pre-instruction)

In addition to the above three most commonly implemented preparatory sensemaking activities, empirical work comparing !PS-I with PS-I can sometimes also involve text comprehension, including reading and/or summarizing text. Reading learning materials involving descriptions of important patterns and distinctions can be conceived as a relatively more passive form of preparation than actively summarizing important elements of the given learning materials, which can, in turn be posited to be a weaker form of preparation than constructing new representations via problem-solving [45]. Comparison of text comprehension with problem-solving as a preparatory sensemaking activity can inform whether students may benefit from elaboration alone, or whether the ability to discern critical task features and develop differentiated knowledge that comes with generation is necessary [1].

Problem-posing, or, the generation of new problems from a given situation, is another preparatory sensemaking activity against which problem-solving has been pitted in prior work. The relatively unconstrained and goal-free nature of problem-posing may lower cognitive load and afford greater prior knowledge activation and differentiation [14], however, at the potential expense of the relevance of prior knowledge activation. Unless student-generated problems

explore relevant states that help noticing the deep structure of the problem space, reduction in cognitive load may not translate into schema acquisition post-instruction [46]. Finally, by pre-instruction as a preparatory sensemaking activity, we refer to a well-structured learning activity informing students about key critical task features [47].

2.3 The Present Meta-Analysis

In light of the above discussed literature and the tradeoffs among designs, the current meta-analysis aims at examining comparisons between PS-I and +PS-I, and between PS-I and !PS-I to see (a) What forms of preparation prior to instruction yield beneficial learning outcomes?, (b) What learning design elements might these differential effects be attributed to? To the best of our knowledge, no former quantitative meta-analytic reviews exist in this area.

One prior narrative review [48] looked at how students exposed to PS-I treatments performed relative to those in +PS-I and !PS-I comparison conditions. Descriptive results suggested that in the absence of spontaneous task reasoning with critical task features (that can potentially be scaffolded within a +PS-I design), students exposed to PS-I are likely to underperform or perform similar relative to +PS-I students. Across the small number of comparisons of PS-I with +PS-I and !PS-I [48], the fidelity of PS-I to the design principles of PF [10] was found to be low to moderate. Here, we statistically validate these descriptive trends with a substantially greater number of experimental comparisons. Bringing empirical evidence to bear on the relative efficacy of preparatory sensemaking approaches is vital for advancing the learning theory as well as practice [7]. Specifically, we address the following research questions for comparisons of PS-I with both +PS-I and !PS-I.

- a. *RQ1* – How does the design of various preparatory activities affect student learning from subsequent instruction?
- b. *RQ2* – How does the strength of such differential effects vary with fidelity of PS-I to PF design principles, student characteristics and intervention characteristics?

- c. *RQ3* – How does publication bias affect the overall pooled effect size?

3. Method

3.1 Search Criteria

Since our analysis focuses on examining empirical evidence for the relative efficacy of instructional methods that can prepare students for learning from instruction, we started our search by identifying records from Google Scholar databases which

- a. cited either of
 - a. the recent qualitative PS-I review [4]
 - b. the two seminal sensemaking prior to instruction articles [1, 49]
 - c. the two seminal PF articles [9, 10] and/or other key follow-up PF articles [7, 14, 43], and
- b. reported experimental or quasi-experimental comparison between PS-I and +PS-I, or between PS-I and !PS-I, and
- c. assessed learning outcomes of conceptual knowledge and/or transfer.

Google Scholar was chosen because it is currently the most comprehensive academic search engine with 389 million records [50]. In addition, it allows access to both published and unpublished literature (e.g., dissertations). Further, since PF, a subset of the PS-I design falls under the broader design paradigm of preparation for future learning, and pre-dates PS-I [10], our search criteria included representative articles from the PF literature. The three follow-up PF articles were published in flagship journals of the Cognitive Science Society (CSS), European Association for Research on Learning and Instruction (EARLI) and American Psychological Association (APA). The expected diversity in readership across these avenues led us to include citations from these articles, in addition to citations from the two seminal PF articles. Our comprehensive and inclusive search criteria *a* resulted in 3073 articles as of 26th June 2019, with the Loibl et al. review [4] contributing 61 articles, each of the two sensemaking

articles contributing 1135 and 726 articles, and each of the five PF articles contributing 552, 349, 171, 33 and 107 articles to the pool. After removal of 862 duplicate records, 33 of the remaining 2272 articles met criteria *b* and *c*. These 33 articles comprised 118 experimental comparisons. Just limiting the search to PS-I studies may have under-determined the total number of studies available for the meta-analysis. Table 1 presents a breakdown of demographic characteristics for the included comparisons. Figure 1 depicts the summarized PRISMA flowchart [51]. References to articles for all comparisons can be found in the supplementary materials.

The majority of the studies span North America (65.3%), target learning concepts in math (64.4%), and span 6th-10th graders (55.1%). There is growing evidence for expansion of intervention-driven research comparing PS-I to +PS-I and !PS-I to undergraduate student populations (23.7%). Clearly, there is lack of work with postgraduate and/or professional student populations. Also, we did not find a sufficiently large number of studies spanning non-STEM domains. The majority of the $n = 60$ +PS-I comparisons fell into the category of *structuring* (cognitive support: 75%, metacognitive support: 15%, cognitive + metacognitive support: 3.33%). Only 6.66% comparisons fell into the category of *problematizing*, instantiated via feature contrast, ranking activities and eliciting explanations. The $n = 58$!PS-I comparisons, on the other hand, comprised the following preparatory activities: ranking contrasting cases (29.31%), worked examples (27.59%), evaluating pre-designed solutions (27.59%), text comprehension (5.17%), problem-posing (6.89%), and pre-instruction (3.45%). Finally, the articles excluded from the meta-analysis fell into the categories of (a) experimental studies (not involving comparisons of PS-I with !PS-I or +PS-I), (b) theoretical or methodological review articles, and (c) observational studies describing student interaction and/or the process of learning. This naturally meant that studies without an explicit instruction phase were excluded. Please refer to supplementary materials for exemplar articles from these categories.

3.2 Meta-Analysis

With these selected articles, our first goal was to get one overall effect size estimate of experimental comparisons between PF and +PS-I ($n = 60$), PF and !PS-I ($n = 58$). These effect sizes corresponded to the learning outcomes of conceptual understanding and/or transfer. As an illustrative example, suppose one wants to teach students a math concept (and its associated procedures) that is novel to them, say standard deviation (SD). Conceptual understanding assessments would test understanding of critical features of SD and deducing its mathematical properties. Transfer assessments would test whether students can adapt knowledge of SD to solve problems on the concept of normalization that is not explicitly covered in the instruction.

Subsequently these effect sizes were broken down for subgroups within the comparisons. For comparisons where information critical for pooling effect sizes was not present in the articles (M , SD , sample size), personal communication was established with relevant researchers. We sincerely thank all researchers who promptly got back.

3.2.1 Pooling Effect Sizes

To answer RQ1, a multilevel meta-analysis model was used to pool the effect sizes [52]. We explicitly accounted for participants nested within studies, and studies nested within the included articles. The use of such a three-level structure was based on the assumption that (a) studies did not come from the same population, and that (b) accounting for the dependency between effect sizes for multiple studies reported within the same research article was critical for statistical independence, a core assumption of meta-analytic pooling. We used Hedge's g (bias corrected standardized mean difference) to assess overall effect along with 95% confidence intervals (CI). Two standard metrics, Cochran's Q (reflecting variance of the distribution of true effect sizes) and Higgin's and Thompson's I^2 , were used for quantifying statistical heterogeneity in the collected effect size data. In addition to these traditionally-used frequentist meta-analysis metrics, we also used Bayesian methods that afford computation of exact probabilities of the true overall effect being smaller than a particular value (e.g., 0.2) –

this can then be used to argue about meaningful differences between conditions using standard conventions in educational research (e.g., Cohen's).

3.2.2 Subgroup Analysis

To answer RQ2, we unpacked between-study heterogeneity in our three-level meta-analytic model. Specifically, we looked at a range of subgroup categories that might explain why the included studies show varying results. Subgroups, along with the definition, coding criteria and brief inclusion rationale, are summarized below.

- a. PF fidelity criteria within the PS-I design ($\kappa = 0.92, 1.0, 0.87, 1.0, 1.0, 0.73$ and 0.91)
 - Given that our prior quantitative meta-analysis [8] has found PS-I to be more effective than I-PS when aligned with the design principles of PF, we coded for these design principles to evaluate the generalizability of these results when comparison conditions differ in the nature of sensemaking activity preceding instruction. Based on the coding scheme created by [8] drawing on the design principles of PF [7, 10], and the nature of information found in the reviewed articles, we coded for the following seven criteria. It must be noted that not all PS-I designs are PF, but only those that follow the design principles of PF articulated below (see [8] for details). See Figure 2 for an illustration.
 - a. Problems affording multiple representations and solution methods (RSMs): *yes*, *no*
 - b. Qualitative or quantitative evidence for multiple RSM generation in the article (e.g., if authors tabulated quantity/quality of solutions students generated): *yes*, *no*. Not all articles where the problem design afforded multiple RSMs necessarily comprised evidence of students actually generating multiple RSMs. Since the quantity and quality of RSMs generated has been considered a proxy for the cognitive mechanism of prior knowledge activation in former PS-I literature [17, 43], this as an important distinction. There was low dependency

between these nominal variables for both +PS-I (Contingency coefficient = 0) and !PS-I (Contingency coefficient = 0.14) comparisons. Note that the coding validity for this PF fidelity criteria may be threatened by studies where the diversity of solution approaches was not part of the research questions, resulting in lack of reporting of empirical evidence for multiple RSM generation in the article.

- c. Affective draw of the problem considered (e.g., if the problem design comprised story problems within/outside virtual environments, contrasting cases, simulations, agent-based modeling, etc): *yes, no*
- d. Groupwork as the participation structure during problem-solving phase: *yes, no*
- e. Instruction building on student solutions: *yes, no*
- f. Social surround facilitation during problem-solving phase: *high* (if the intervention explicitly mentioned (considered) more than one of the following sub-criteria (a) the problem-solving phase as a safe space to explore and generate ideas, (b) the social and mathematical norms around the activity (e.g., it is okay not to be able to solve problems as long you try various ways of solving them; highlighting to students that there are multiple solution approaches for the problem-solving; setting the expectation that understanding why and under what conditions some solutions are better than others is important), (c) provision of affective/motivational support to persist/keep trying (excluding prompts for cognitive or metacognitive support), and/or if teacher training was held on the PF instructional design prior to the intervention), *low* (only one of these criteria were considered). There was 35% ($n = 21$) unreported data for this PF fidelity criteria in PS-I v/s +PS-I comparisons, and 53.45% ($n = 31$) unreported data in PS-I v/s !PS-I comparisons. The relatively low inter-rater reliability for this

criterion ($\kappa = 0.73$) reflects that we found it relatively harder to code compared to rest of the criteria. Because of the lack of sufficient variation in the included comparisons, it was not plausible to apply a finer-grained coding scheme.

- g. Social surround facilitation during instruction phase: *dialogue-dominant* (the teacher asked clarifying questions and/or paraphrased student solutions and/or asked students to elaborate upon each other's ideas and/or engaged students in arguments/conflict and/or used other kinds of facilitation strategies that enhance student engagement), *monologue-dominant* (one-way transmission of information about the concept, expert solutions or common student solutions).

In addition to looking at each of these seven binary PF fidelity criteria in isolation for every included comparison, we also computed an *overall PF fidelity score* for later analyses – this is defined as a ratio (percentage) of the number of PF fidelity criteria present within the PS-I design to the total number of criteria that could be coded.

- b. Student characteristics ($\kappa = 1.0, 1.0$ and 0.84) – Following standard meta-analysis protocols, demographic and/or incoming student characteristics were included. Additionally, we coded for the nature of pretesting to differentiate between facilitatory effects of forward pre-testing [53] across the intervention versus the potential dilution of effects favoring PS-I with the inclusion of a pretesting phase already activating learning mechanisms (e.g., prior knowledge activation) similar to the initial problem-solving phase [54].
 - a. Age range (as assessed by their grade level): *2nd-5th graders, 6th-10th graders, undergraduates, postgraduates*
 - b. Geographical distribution: *Europe, North America, Asia, Australia*
 - c. Nature of pretesting: *none* (no pretest administered), *prerequisite* (pretest was administered on prerequisite concepts but not those targeted in the intervention),

targeted (pretest was administered on concepts similar to those targeted in the intervention). As an illustrative example, the pretest for a PS-I intervention targeting the learning concept of standard deviation would be coded as *prerequisite* if the questions comprise only basic descriptive statistics (e.g., mean, median), and *targeted* if the questions additionally also comprise standard deviation.

- c. Intervention characteristics ($\kappa = 1.0, 1.0, 1.0, 1.0, 1.0, 1.0$) – Following standard meta-analysis protocols, the experimental nature of the intervention and its time span along with the targeted domain and learning outcomes were coded. We also separated overall effects by the type of scaffolding and the type of alternative preparatory activity.
 - a. Type: *experimental* (random assignment to treatment or control groups; groups differ only in terms of experimental treatment they receive, that is, the presence of scaffolding during the problem-solving phase or a preparatory activity other than problem-solving), *quasi-experimental* (classroom studies where random assignment is not possible and whole classes are assigned to the treatment or control groups; treatment and control groups differ in more than just the experimental treatment they receive, e.g., different teachers, different modality of presentation during instruction). Explicitly coding for intervention type accounts for quality of the interventions and clearly differentiates studies where altering only one variable at a time is not possible/plausible.
 - b. Time span: *a few hours* (within 1 day), *a few days* (> 1 day). The intervention length comprises the actual experiment (problem-solving and instruction phases), along with any administered pretests and posttests.

- c. Learning concepts targeted: *Math, Basic sciences (Physics, Chemistry, Biology), Domain-general skill (e.g., Rubik's cube), Psychology, Others (e.g., management theory, assessment of learning strategies)*
- d. Learning outcomes assessed: *conceptual understanding, transfer, clubbed outcomes* (single posttest assessing both conceptual understanding and transfer)
- e. Type of scaffolding (only for +PS-I comparisons): *structuring (cognitive), structuring (metacognitive), structuring (mixed, involving both cognitive and metacognitive support), problematizing*
- f. Type of preparatory activity (only for !PS-I comparisons): *worked example, evaluating pre-designed solutions, ranking contrasting cases, text comprehension, problem-posing, pre-instruction.*

For PF design fidelity criteria, the primary authors discussed application of the criteria to 20% example articles and agreed on the coding criteria, after which the first author (with the help of a research assistant) coded the remaining comparisons. Inter-rater reliability was $\kappa > 0.7$. Disagreements were resolved with discussion. Coding scheme for other relatively more subjective categories such as prior knowledge activation, instructional discourse style, etc were iteratively developed. Inter-rater reliability $\kappa > 0.7$ was established, after which one rater coded rest of the data. As before, disagreements were resolved with discussion. Table 2 showcases some examples of comparisons with varying subgroup categories [23, 42, 54, 55]. Complete coding of subgroup categories and finer-grained details on the type of scaffolding (for +PS-I designs) can be found in the supplementary materials.

Note that our described findings for subgroup analyses will be predominantly exploratory in that we intend to test for potential main effects as well as interactions that may illuminate the differential nature of effects found across comparisons of PS-I with +PS-I and !PS-I. Because of the aforementioned tradeoffs in the introductory background section

regarding how the type of scaffolding or an alternative preparatory activity may affect learning, specific directional hypotheses are not formulated. Confirmatory findings will therefore only be limited to (a) evaluating the generalizable impact of PS-I's fidelity to PF fidelity criteria, and (b) assessing how the presence and nature of pretesting differentially impacts learning.

3.2.3 Publication Bias

As part of the meta-analysis, we also investigated publication bias in the selected articles based on two different theoretical assumptions. First, we visualized whether small studies with small effect sizes were missing through funnel plots. The underlying assumption was that small studies are at greatest risk for being non-significant (and thus being missing), while large studies are likely to get published irrespective of the significance of results. In the case of no publication bias, all studies would lie symmetrically around the pooled effect size within the form of the funnel. In the presence of publication bias, the funnel should look asymmetrical, because only the small studies with a large effect size would be published, while small studies without a significant, large effect would be missing. Additional inferential statistics (Egger's test) were used to test for presence of asymmetry in the funnel plot.

Second, we explored an alternative way to assess the possibility of publication bias. We visualized p-curves (distribution of significant p-values) for comparisons of PS-I with +PS-I (and PS-I with !PS-I) to examine whether our data reported more significant p values that were low ($p < .01$) rather than high ($.04 < p < .05$). True effects, those that differ from zero (e.g., more .01s than .04s), lead to right-skewed p-curves, and nonexistent effects lead to flat p-curves (as many .01s as .04s). Follow up statistical tests were conducted to assess whether the p-curve was significantly right-skewed (indicative of a true effect behind the data), and whether it was flat (indicative of insufficient power, or there being no true effect behind the data). Based on a significant right-skewness test and a non-significant flatness test, we could ascertain the presence or absence of evidential value in the p-curve (see [56] for technical details).

4. Results and Discussion

As previously stated, the goal of this article is to meta-analytically compare PS-I with scaffolded PS-I (+PS-I) and alternative preparatory activity followed by instruction (!PS-I). Below, we separately discuss pooled effect size estimates, results of subgroup analyses and empirical evidence for publication bias for comparison of PS-I with +PS-I and !PS-I. Note that current work does not (and cannot) say anything about how PS-I fares relative to I-PS.

4.1 Comparing PS-I with Scaffolded Problem-Solving followed by Instruction

4.1.1 Pooled Effect Size Estimates (RQ1)

Results for RQ1 suggest that overall, the SMD or effect size was small and not significant (Hedge's $g = -0.08$, $p = 0.20$, 95% CI $[-0.20, 0.04]$). The heterogeneity between comparisons was low and not significant ($Q(df = 59) = 42.09$, $p = 0.95$). The standardized mean difference (SMD), effect size, and the 95% confidence intervals (lower CI, upper CI) of each comparison can be found in supplementary materials. Results from a complementary Bayesian multilevel meta-analysis model [57] with weakly informative priors for true pooled effect size ($\mu \sim N(0, 1)$) and between-study heterogeneity ($\tau \sim HC(0, 0.5)$) aligned with these frequentist pooled effect size estimates – Hedge's $g = -0.03$, 95% CI $[-0.21, 0.16]$. Based on recommendations in [57], 4000 iterations of the Markov Chain Monte Carlo-based NUTS sampling procedure were used for the Bayesian meta-analysis. The empirical cumulative distribution function of the posterior distribution for the effect size suggested the probability of the SMD being < 0.2 : 99%, < 0.3 : 99.91%, < 0.4 : 100%.

4.1.2 Subgroup Analysis (RQ2)

Subgroups accounting for heterogeneity in the overall effect size are shown in Table 3. Results for RQ2 suggest that none of the PF fidelity criteria and students' demographic and/or incoming characteristics split the data into subgroups with significant differences. Further, the type of scaffolding was the only intervention characteristic with significant subgroup differences. The combination of cognitive and metacognitive support together seemed to

significantly disfavor +PS-I (although low number of comparisons ($n = 2$) resulted in very wide CIs). Neither age range (as assessed by students' *grade level*) and the *nature of pretesting*, nor their interaction (exploring whether the pretesting effects may be confounded by age) split the comparison data into subgroups with significant differences.

The *overall PF fidelity score* (computed as a percentage) did not significantly predict the effect size ($\beta = -0.0014$, $p = 0.14$, 95% CI $[-0.0032, 0.0005]$). However, investigating differences in PF fidelity across grade levels revealed a significant interaction of overall PF fidelity score (computed as a percentage) with the subset of comparisons involving *6th-10th graders* ($\beta = 0.0319$, $p = 0.0018$, 95% CI $[0.0125, 0.0513]$) and those involving *undergraduates* ($\beta = 0.0324$, $p = 0.0038$, 95% CI $[0.0109, 0.0538]$). These estimates suggest that the relationship between these two grade levels and the estimated effect size favored PS-I with increase in PS-I's fidelity to PF design criteria.

Next, we found that longer interventions had a higher overall PF fidelity score. A Welch two-sample t-test ($t(30.06) = 5.00$, $p < 0.0001$) revealed significant differences in overall PF fidelity score, with the mean of interventions spanning *a few days* being 70.15 ($SD = 12.82$) and those spanning *a few hours* being 49.87 ($SD = 15.38$). This observation led us to explore the potential confound of length of the intervention (instead of the PF fidelity) being responsible for explaining the observed effects. We ran a meta-regression model to explicitly incorporate the interaction of intervention time span with the overall PF fidelity score. Results suggested that despite an overall increase in effect size with increase in overall PF fidelity ($\beta = 0.0037$, $p = 0.5196$, 95% CI $[-0.0078, 0.0153]$), there was in fact a marginally significant decrease for interventions spanning *a few hours* ($\beta = -0.0158$, $p = 0.0835$, 95% CI $[-0.0337, -0.0022]$). This suggests that cramming too many design features into an intervention within a short time may not be optimal.

The overall PF fidelity (computed as a percentage) was also significantly higher in *quasi-experimental* comparisons ($M = 73.65$, $SD = 18.52$) compared to *experimental* comparisons ($M = 60.42$, $SD = 14.62$), as revealed by a Welch two-sample t-test ($t(20.14) = 2.52$, $p = 0.0204$). A plausible hypothesis is that lower manipulation flexibility in experimental studies might result in them having low overall PF fidelity, which could contribute to the descriptively (although, not significant) lower effect size. An alternative hypothesis contributing to this descriptive trend might stem from time constraints – experimental comparisons might be constrained by time, and a shorter duration (consequently, lesser time on task for students) might be responsible for relatively lower effect sizes. However, in the included comparisons, neither intervention type and time span nor explicitly modeling their interaction with the PF fidelity score resulted in significant subgroup differences.

4.1.3 Publication Bias (RQ3)

The funnel plot visualization to detect if small studies with small effect size were missing is shown in Figure 3. The absence of asymmetry is visually evident. Egger's test of asymmetry was also not significant ($\beta = 0.04$, $p = 0.9522$). This first source of publication bias could therefore be ruled out. The p-curve analysis as shown in Figure 4, however revealed interesting results. Visually, the p-curve looked skewed to the right. The estimated power of the comparisons included in the analysis, which is useful both in designing future studies and in interpreting existing results [58, 59], was high (86%, CI [66.7%, 95%]). Numerically, the test for right-skewness was significant (meaning the p-curve for was heavily right skewed), and test for flatness was not significant. Based on results of the right-skewness and the flatness test, we could therefore conclude that evidential value was present in the p-curve. The estimate of true effect size (in absence of publication bias) was computed to be Hedge's $g = 0.55$. With a low ($< 50\%$) level of heterogeneity in our comparisons, we expect these p-curve estimates to be robust [60]. Taken together, results for RQ3 show a robust evidence for publication bias.

4.2 Comparing PS-I with Alternative Preparatory Activity followed by Instruction

4.2.1 Pooled Effect Size Estimates (RQ1)

Results for RQ1 suggest that overall, the SMD was moderate and not significant (Hedge's $g = 0.22$, $p = 0.12$, 95% CI [-0.06, 0.51]). The heterogeneity between comparisons was moderate and significant ($Q(df = 57) = 110.03$, $p < 0.0001$). The standardized mean difference (SMD), effect size, and the 95% confidence intervals (lower CI, upper CI) of each comparison can be found in supplementary materials. Results from a complementary Bayesian multilevel meta-analysis model [57] with weakly informative priors for true pooled effect size ($\mu \sim N(0, 1)$) and between-study heterogeneity ($\tau \sim HC(0, 0.5)$) aligned with these frequentist pooled effect size estimates – Hedge's $g = 0.20$, 95% CI [-0.07, 0.51]. Based on recommendations in [57], 4000 iterations of the Markov Chain Monte Carlo-based NUTS sampling procedure were used for the Bayesian meta-analysis. The empirical cumulative distribution function of the posterior distribution for the effect size suggested the probability of the SMD being < 0.2 : 50.3%, < 0.3 : 76.17%, < 0.4 : 90.67%.

4.2.2 Subgroup Analysis (RQ2)

Subgroups accounting for heterogeneity in the overall effect size are shown in Table 4. Results for RQ2 suggest that the three PF fidelity criteria of *groupwork*, *social surround facilitation during the problem-solving phase* and *instruction building on student solutions* were associated with significant or marginally significant subgroup differences disfavoring PS-I. This means that effect sizes favoring PS-I were relatively higher when the PS-I design did not implement these particular PF fidelity criteria. None of students' demographic and/or incoming characteristics split the data into subgroups with significant differences. However, *learning concepts targeted* (psychology) and the *type of preparatory activity* (text comprehension) were two intervention characteristics that showed significant subgroup differences favoring PS-I. The low number of corresponding comparisons for both these subgroups ($n = 3$) warrant taking these effect sizes with caution. Further, experimental

comparisons showed relatively higher effect size favoring PS-I, although the subgroup difference for intervention *type* was only marginally significant.

Neither age range (as assessed by students' *grade level*) and the *nature of pretesting*, nor their interaction (exploring whether the pretesting effects may be confounded by age) split the comparison data into subgroups with significant differences. Further, neither the *overall PF fidelity score* nor its interaction with age range (as assessed by students' *grade level*) revealed significant effects.

We also found that shorter interventions had a higher overall PF fidelity score. A Welch two-sample t-test ($t(38.29) = 3.862, p < 0.0001$) revealed significant differences in overall PF fidelity score, with the mean of interventions spanning *a few hours* being 62.47 ($SD = 20.26$) and those spanning *a few days* being 38.48 ($SD = 21.15$). This observation led us to explore the potential confound of length of the intervention (instead of the PF fidelity) as an explanatory basis for the observed effects. A meta-regression model, where we explicitly incorporated the interaction of intervention time span with the overall PF fidelity score, revealed a significant interaction effect. Similar to +PS-I comparisons, results for !PS-I comparisons suggested that despite the overall increase in effect size with increase in overall PF fidelity ($\beta = 0.0062, p = 0.3584, 95\% \text{ CI } [-0.0073, 0.0197]$), there was a marginally significant decrease for interventions spanning *a few hours* ($\beta = -0.0252, p = 0.0386, 95\% \text{ CI } [-0.0489, -0.0014]$).

Finally, with respect to intervention type, we found *experimental* comparisons to more likely span *a few hours* ($\chi^2(1) = 4.62, p = 0.0315$) as well descriptively have lower overall PF fidelity score ($M = 43.68, SD = 19.12$) compared to *quasi-experimental* comparisons ($M = 51.46, SD = 28.16$). Taken together with the low impact of shorter intervention time span and PF fidelity criteria (cf. Table 4), these convergent interaction trends may have strengthened net effects favoring PS-I for experimental comparisons, albeit only marginally significantly.

4.2.3 Publication Bias (RQ3)

The funnel plot visualization to detect if small studies (small sample size) with small effect size were missing is shown in Figure 5. The absence of asymmetry is visually evident. Egger's test of asymmetry was also not significant ($\beta = 0.84$, $p = 0.5191$). This first source of publication bias could therefore be ruled out. The p-curve, as shown in Figure 6, however looked skewed to the right. The estimated power of the comparisons in the analysis was high (81%, CI [68.6%, 89.8%]). Numerically, the test for right-skewness was significant (meaning the p-curve for was heavily right skewed), and test for flatness was not significant. Based on results of the right-skewness and the flatness test, we could therefore conclude that evidential value was present in the p-curve. The estimate of true effect size (in absence of publication bias) was computed to be Hedge's $g = 0.64$. With a slightly over 50% heterogeneity ($I^2 = 51.44\%$) in our comparisons, we expect these p-curve estimates to be not very robust [60]. Taken together, results for RQ3 show a non-robust evidence for publication bias.

5. General Discussion and Implications

Overall, our results showed a small and non-significant difference between PS-I and +PS-I (Hedge's g -0.08, 95% CI [-0.20, 0.04]). A complementary Bayesian analysis showed 99% probability of the true overall effect size being smaller than 0.2, which strongly suggests odds favoring the null hypothesis of no meaningful difference between PS-I and +PS-I. After accounting for publication bias, true effect size estimates were moderate and still in favor of PS-I (Hedge's g 0.55). With the low number of comparisons between PS-I and +PS-I ($n = 60$), the only significant result that we found via subgroup analyses was for the type of scaffolding, with combined cognitive and metacognitive support disfavoring +PS-I. Looking more deeply into the comparisons included in our meta-analysis, this begs the question of why might scaffolding preparatory problem-solving towards success via explicit cognitive support [16, 17, 61, 62] or metacognitive support [27, 28, 37, 63, 64, 65] be largely unsuccessful? We

found that the presence or absence of different PF fidelity criteria within PS-I did not make a significant difference in explaining the pattern of results.

Although we cannot say for certain that PF fidelity criteria are unrelated/not necessary (because of non-significant subgroup differences), an alternative conjecture is that other tradeoffs stemming from the design of scaffolding (as discussed in the introductory background section) itself might be responsible for the observed null effects. Taken together, consideration of these tradeoffs warrants taking the non-significant difference between PS-I and +PS-I experimental comparisons with caution. More research is needed to investigate if and whether alternative forms of scaffolding might benefit preparatory problem-solving, e.g., explicitly towards failure rather than success by asking students to explicitly generate suboptimal RSMs [20, 66]. We see problematizing, which was descriptively associated with higher effect sizes favoring +PS-I relative to structuring (cf. Table 3), as an important direction that future work on scaffolding PS-I might take.

In comparisons with !PS-I, results of our meta-analysis showed a moderate but non-significant effect (Hedge's g 0.22 [95% CI -0.06, 0.51]) favoring PS-I. A complementary Bayesian analysis showed 40.37% probability of the true overall effect size (favoring PS-I) to be in moderate range of 0.2 to 0.4. After accounting for publication bias, true effect size estimates were moderate and still in favor of PS-I (Hedge's g 0.64). Surprisingly, although the presence of certain PF fidelity criteria (e.g., problems affording multiple RSMs, evidence for multiple RSM generation, affective draw of the problem and instructional social surround facilitation) descriptively benefited PS-I, presence of the remaining fidelity criteria in fact hurt PS-I (cf. Table 4). This variability in the impact of PF design criteria on learning suggests that they may not be reliable predictors of PS-I efficacy for comparisons involving !PS-I. On the one hand, this could be because PF fidelity criteria such as groupwork and instruction building on student solutions can be also be plausibly implemented within !PS-I designs and trigger

similar learning mechanisms, thereby nullifying their differential impact. On the other hand, there could be different PS-I design criteria not coded in the current meta-analysis that trigger relevant learning mechanisms similar to those triggered by the design affordances of PS-I (e.g., prior knowledge activation, raising knowledge gap awareness, critical feature identification).

Taken together, our meta-analysis holds important implications for teaching and learning. For educators who wish to implement scaffolding during preparatory problem-solving, our results provide concrete empirical evidence to suggest that it may be worthwhile not to short-circuit student thinking and reasoning processes early on. Allowing students to freely explore well-designed problems (following, for e.g., design fidelity criteria of PF) in the absence of explicit cognitive or metacognitive scaffolding might be sufficient to trigger preparatory benefits. Our results also imply that educators need to carefully consider when might it be fruitful to implement alternative sensemaking activities prior to formal instruction. A 40.37% probability of obtaining at least a moderate effect size favoring PS-I over alternative forms of sensemaking prior to instruction (e.g., worked examples, evaluating pre-designed solutions etc) suggests that on average, it may be the first design choice. More work in the field is needed before generalizing these claims to specific student subgroups.

6. Limitations and Future Work

To the best of our knowledge, this meta-analysis provides the most comprehensive integrative review of the relative efficacy of preparatory problem-solving and alternative sensemaking approaches to date. We acknowledge that the choice of the included subgroup categories and their dichotomous coding only reflect a balance between factors that are consistently reported across different comparisons, and those that can be reliably codified.

Several other subgroup categories, which we could not code, might be responsible for the current pattern of effects. For instance, we did not have any information about individual differences that might differentially benefit students' learning via problem-solving or

alternative preparatory approaches. Students' learning goal orientation, effort regulation, self-esteem and attitude towards mistakes are few examples of inter-individual factors that have been empirically found to be associated with differential learning outcomes within PS-I studies [20, 67, 68]. Teacher characteristics (e.g., knowledge of the subject-area, classroom management skills) and parent characteristics (e.g., failure mindsets) might further influence these patterns of results [69, 70]. Finally, the complexity of the presented learning materials, often proxied via element interactivity, may also be a confounding factor. However, "the problem with the concept of element interactivity is that it is not defined in any measurable way, and consequently, cognitive load and overload are not defined in such a way either. Besides, although element interactivity is recognised as a key factor in cognitive load, it is rarely clear how element interactivity is manipulated in experiments inspired by CLT" [71, p. 2]. Despite accounting for students' grade level in the current meta-analysis (that can be taken as a weak proxy for expertise), our results are inconclusive regarding whether adding information to problems via scaffolding (cf. Table 3) benefits novices but is detrimental for experts. At least descriptively, these results comparing PS-I with +PS-I show that scaffolding benefits adult learners too, although with a small effect size. Taken together, a critical consideration of these factors along with consistent reporting will afford studying alternative explanations for our results.

Methodologically, although meta-analysis offers a valuable tool, it comes with certain limitations. For instance, subgroup analysis when comparing PS-I with +PS-I and !PS-I to determine which participants most benefit from the treatment based on student and intervention characteristics was often based on a small number of comparisons. Although reporting information on subgroups is a standard procedure in meta-analysis work [57], this may have limited the ability of such analysis to detect meaningful differences because of insufficient statistical power. Consequently, non-significant subgroup differences should be cautiously

interpreted – for categories with no difference in effect sizes between the subgroups (cf. Table 3 and Table 4), this does not automatically mean that the subgroups produce equivalent outcomes.. The Bayesian methods that we used to directly model the uncertainty in estimating between-study heterogeneity and pooled effects are therefore particularly useful.

7. Conclusion

In conclusion, our meta-analysis brings robust and generalizable empirical evidence to bear on the relative efficacy of different preparatory approaches for future learning [1]. We clearly established that both scaffolding PS-I (+PS-I) and implementing alternative forms of preparation prior to instruction (!PS-I) do not provide any differential advantage over and above preparatory problem-solving (PS-I). Although the pooled effects found using traditional frequentist meta-analysis approaches were non-significant, Bayesian analyses in fact strongly favored the null hypothesis for the comparison of PS-I with +PS-I (suggesting a 99% probability of the difference in effect between these designs being less than 0.2), while it suggested a 40.37% probability of at least moderate effect favoring PS-I relative to !PS-I. Furthermore, after considering evidence regarding publication bias, effects moderately favored PS-I for comparisons with both +PS-I and !PS-I. Taken together with our former meta-analysis results regarding the efficacy of PS-I over I-PS [8], and results from the current meta-analysis about PS-I not compromising conceptual understanding and transfer (to say the least), we advance the field by providing solid empirical evidence, in the context of existing literature, for using preparatory problem-solving as a powerful sensemaking activity to prepare students for future learning.

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Table 1*Descriptive Characteristics of Articles Included in the Review (n = 118 Comparisons)*

	Subcategories	# of Comparisons (%)
1. Geographical distribution	Europe (Germany, UK, Israel)	19 (16.1%)
	North America (USA, Canada)	77 (65.3%)
	Asia (Singapore, India)	20 (16.9%)
	Australia	2 (1.7%)
2. Learner grade / age range	2nd - 5th graders	24 (20.3%)
	6th - 10th graders	65 (55.1%)
	Undergraduates	28 (23.7%)
	Postgraduates	1 (0.9%)
3. Targeted concepts	Math (geometry, fractions, variance, correlations, central tendencies, least squares fitting, weighted averages, crypt-arithmetic, equivalence)	76 (64.4%)
	Basic Sciences (Physics, Chemistry, Biology)	35 (29.7%)
	Physics (density, electricity, magnetism, mechanics, buoyancy)	30 (25.4%)
	Chemistry (atomic structure)	2 (1.7%)
	Biology (genetics)	3 (2.5%)
	Domain general skill (Rubik's cube)	2 (1.7%)
	Others (Psychology (memory), management theory, learning strategy assessment)	5 (4.2%)

Table 2*Examples of PS-I Designs with Varying Design Fidelity to PF*

	Roelle & Berthold, 2016 [23]	Kim et al., 2015 [55]	Roll et al., 2011 [42]	Newman & DeCaro, 2019 (study 1 and study 3) [54]
Problems affording multiple RSMs	yes	yes	yes	yes
Evidence for multiple RSM generation	yes	yes	no	no
Affective draw of the problem	yes	yes	yes	yes
Groupwork as the participation structure	no	yes	yes	no
Instruction building on student solutions	no	no	yes	no
Social surround facilitation (Problem-solving phase)	low	high	low	—
Social surround facilitation (Instruction phase)	Monologue-dominant	Monologue-dominant	Monologue-dominant	Monologue-dominant
Overall PF fidelity score (computed as a percentage)	42.86%	71.43%	57.14%	50%
Comparison Condition	+PS-I	+PS-I	!PS-I (evaluating predesigned solutions)	!PS-I (worked example)
Hedge's <i>g</i> (effect size) [95% Confidence Interval]	0.3 [-0.81, 1.4]	-0.29 [-1.33, 0.74]	1.33 [0.37, 2.29]	study 1 / 3: -0.54 / -0.61 [-1.47, 0.39 / -1.57, 0.35]
Grade level	6th-10th graders	6th-10th graders	6th-10th graders	Undergraduates
Geographical distribution	Europe	Asia	North America	North America
Nature of pretesting	Targeted	Targeted	Targeted	Targeted
Intervention type	Experimental	Quasi-experimental	Experimental	Experimental
Intervention time span	A few hours	A few days	A few days	A few hours
Learning concept	Chemistry	Biology	Math	Math
Learning outcome	Conceptual Understanding	Clubbed outcomes	Transfer	Clubbed outcomes

Table 3

Subgroup Analysis for $n = 60$ Comparisons Between PS-I and +PS-I. Positive (negative) effect sizes depict results favoring PS-I (+PS-I). For categories with >2 subgroups, significance refers to comparison of a particular sub-category with rest of the sub-categories.

	Subgroups (significant differences, if any, marked)	Hedge's g [95% CI]
PF Fidelity Criteria	Problems affording multiple RSMs [NA]	Yes (-0.08 [-0.20, 0.04]) No (-)
	Evidence for multiple RSM generation [n.s.]	Yes (-0.11 [-0.70, 0.1]) No (0.19 [-0.17, 0.55])
	Affective draw of the problem [n.s.]	Yes (-0.02 [-0.19, 0.46]) No (-0.16 [-0.44, 0.12])
	Groupwork as the participation structure [n.s.]	Yes (-0.07 [-0.32, 0.29]) No (-0.05 [-0.23, 0.12])
	Instruction building on student solutions [n.s.]	Yes (-0.06 [-0.34, 0.33]) No (-0.06 [-0.23, 0.11])
	Social surround facilitation [n.s.] (Problem-solving phase)	High (-0.28 [-0.97, 0.20]) Low (0.10 [-0.20, 0.40])
	Social surround facilitation [n.s.] (Instruction phase)	Dialogue-dominant (-0.06 [-0.32, 0.32]) Monologue-dominant (-0.06 [-0.22, 0.10])
Demographic and/or Incoming Characteristics	Age range (grade level) [n.s., n.s., n.s.]	2nd-5th graders (0.00 [-0.24, 0.41]) 6th-10th graders (-0.09 [-0.35, 0.26]) Undergraduates (-0.08 [-0.34, 0.28])
	Geographical distribution [n.s., n.s., n.s.]	Europe (0.09 [-0.09, 0.52]) North America (-0.13 [-0.44, 0.10]) Asia (-0.07 [-0.44, 0.45])
	Nature of pretesting [n.s.]	Targeted (-0.06 [-0.38, 0.43]) None (-0.08 [-0.46, 0.29])
Intervention Characteristics	Type [n.s.]	Quasi-experimental (0.00 [-0.18, 0.39]) Experimental (-0.10 [-0.26, 0.06])
	Time span [n.s.]	A few days (-0.10 [-0.39, 0.18]) A few hours (0.00 [-0.22, 0.23])
	Learning concepts targeted [n.s., n.s., n.s., n.s., n.s.]	Math (0.02 [-0.15, 0.34]) Physics (-0.08 [-0.33, 0.25]) Chemistry (-0.20 [-0.96, 0.69]) Biology (-0.24 [-0.8, 0.44]) Domain-general skills (-0.55 [-1.36, 0.38])
	Learning outcomes assessed [n.s., n.s., n.s.]	Conceptual (-0.03 [-0.17, 0.36]) Transfer (-0.12 [-0.35, 0.22]) Clubbed (-0.15 [-0.51, 0.33])
	Type of scaffolding [n.s., n.s., ***, n.s.]	Structuring (Cognitive) (-0.10 [-0.36, 0.20]) Structuring (Metacognitive) (-0.19 [-0.53, 0.22]) Structuring (Mixed) (1.00 [0.50, 1.75]) Problematizing (-0.22 [-0.62, 0.30])

Table 4

Subgroup Analysis for $n = 58$ Comparisons Between PS-I and !PS-I. Positive (negative)

effect sizes depict results favoring PS-I (!PS-I). For categories with >2 subgroups,

significance refers to comparison of a particular sub-category with rest of the sub-categories.

	Subgroups (significant differences, if any, marked)	Hedge's g [95% CI]
PF Fidelity Criteria	Problems affording multiple RSMs [n.s.]	Yes (0.24 [-1.10, 1.50]) No (0.04 [-1.22, 1.31])
	Evidence for multiple RSM generation [n.s.]	Yes (0.37 [-0.37, 0.81]) No (0.15 [-0.22, 0.53])
	Affective draw of the problem [n.s.]	Yes (0.31 [-1.09, 1.62]) No (0.04 [-1.26, 1.35])
	Groupwork as the participation structure [*]	Yes (0.00 [-0.97, 0.18]) No (0.40 [0.02, 0.78])
	Instruction building on student solutions [+]	Yes (0.09 [-0.83, 0.38]) No (0.31 [-0.06, 0.68])
	Social surround facilitation [*] (Problem-solving phase)	High (-0.57 [-1.59, 1.17]) Low (0.31 [0.00, 0.62])
	Social surround facilitation [n.s.] (Instruction phase)	Dialogue-dominant (0.37 [-0.54, 0.88]) Monologue-dominant (0.20 [-0.14, 0.54])
Demographic and/or Incoming Characteristics	Age range (grade level) [n.s., n.s., n.s.]	6th-10th graders (0.14 [-1.00, 0.28]) Undergraduates (0.36 [-0.53, 0.86]) Postgraduates (1.22 [-0.36, 2.37])
	Geographical distribution [n.s., n.s., n.s., n.s.]	Europe (0.38 [-0.56, 0.92]) North America (0.21 [-0.64, 0.57]) Asia (0.24 [-0.69, 0.60]) Australia (0.04 [-1.50, 1.10])
	Nature of pretesting [n.s., n.s., n.s.]	Targeted (0.22 [-0.59, 0.50]) None (0.29 [-0.48, 0.65]) Prerequisite (0.04 [-1.50, 1.10])
Intervention Characteristics	Type [+]	Quasi-experimental (0.07 [-0.86, 0.40]) Experimental (0.30 [-0.05, 0.65])
	Time span [n.s.]	A few days (0.36 [-0.30, 0.85]) A few hours (0.08 [-0.33, 0.50])
	Learning concepts targeted [n.s., n.s., *, n.s.]	Math (0.08 [-1.00, 0.15]) Physics (0.09 [-0.85, 0.47]) Psychology (1.53 [0.27, 2.5]) Others (0.95 [-0.27, 1.80])
	Learning outcomes assessed [n.s., n.s., n.s.]	Conceptual (0.43 [-0.14, 0.74]) Transfer (0.13 [-0.66, 0.21]) Clubbed (0.05 [-1.00, 0.57])
	Type of preparatory activity [n.s., n.s., n.s., ***, n.s., n.s.]	Worked example (0.08 [-0.85, 0.38]) Evaluating pre-designed solutions (0.31 [-0.52, 0.74]) Ranking contrasting cases (0.04 [-1.25, 0.81]) Text comprehension (1.53 [0.27, 2.52]) Pre-instruction (0.17 [-1.39, 1.25]) Problem posing (0.00 [-1.39, 0.87])

Figure 1

PRISMA Flowchart Summarizing the Overall Process for Selecting Studies for Inclusion.

Exemplar Categories of Articles Excluded From the Meta-analysis can be Found in Supplementary Materials.

Figure 2

Venn Diagram Illustrating the Hierarchy of PF, PS-I and PFL Learning Designs

Note: Here, we depict one category of preparation for Future Learning (PFL) designs where sensemaking experiences precede *instruction*. However, more broadly, PFL can be conceived as any experience that prepares students to learn in the future – that learning could occur not just through explicit instruction but also through exploration, practice, etc.

Figure 3*Funnel Plot to Assess Publication Bias for PS-I and +PS-I Comparisons*

Note. Funnel plots colored by the significance level into which the effect size of each of the $n = 60$ PS-I and +PS-I comparisons fall. Bubbles in the plot depict comparisons included in the review. The Y-axis shows the standard error (SE) of each comparison, with larger comparisons (which have a smaller SE) plotted on top of the Y-axis. The X-axis shows the standardized effect size of each comparison. Striped vertical line corresponds to the pooled effect size estimate. Color coding - dark blue (dark gray): $p < 0.05$, blue (gray): $p < 0.025$, light blue (light gray): $p < 0.01$.

Figure 4

P-curve to Assess Publication Bias for PS-I and +PS-I Comparisons

Note. P-curve for $n = 60$ PS-I and +PS-I comparisons is shown with a blue/solid line. The Y-axis shows the percentage of comparisons. The X-axis shows the p-values.

Figure 5*Funnel Plot to Assess Publication Bias for PS-I and !PS-I Comparisons*

Note. Funnel plots colored by the significance level into which the effect size of each of the $n = 58$ PS-I and !PS-I comparisons fall. Bubbles in the plot depict comparisons included in the review. The Y-axis shows the standard error (SE) of each comparison, with larger comparisons (which thus have a smaller SE) plotted on top of the Y-axis. The X-axis shows the standardized effect size of each comparison. Striped vertical line corresponds to the pooled effect size estimate. Color coding - dark blue (dark gray): $p < 0.05$, blue (gray): $p < 0.025$, light blue (light gray): $p < 0.01$.

Figure 6

P-curve to Assess Publication Bias for PS-I and !PS-I Comparisons

Note. P-curve for $n = 58$ PS-I and !PS-I comparisons is shown with a blue/solid line. The Y-axis shows the percentage of comparisons. The X-axis shows the p-values.