Does Discovery-Based Instruction Enhance Learning?

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Abstract

Discovery learning approaches to education have recently come under scrutiny (Tobias & Duffy, 2009) with many studies indicating limitations to discovery learning practices. Therefore, two meta-analyses were conducted using a sample of 164 studies: The first examined the effects of unassisted discovery learning versus explicit instruction and the second examined the effects of enhanced and/or assisted discovery versus other types of instruction (e.g., explicit, unassisted discovery, etc.). Random effects analyses of 580 comparisons revealed that outcomes were favorable for explicit instruction when compared to unassisted discovery under most conditions, d = -.38 (95% CI = -.44/-.31). In contrast, analyses of 360 comparisons revealed that outcomes were favorable for enhanced discovery when compared to other forms of instruction, d = .30 (95% CI = .23/.36). The findings suggest that unassisted discovery does not benefit learners, whereas feedback, worked examples, scaffolding, and elicited explanations do.

Keywords: discovery learning, explicit instruction, scaffolding

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> …the average student will be unable to recall most of the factual content of a typical lecture within fifteen minutes after the end of class. In contrast, interests, values, and cognitive skills are all likely to last longer, as are concepts and knowledge that students have acquired not by passively reading or listening to lectures but through their own mental efforts (Bok, 2006, pp. 48-49).

Over the past several decades, conventional explicit instruction has been increasingly supplanted by approaches more closely aligned with constructivist concepts of exploration, discovery, and invention (i.e., discovery learning), at least in part because of an appreciation of which learning outcomes are most valuable (Bok, 2006). Allowing learners to interact with materials, manipulate variables, explore phenomena, and attempt to apply principles affords them with opportunities to notice patterns, discover underlying causalities, and learn in ways that are seemingly more robust. Such self-guided learning approaches, like Piaget (1952; 1965; 1980) proposed, posit the child/learner at the center of the learning process as they attempt to make sense of the world. From an ecological perspective, people learn many complex skills without formal instruction through participation in daily activities and observation of others (Rogoff, 1990). Indeed, in cultures without institutionalized formal education, complex skills and modes of thought are learned in the absence of explicit, verbal teaching. Nonetheless, debate remains concerning the limitations of discovery learning (e.g., Bruner, 1961; Kirschner, Sweller, & Clark, 2006; Klahr & Nigam, 2004; Mayer, 2004; Sweller, Kirschner, & Clark, 2007; Tobias & Duffy, 2009). Pedagogical and cognitive concerns have led to some disagreement as to what constitutes effective discovery learning methods and how and when such methods should be applied. Two recent review papers (Kirschner, Sweller, & Clark, 2006; Mayer, 2004) have outlined some of the problems associated with various discovery-based instructional methods;

however, no systematic meta-analysis has been conducted on this literature. For instance, it is unclear whether or not the process of how to discover information on one's own needs to be taught to learners (e.g., Ausubel, 1964; Bruner, 1961), to what extent discovery tasks should be structured (Mayer, 2004), which types of tasks are within the realm of discovery methods (Klahr & Nigam, 2004), and whether the working memory demands of discovery-learning situations jeopardize the efficacy of the instruction (Kirschner et al., 2006). The current meta-analyses evaluate these concerns.

A Definition of Discovery Learning

Before proceeding, it is necessary to reflect on the wide range of instructional conditions that have been included under the rubric of discovery learning. Because methods employing discovery learning involve a wide variety of intended accomplishments during the acquisition of the target content, a definition of *discovery learning* is needed. However, there is a myriad of discovery-based learning approaches presented within the literature without a precise definition (Klahr & Nigam, 2004). Learning tasks considered to be within the realm of discovery learning range from implicit pattern detection (e.g., Destrebecqz, 2004; Jimenez, Mendez, & Cleeremans, 1996) to the elicitation of explanations (e.g., Chi, de Leeuw, Chiu, & La Vancher, 1994; Rittle-Johnson, 2006), and from working through manuals (e.g., Lazonder & VanderMeij, 1993) to conducting simulations (e.g., Stark, Gruber, Renkl, & Mandl, 1998). What exactly constitutes a discovery-learning situation is seemingly yet undetermined by the field as a whole. At times, the discovery condition seems less influenced by the learning methods and more by the comparison methods. That is, when a comparison group has received some greater amount of explicit instruction, whatever the type or degree, investigators often refer to the other group as a discovery group because it has been assisted less during the learning process.

A review of the literature suggests that discovery learning occurs whenever the learner is not provided with the target information or conceptual understanding and must find it independently and with only the provided materials. Within discovery-learning methods, there is an opportunity to provide the learners with intensive, or conversely, minimal guidance and both types can take many forms (e.g., manuals, simulations, feedback, example problems). The extent to which the learner is provided with assistance seems to be contingent upon the difficulty in discovering the target information with less assistance, and also on the instructional methodologies to which it is being compared. Common to all of the literature however, is that the target information must be discovered by the learner within the confines of the task and its material.

Concerns and Warnings about Discovery Learning

As early as the 1950s, research had begun to investigate the effects of discovery learning methods in comparison to other forms of instruction. Bruner (1961) and others (e.g., Ausubel, 1964; Ballew, 1967; Craig, 1965; Guthrie, 1967; Kagan, 1966; Kendler, 1966; Kersh, 1958, 1962; Ray, 1961; Scandura, 1964; Wittrock, 1963; Worthen, 1968) advocated learning situations that elicited explanations or self-guided comprehension from learners and provided opportunities for learners to gain insights into their domains of study. Bruner (1961) emphasized that such discovery-based learning could enhance the entire learning experience while also cautioning that such discovery could not be made a priori or without at least some base of knowledge in the domain in question. While Bruner's (1961) article has often been cited as support for discovery learning, many have seemingly ignored his warnings (i.e., the limitations of such an approach to instruction).

Recently, Mayer (2004) argued that pure, unassisted discovery-learning practices should be abandoned because of a lack of evidence that such practices improve learning outcomes. Through a review of the literature, he illustrated that unassisted discovery-learning tasks did not help learners discover problem-solving rules, conservation strategies, or programming concepts. Mayer emphasized that although constructivist-based approaches might be beneficial to learning under some circumstances, unassisted discovery learning does not seem advantageous because of its lack of structure. He further emphasized that unassisted discovery-learning tasks involving hands-on activities, even with large group discussions, do not guarantee that learners will understand the task or that they will come into contact with the to-be-learned material.

Furthermore, Klahr (2009) and others (e.g., Clark, 2009; Mayer, 2009; Rosenshine, 2009; Sweller, 2009) have emphasized that there are times when more explicit instruction or at least directive guidance is optimal. Although Klahr’s concerns were in teaching the control of variables strategy (CVS), his arguments regarding instructional times, feedback, instructional sequences, and generalization of skills, emphasize that in certain situations some amount of direct instruction is advantageous. In the case of CVS, Klahr argues that learners might have difficulty arriving at the proper strategy of holding all other variables constant while manipulating only one. He explains that such scientific problem solving, while commonplace to cognitive scientists who have a great understanding of the cognitive processes involved in such a task, might not arise simply by asking novice learners to figure out how to use the provided materials. Even if such a strategy is reached and implemented by learners, it might require a great deal of time, which could have been saved through direct teaching of the CVS strategy. Klahr suggests that perhaps it would be more time efficient to instruct learners directly on how to implement CVS and then give them ample opportunities to practice it. Moreover, direct instruction in CVS learning tasks might be necessary because the manipulation of the materials alone does not provide sufficient feedback; learners are not presented with any indication of shortcomings in their strategies if they fail to manipulate only one variable at a time. By explicitly teaching learners about the cognitive processes involved in problem solving and the ways in which scientists go about uncovering causal factors, Klahr argues that learners will be empowered to use these skills and that their understandings can be strengthened by activities that afford them with opportunities to practice these skills in a domain of interest and consequently, to discover knowledge in that domain by doing so.

Similarly, Sweller, Kirschner, and Clark (2007) have emphasized the usefulness of worked examples over other forms of instruction. They suggest that instructors should provide a complete problem solution for learners to study and practice for themselves. They argue that such a learning technique would be superior to less guided forms of instruction because of the limited capacity of working memory. Although that claim will be addressed in a subsequent section, it is noteworthy that the encouragement to use worked examples is similar to Klahr’s (2009) suggestion to demonstrate CVS to learners and then to provide them with opportunities for practice.

Direct Instruction and Construction

The example of teaching CVS directly, as described by Klahr (2009), illustrates the variability of what is meant by direct instruction. Klahr is not suggesting lecture-type instructional situations. Instead he suggests some degree of guidance as to what learners should expect as evidence of successful learning and then giving them opportunities to practice using such skills on their own. This suggestion is not unique to Klahr but has been raised by a number of researchers on both sides of the debate (e.g., Clark, 2009; Herman & Gomez, 2009; Kintsch, 2009; Pea, 2004; Rosenshine, 2009; Sweller, Kirschner, & Clark, 2007; Wise & O'Neill, 2009). While Klahr's arguments might not be appropriate in all domains or for all learning tasks, his suggestions to employ direct instruction as a basis for subsequent discovery addresses some of the concerns that discovery-learning tasks lack structure and therefore, overwhelm the learner's cognitive workspace.

Note also that Klahr does not position direct instruction in opposition to constructivism in that he asserts that learners should be provided with opportunities to manipulate materials directly. In a way, Klahr might be helping to unite constructivism and more direct forms of instruction by emphasizing that sometimes, as in the case of CVS, direct instruction will facilitate constructivist learning by reducing task ambiguities and learning times, while improving process comprehension and potential generalization. More generally, Klahr's suggestions to provide some amount of direct instruction might reduce the cognitive demands of discovery tasks by familiarizing learners with the processes involved, as will be discussed below.

Cognitive Factors

At the most basic level, memory is enhanced when learning materials are generated by the learner in some way; this is commonly referred to as the generation effect (Slamecka & Graf, 1978). The robust effect is that materials generated or even merely completed by learners are remembered more often and/or in greater detail than materials provided by an instructor. This effect is often presented as evidence that discovery learning is efficacious because such learning involves the discovery and generation of general principles or explanations of domain-specific patterns after discovering such on one's own (e.g., Chi, de Leeuw, Chiu, & LaVancher, 1994; Crowley & Siegler, 1999; Schwartz & Bransford, 1998). Therefore, the expectation is that discovery-based approaches, because of the requirement that learners construct their own understandings and consequently the content, should yield greater learning, comprehension, and/or retention. Note, however, that the majority of tasks used in the generation effect are simple (e.g., recalling a word) unlike much of the research on discovery learning, which involves more involved tasks such as CVS.

Cognitive load theory and concerns. With regard to the cognitive processes involved in discovery learning, Mayer (2003) emphasized that discovery-based pedagogy works best in promoting meaningful learning when the learner strives to make sense of the presented materials by selecting relevant incoming information, organizing it into a coherent structure, and integrating it with other organized knowledge. However, to select, organize, and integrate high-level information in a task-appropriate way is quite demanding of learners. Both Sweller (1988) and Rittle-Johnson (2006) have emphasized that because discovery learning relies on an extensive search through problem-solving space, the process taxes learners' limited working-memory capacity and frequently does not lead to learning. In addition, learners need the ability to monitor their own processes of attention to relevant information (Case, 1998; Kirschner, Sweller, & Clark, 2006). This would seem to require learners to have considerable metacognitive skills, and it is unlikely that all learners, in particular children, would have such skills (Dewey, 1910; Flavell, 2000; Kuhn & Dean, 2004). Thus, learning by discovery seems to require a greater number of mental operations, as well as better executive control of attention, in comparison to learning under a more directive approach. Furthermore, cognitive load theory suggests that the exploration of complex phenomena or learning domains imposes heavy loads on working memory detrimental to learning (Chandler & Sweller, 1991; Kirschner, Sweller, & Clark, 2006; Paas, Renkl, & Sweller, 2003; Sweller, 1988; 1994).

Predictions. The cognitive demands involved in discovery-based pedagogies make them seem daunting and implicate a number of predictions. For example, young learners (i.e., children) might be least likely to benefit from such methods (Case, 1998; Kirshner, Sweller, & Clark, 2006, Mayer, 2004) compared to their older counterparts. Younger learners would have comparatively limited amounts of organized, preexisting knowledge and schemas to be able to integrate new information effectively. Children have more limited working memory capacities (Kirschner, Sweller, & Clark, 2006) and experiences in using the cognitive processes outlined by Mayer (2004) and others. Furthermore, they lack the metacognitive skills required to monitor their cognitive processes (Flavell, 2000; Kuhn & Dean, 2004).

Issues of Guidance within the Debate between Constructivist Instruction and Explicit Instruction

Of course constructivism does not assert that all learning should be unaided (Hmelo-Silver, Duncan, & Chinn, 2007; Schmidt, Loyens, van Gog, & Paas, 2007; Spiro & DeSchryver, 2009). Nonetheless, while guidance has been an important component of instruction on both sides of the debate concerning constructivist instruction (Tobias & Duffy, 2009), there remains a remarkable number of discovery-based instructional tasks that are largely unassisted. As Duffy (2009) explains, explicit instruction advocates seemingly intend for their students to reach their learning objectives in the most efficient ways possible, whereas constructivism advocates emphasize learners’ motivation, and tend to provide guidance or feedback only when learners prompt it through inquiry.

An illustration of these different standpoints can be found in the correspondence of Fletcher (2009) with Schwartz, Lindgren, and Lewis (2009) in which he claims that more direct forms of instruction work better when learners have little prior knowledge. In response, Schwartz et al. provide the example of children having to learn to tie their shoes without having ever seen a shoe before. They argue that in such a case, hands-on exploration would be optimal so that the children could familiarize themselves with the layout of the shoe, its laces, etc. However, because these children have never seen a shoe before, one might argue just the opposite: to understand the utility of having shoes tied, children should be provided explicitly with the task objective and a means for achieving the goal.

Because their intentions and learning objectives are different (Schwartz, Lindgren, & Lewis, 2009), the ways in which the explicit instruction and constructivism camps understand learning situations are different (Duffy, 2009; Kuhn, 2007). However, both camps have tended to include some forms of guidance within instructional designs (Tobias & Duffy, 2009) and it is the intention of the current analyses to determine which types of enhancement are best. Enhanced-discovery methods include a number of techniques from feedback to scaffolding (Rosenshine, 2009), and many studies have been conducted that employ different forms and degrees of guidance during learning tasks.

We conducted two meta-analyses because of the ambiguity within the literature as to what constitutes a discovery-learning method and how and when such methods should be applied. The first meta-analysis compared unassisted discovery-learning methods (e.g., teaching oneself, completing practice problems, conducting simulations) to more explicit instruction. The second meta-analysis compared enhanced discovery-learning methods (e.g., guided discovery, elicited self-explanation) to a variety of instructional conditions including unassisted discovery as well as explicit instruction.

Method

Literature Search

Articles examining different types of discovery learning were identified through a variety of sources. The majority of the articles were identified using *PsychInfo*, *ERIC*, and *Dissertations Abstracts International* computerized literature searches. Studies were also identified from citations in articles. The selection criteria for the first meta-analysis was that studies had to test directly for differences between an explicit training or instruction condition (explicit) and a condition in which unassisted discovery learning occurred, which was operationally defined as being provided with no guidance or feedback during the learning task. The selection criteria for the second meta-analysis was that the study included a condition in which discovery learning was operationally defined as being provided with guidance in the learning task, along with a comparison condition. In other words, the first meta-analysis evaluated the effects of unassisted discovery-learning conditions versus explicit instruction, whereas the second meta-analysis evaluated the effects of guided or enhanced discovery-learning conditions versus other forms of instruction.

Exclusion criteria precluded the use of several potentially relevant studies. First, articles with unclear statistical information or those which were based on only qualitative data alone were not included.[1] However, before discarding any articles, authors were contacted for information that could be included in the meta-analysis. Second, articles needed to include comparable conditions that consistently differed in the type of instruction. Those comparing conditions that were fundamentally different or that were equivocated prior to testing could not be included.

Units of Analysis and Data Sets

As the unit of analysis, group samples of studies and comparisons were considered separately. *Studies* as a unit of analysis referred to individual experiments with different participants. *Studies*, thus, treats multiple experiments reported within a single article as separate studies if they involved different participants. *Comparisons* were also used as a unit of analysis. Analysis at the level of *comparisons* refers to counting each individual statistical comparison as an independent contribution. Articles that run many comparisons have more weight in the overall computation of the effect than those that run fewer. Because many potentially moderating variables differ between comparisons, only one moderator (i.e., publication rank) could be tested using studies as the unit of analysis. All other moderators were analyzed at the level of comparisons. While multiple comparisons reported for a single sample violate assumptions of independence, analysis at this level was required to test for effects of moderating variables.

Variables Coded from Studies as Possible Moderators for the Meta-analyses

Six moderators were used for blocking purposes in both meta-analyses. See Table 1 for the complete listing of the categories of each moderator. Publication rank was the first moderator to be considered. Studies from top-ranked journals were compared with studies from other

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[1] Because we did not want to perform simply a sign test, we did not include articles that did not provide useable statistical information.

sources. Top-ranked journals included any journal with an impact factor greater than 1.5 based on the 2001 listings of impact factors. All other journal publications that ranked below 1.5 were coded as second-tier journal articles. Studies published in book chapters were coded separately and studies included in dissertations or unpublished works (e.g., conference poster presentations) were coded separately. Although impact factors have increased in the intervening years, the rank ordering of journals has changed very little.

Second, the domains of the studies were considered. The following domains were coded for: 1) math/numbers 2) computer skills 3) science 4) problem solving 5) physical/motor skills and 6) verbal/social skills. Next, the ages of participants were coded. Participants were considered children if they were 12 years-old or younger, adolescents if they were between 13 and 17 years-old, and adults if they were 18 years-old or older. If the same statistical test included a range of ages, the mean age of the sample was used for coding purposes. If the exact ages were not provided but their grade levels were, participants were coded as children through sixth grade, as adolescents from seventh to twelfth grades, and as adults thereafter.

The dependent variable was the next moderator considered. *Post-tests* were assessments administered after the learning phases. These scores included a variety of assessment types from pure post-test scores to improvement scores with previous assessments used as baseline measures on tasks ranging from error detection/correction to content recall, depending on the domain in question. *Acquisition scores* included measurements of learning, success, or failed attempts/errors during the learning phases. *Reaction time scores* reflect the amount of time employed to arrive at the target answer. *Self-ratings* included ratings by learners of their own motivation levels, competencies, or other aspects of the learning tasks. *Peer ratings* included ratings by observing peers or other learners in regard to the learners’ competencies or other aspects of the learning tasks. *Mental effort* reflected scores determined by the experimenters who calculated mental load reflective of the amount of information being considered, the number of variables to be manipulated, the number of possible solutions, etc. that learners had to manage to complete the task successfully.

The fifth moderator to be considered was the type of discovery learning condition employed. The types of discovery learning for the first meta-analysis, comparing explicit to unassisted discovery learning conditions, included the following: unassisted, invention, matched probes, simulation, and work with a naïve peer. The *unassisted* conditions included the learner’s investigation or manipulation of relevant materials without guidance, the learners teaching themselves through trial-and-error or some other means, and/or the learners attempting practice problems. The *invention* conditions included tasks that required learners to invent their own strategies or design their own experiments. The *matched probes* conditions included hints in the form of probe questions, which were asked of learners in both the unassisted-discovery conditions and explicit instruction conditions. The *simulation* conditions included computer-generated simulations that required learners to manipulate components or engage in some type of practice to foster comprehension. The *work with a naïve peer* conditions were those that paired learners with novice or equal learning partners.

The types of discovery learning for the second meta-analysis were considered to be enhanced forms of discovery learning methods and included generation, elicited explanations, and guided discovery conditions. *Generation* conditions required learners to generate rules, strategies, images, or answers to experimenters’ questions. *Elicited explanation* conditions required that learners explain some aspect of the target task or target material, either to themselves or to the experimenters. The *guided discovery* conditions involved either some form of instructional guidance (i.e., scaffolding) or regular feedback to assist the learner at each stage of the learning tasks.

Lastly, the type of comparison condition was investigated. *Direct teaching* conditions included the explicit teaching of strategies, procedures, concepts, or rules in the form of formal lectures, models, demonstrations, etc. and/or structured problem solving. *Feedback* conditions took priority over other coding and included any instructional design in which experimenters responded to learners' progress to provide hints, cues, or objectives. Conditions of *worked examples* included provided solutions to problems similar to the targets. *Baseline* conditions included designs in which learners were not given the basic instructions available to the discovery group, learners were asked to complete an unrelated task that required as much time as the discovery group's intervention, or learners were asked to complete pre- and post-tests only with a time interval matched to the discovery group's. The *explanations provided* conditions were those in which explanations were provided to learners about the target material or the goal task. *Other* conditions included conditions (i.e., one comparison in the analysis of unassisted discovery and two comparisons in the analysis of enhanced discovery) that were largely experiment-specific in that the condition could not fairly be categorized as any other code because the instructional change involved only a minimal change in design.

Comparison conditions for the second meta-analysis included all of the above except for *feedback* conditions. Also, the *baseline* conditions for the second meta-analysis differed slightly in that such conditions in the second meta-analysis more often involved designs in which learners were asked to teach themselves either through physical manipulations or through textbook learning (i.e., similar to the unassisted-discovery conditions of the first meta-analysis), and designs in which only pre- and post-tests were administered with interceding time intervals matched to the discovery group.

Reliability on Moderators

Coding for moderators was accomplished with recommendations from the four authors who decided on moderator codes to include the range of conditions, completely and yet concisely. Reliability on all moderators for both meta-analyses was found to be consistently high leading to an overall kappa of 0.87. All disagreements were resolved through a discussion of how best to classify the variable in question both within the context of the study and the purposes of analysis.

Computation and Analysis of Effect Sizes

Given the great variety of discovery learning designs and the variety of undetermined factors involved in any potential effects, a random effects model was used in all analyses in the *Comprehensive Meta-analysis, Version 2* (CMA) program (Borenstein, Hedges, Higgins, & Rothstein, 2005). A random effects model is appropriate when participant samples and intervention factors cannot be presumed to be functionally equivalent. Consequently, effect sizes cannot be presumed to share a common effect size because they may differ because of any one or a number of different factors between studies. However, the current meta-analyses report overall results from both fixed and random effects models and then present subsequent results only from the random effects model.

Effect sizes. Computation formulae included within the CMA program allowed for direct entry of group statistics in order to calculate effect sizes for each test-by-test comparison. When the only statistics available were *F*-values and group means, DSTAT (Johnson, 1993) allowed us to convert those statistics to a common metric, *g*, which represents the difference in standard deviation units. More specifically, *g* is computed by calculating the difference of the two means divided by the pooled standard deviation of the two samples (e.g., the difference between two groups' mean reaction times, divided by the pooled standard deviation). Those *g* scores and other group statistics were then entered into the CMA program. For analyses at the level of studies, overall *g* statistics were calculated in DSTAT before entry into the CMA program.

Because *g*-values may "overestimate the population effect size" when samples are small (Johnson, 1993, p. 19), *Cohen's d* values are reported here as calculated by the CMA program. *Cohen's d*s between .20 and .50 indicate a small effect size, *Cohen's d*s between .50 and .80 indicate a medium effect, and *d*s greater than .80 indicate a large effect (Cohen, 1988). Of course, the effect size alone does not determine significance and we determined the significance of effect sizes based on the *p*-values of the resultant *Z*-scores.

Post-hoc Comparisons

After grouping the effect sizes by a particular moderator and finding significant heterogeneity among different levels of the same moderator, each level was compared to all others within the CMA program, indicated by *Q*, to determine if the effect sizes between the groups were significantly different from one another. Post hoc *p*-values were adjusted for the number of comparisons conducted. For example, post-hoc comparisons of the domain categories required 15 comparisons and consequently led to a set alpha level of .003 for levels to be considered significantly different from one another.

Results

The effect sizes comparing discovery conditions to other forms of instruction were analyzed in four separate meta-analyses, two at the level of studies and two at the level of comparisons. Table 2 displays the results overall for each of the meta-analyses and includes results for both fixed and random effects models. Effects sizes were coded so that a negative effect size indicates that participants in the compared instructional conditions evidenced greater learning than participants in discovery conditions, whereas a positive effect size indicates that participants in the discovery conditions evidenced greater learning than participants in the compared instructional conditions. Moreover, even the effect sizes for the dependent measures of *reaction times* and *mental effort/load* were coded so that scores higher in number reflected poorer performances and thus, negative effect sizes for those dependent measures reflect the superiority of the comparison conditions.

Moderators

An advantage of quantitative meta-analytic techniques is the ability to examine potential moderators of relations with ample statistical power. In the present meta-analyses, the following potential moderators were investigated: publication rank, domain, age of participants, dependent variable, type of discovery condition, and type of compared instructional condition. Whenever heterogeneity of variance was indicated (Johnson, 1989), moderators were tested for each of the meta-analyses. Post hoc p values were used to determine statistical significance. All moderators for both meta-analyses were examined using statistical comparisons as the unit of analysis, assuming independence, except for publication rank, which was examined at the level of studies.

Unassisted Discovery

Overall Effects

A total of 580 comparisons from 108 studies compared unassisted discovery learning with more explicit teaching methods. Table 3 lists each sample. With the random effects analysis, the 108 studies had a mean effect size of d = -.38 (95% CI = -.50/-.25), indicating that explicit teaching was more beneficial to learning than unassisted discovery. This constitutes a small but meaningful effect size ($p < .001$). The effects are highly heterogeneous across the studies, Q (107) = 522.11, $p < .001$. Such heterogeneity is to be expected given the diversity of research methods, participant samples, and learning tasks. To address issues of publication bias, failsafe Ns were calculated both at the level of comparisons and at the level of studies with alphas set to .05, two-tailed. At the level of comparisons, 3,588 unpublished studies and at the level of studies, 3,551 unpublished studies would be needed to reduce these effects to nonsignificance.

Moderators

First, using studies as the unit of analysis, the type of publication moderated the findings, Q (3) = 10.86, $p < .05$. Articles in first-tier journals (d = -.67) evidenced larger effect sizes in favor of explicit instruction than did articles in second-tier publications (d = -.24). Post-hoc comparisons revealed that these mean effect sizes were significantly different from one another, Q (1) = 10.20, $p < .008$. Effect sizes from book chapters (d = -.12) and unpublished works (d = -.01) did not reach significance.

The domain was also found to moderate effect sizes, Q (5) = 91.75, $p < .001$. As Table 4 shows that in the domains of math (d = -.16), science (d = -.39), problem solving (d = -.48), and verbal and social skills, ($d = -.95$) participants evidenced less learning in the unassisted-discovery conditions than in the explicit conditions. Post-hoc comparisons indicated that the mean effect size favoring explicit conditions within the verbal/social skills domain was significantly greater than within the domains of math, $Q (1) = 50.03$, $p < .001$, computer skills, $Q (1) = 58.17$, $p < .001$, science, $Q (1) = 22.65$, $p < .001$, problem solving, $Q (1) = 18.35$, $p < .001$, and physical/motor skills, $Q (1) = 14.87$, $p < .001$. The mean effect size favoring explicit conditions within the domain of problem solving was also significantly greater than within the domains of math, $Q (1) = 13.65$, $p < .001$, and computer skills $Q (1) = 28.29$, $p < .001$. Lastly, the mean effect size favoring explicit conditions in the domain of science was significantly greater than within the domain of computer skills, $Q (1) = 16.64$, $p < .001$.

The next moderator investigated was participant age, which also moderated the findings, $Q (2) = 12.29$, $p < .01$. Table 5 displays the effect sizes by the age group of the participants. As can be seen, effect sizes for all age groups showed significant advantages for more explicit instruction over unassisted discovery. Post-hoc comparisons revealed that the mean effect size for adolescents ($d = -.53$) was significantly greater than the mean effect size for adults ($d = -.26$), $Q (1) = 10.41$, $p = .001$. The type of dependent variable was also found to moderate the findings, $Q (5) = 37.38$, $p < .001$. Measures of post-test scores ($d = -.35$), acquisition scores ($d = -.95$), and time to solution ($d = -.21$) favored participants in explicit conditions, as can be seen in Table 6. Post-hoc comparisons indicated that the measure of acquisition scores led to significantly greater effect sizes in favor of explicit conditions than did the measures of post-test scores, $Q (1) = 31.41$, $p < .001$, time to solution, $Q (1) = 23.84$, $p < .001$, and self-ratings $Q (1) = 15.89$, $p < .001$.

The type of unassisted-discovery condition moderated the findings, Q (4) = 10.02, $p <$ .05, but post-hoc comparisons failed to reveal any reliable differences. Table 7 displays that all levels of unassisted-discovery conditions except for matched probes somewhat favored participants in the explicit conditions. Next, we investigated the explicit conditions to which unassisted-discovery conditions were compared. The type of explicit condition moderated the findings, Q (5) = 32.31, $p < .001$. Participants in unassisted discovery fared worse than participants in comparison conditions of direct teaching (d = -.29), feedback (d = -.46), worked examples (d = -.63), and explanations provided (d = -.28). Table 8 provides more information regarding these comparisons. Post-hoc comparisons revealed that effect sizes for direct teaching and worked examples were significantly different from one another, Q (1) = 18.98, $p < .001$, and indicated that participants learning with worked examples outperformed participants learning through unassisted discovery to a greater extent than did participants learning from direct teaching outperform participants learning from unassisted discovery. Post-hoc comparisons also revealed that feedback, Q (1) = 9.15, $p < .003$, and worked examples, Q (1) = 13.70, $p < .001$, benefitted learners more than having no exposure with pre- and post-tests only.

Overall, the findings indicate that explicit instructional conditions lead to greater learning than do unassisted-discovery conditions. The lack of significant differences between the mean effect sizes of the unassisted-discovery conditions helps to illustrate that claim.

Enhanced Discovery

Overall Effects

A total of 360 comparisons from 56 studies compared enhanced discovery learning (i.e., generation, elicited explanation, or guided discovery) with other types of instructional methods.

Table 9 lists each sample. With the random effects analysis, the 56 studies had a mean effect size of d = .30 (95% CI = .15/.44), indicating that enhanced-discovery methods led to greater learning than did comparison methods of instruction. This constitutes a small but meaningful effect size ($p < .001$). The effects are highly heterogeneous across the studies, Q (55) = 260.14, $p < .001$. Again, such heterogeneity is to be expected given the diversity of research methods, participant samples, and learning tasks. To address issues of publication bias, failsafe *N*s were calculated both at the level of comparisons and at the level of studies with alphas set to .05, two-tailed. At the level of comparisons, 4,138 unpublished studies and at the level of studies, 960 unpublished studies would be needed to reduce effects to nonsignificance.

Moderators

First, using studies as the unit of analysis, the type of publication moderated the findings, Q (2) = 18.66, p = .001. Articles in first-tier journals (d = .35) and second-tier journals (d = .40) generally favored enhanced-discovery conditions, whereas datasets from unpublished studies and dissertations did not (d = -.54). Post-hoc comparisons revealed that while the effect sizes derived from first-tier and second-tier journal articles were not significantly different, Q (1) = .10, *ns*, the mean effect size from unpublished works and dissertations differed from both the mean effect size from first-tier journals, Q (1) = 9.65, $p < .003$, and the mean effect size from second-tier journals, Q (1) = 21.59, $p < .001$.

Domain was also found to moderate the findings, Q (5) = 65.53, $p < .001$. As can be seen in Table 10, in the domains of math (d = .29), computer skills (d = .64), science (d = .11), physical/motor (d = 1.05), and verbal and social skills (d = .58), participants evidenced more learning in the enhanced-discovery conditions than in the comparison conditions. Post-hoc comparisons indicated that the mean effect size in the physical/motor domain was significantly greater than the effect sizes in the domains of math, Q (1) = 34.59, $p < .001$, science, Q (1) = 41.67, $p < .001$, and problem solving, Q (1) = 15.73, $p < .001$. Also, the mean effect size for the domain of computer skills was significantly greater than the effect sizes in the domains of math, Q (1) = 12.14, $p < .001$ and science, Q (1) = 18.65, $p < .001$.

The next moderator, participant age, also influenced the findings, Q (2) = 10.68, $p < .01$. Table 11 displays the effect sizes by the age group of the participants. Post-hoc comparisons revealed that the mean effect size for adults was significantly greater than the effect size for children, Q (1) = 7.64, $p < .01$. Although superficially there was a greater difference between the mean effect sizes of adults and adolescents, that difference was not found to be significant due to the larger variance within the adolescents (95% CI = .04/.33). Next, the type of dependent variable was found to moderate the findings, Q (4) = 64.60, $p < .001$. Measures of post-test scores ($d = .28$), acquisition scores ($d = .54$), and self-ratings ($d = 1.25$) favored participants in enhanced-discovery conditions over participants in comparison conditions, whereas measures of reaction times ($d = -.72$) favored participants in comparison conditions over participants in enhanced-discovery conditions. See Table 12. Post-hoc comparisons indicated that the measure of post-test scores led to significantly greater effect sizes in favor of participants in enhanced-discovery conditions than did the measure of self-ratings, Q (1) = 29.68, $p < .001$. Comparisons also indicated that the mean effect size derived from reaction time measures was significantly different (i.e., significantly opposite in effect size direction) from both the mean effect size derived from acquisition scores, Q (1) = 10.19, $p = .001$, and the mean effect size derived from post-tests, Q (1) = 31.61, $p < .001$. Lastly, the mean effect size for self-ratings which favored enhanced discovery was found to be significantly different (i.e., opposite to) the mean effect size for mental effort/load which showed trends favoring other forms of instruction.

The type of enhanced-discovery condition used also moderated the findings, *Q* (2) = 65.00, $p < .001$. Table 13 shows that elicited explanation ($d = .36$) and guided discovery ($d = .50$) favored enhanced discovery whereas generation ($d = -.15$) favored other instructional methods. Post-hoc comparisons indicated that indeed, generation conditions were significantly different in their effect sizes to both elicited explanation, *Q* (1) = 33.20, $p < .001$, and guided discovery, *Q* (1) = 57.43, $p < .001$, but the effect sizes for elicited explanation and guided discovery did not differ from one another. Next, we investigated the instructional conditions to which enhanced-discovery conditions were compared but the type of comparison condition failed to moderate the findings, *Q* (4) = 9.12, $p = .06$, *n.s.* As shown in Table 14, with the exception of worked examples ($d = .06$, *n.s.*), all other comparisons conditions indicated significantly superior performances in the enhanced-discovery conditions.

Overall, results seemed to favor enhanced-discovery methods over other forms of instruction. However, the dependent measure and the type of enhanced discovery employed affected the outcome assessments.

Discussion

The first meta-analysis was intended to investigate under which conditions unassisted discovery learning might lead to better learning outcomes than explicit-instructional tasks. However, more explicit-instructional tasks were found to be superior to unassisted-discovery tasks. Moreover the type of publication, the domain of study, the age of participants, the dependent measure, the type of unassisted-discovery task, and the comparison condition all moderated outcomes. Post-hoc comparisons revealed that on average, publications in first-tier journals showed greater benefits for explicit-instructional tasks than did publications in second-tier journals. Among the variety of different domains in which more explicit instruction was found to benefit learners, verbal and social learning tasks seemed to favor explicit instruction most, followed by problem solving and science. Adolescents were found to benefit significantly more from explicit instruction than did adults. Analysis of dependent measures indicated that learners' acquisition scores showed a greater detriment under discovery conditions than did post-test scores, time to solution, and self-ratings. Although the type of unassisted-discovery task moderated trends favoring explicit instruction, unassisted tasks, tasks requiring invention, and tasks involving collaboration with a naïve peer were all found to be equally detrimental to learning. Analyses of the types of explicit instruction in the comparison conditions indicated that worked examples benefited learners more than direct teaching and also indicated that feedback and providing explanations are useful aids to learning. The finding that worked examples evidenced greater learning than did unassisted discovery is expected given the worked-example effect (Sweller, Kirschner, & Clark, 2007). However, the finding that worked examples benefitted learners to a greater extent than did direct teaching was unexpected.

The second meta-analysis investigated under which conditions enhanced forms of discovery-learning tasks might be beneficial. This meta-analysis showed better learning for enhanced-discovery instructional methods, with the type of publication, the domain, the age of participants, the dependent measure, and the type of enhanced-discovery task moderating the findings. Unpublished studies and dissertations were found to show disadvantages for enhanced-discovery conditions whereas first and second-tier journal articles favored enhanced discovery. Of the different task domains, physical/motor[2], computer skills, and verbal and social skills

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[2] Because of concerns that the domain category of physical/motor skills might be dominating the overall analysis of enhanced discovery, those 24 comparisons were removed and analyses were

benefited most from enhanced discovery. Also, analyses revealed that adult participants benefit more from enhanced discovery than children. Of the three types of enhanced discovery, the generation method of enhanced discovery failed to produce learning benefits over other instructional methods, which was unexpected given the typical benefits reported as the generation effect (Bertsch, Pesta, Wiscott, & McDaniel, 2007; Slamecka & Graf, 1978). It should be noted that the advantage of other forms of instruction over generation also led to the finding that unpublished studies and dissertations showed an advantage for other forms of instruction over enhanced discovery. This was due to the fact that four out of the five studies sampled from unpublished works or dissertations employed generation conditions. Although the meta-analysis indicated that the type of comparison condition did not moderate the results, note that enhanced discovery was generally better than both direct teaching and explanations provided. Thus, the construction of explanations or participation in guided discovery is better for learners than being provided with an explanation or explicitly taught how to succeed on a task, in support of constructivist claims. In regard to the large mean effect size for the category of comparison conditions labeled *other*, it should be noted that this category included only two comparisons; these two comparisons[3] were included to ensure a complete inclusion of comparison conditions, despite the fact that they did not fit into the other categories. Lastly,

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run again. The removal of physical/motor skills from the overall analyses under the random effects model only reduced the mean effect size slightly [i.e., from (d = .30) to (d = .25)]. Consequently, we retained the category of physical/motor skills within our analyses.

[3] The participants in the first *other* comparison condition were asked the same questions that were asked of the elicited explanations group but the elicited explanations condition required participants to provide a specific target answer before proceeding to the next question, and the comparison condition did not. The participants in the second *other* comparison condition were asked to discuss how/why things balance on a beam within a group without input from the experimenter, and were compared to participants who were asked to explain to the experimenter who guided the learner with subsequent questions toward the target explanation.

analysis of the dependent measure indicated that while learners’ post-test and acquisition scores benefited from enhanced-discovery tasks, reaction times did not. This suggests that learners may take more time to find problem solutions or perform target responses when engaged in enhanced-discovery tasks.

The moderating effect of age across the two meta-analyses did not follow the expected pattern of results. First, the adolescent age group was shown to benefit least from unassisted-discovery conditions, as opposed to the children, as had been predicted. While enhanced-discovery conditions led to better learning outcomes for all age groups, adults seemed to benefit from enhanced-discovery tasks more so than children. Interestingly, the adolescents tended to benefit least and the adults tended to benefit most from both unassisted-discovery tasks and enhanced-discovery tasks. One might speculate that the negative trend among adolescents could reflect a general lack of motivation or lack of domain-relevant knowledge (Mayer, 2009). However, if the trend was the result of a lack of domain-relevant knowledge, one might expect to see even larger deficits in children. With regards to the adults, perhaps their greater domain-relevant knowledge helped them to succeed on unassisted-discovery tasks to a greater extent than the adolescents. It is also possible that the tasks used in the enhanced-discovery studies were more appropriate for adult learners (e.g., having participants explain the strategies they were using to solve problems) than for young learners. Organizing guidance to facilitate discovery requires sensitivity to the learner’s zone of proximal development (Vygotsky, 1962; Pea, 2004) if it is to be maximally useful.

Implications for Teaching

The results of the first meta-analysis indicate that unassisted discovery generally does not benefit learning. Although direct teaching is better than unassisted discovery, providing learners with worked examples or timely feedback is preferable. Whereas providing well-timed, individualized feedback to all learners might be impossible (e.g., in a classroom setting), providing such feedback on homework assignments seems possible and worthwhile. Students might also benefit from having worked examples provided on those homework assignments, when the content allows for it. Furthermore, the second meta-analysis suggests that teaching practices should employ scaffolded tasks that have support in place as learners attempt to reach some objective, and/or activities that require learners to explain their own ideas. The benefits of feedback, worked examples, scaffolding, and elicited explanation can be understood to be part of a more general need for learners to be redirected, to some extent, when they are mis-constructing. Feedback, scaffolding, and elicited explanations do so in more obvious ways through an interaction with the instructor, but worked examples help lead learners through problem sets in their entireties and perhaps help to promote accurate constructions as a result. Although our suggestions are conservative as to how to apply the current findings, we suspect and hope that these analyses will be influential in subsequent designs, both instructional and empirical.

Theoretical Implications

Perhaps the inferior outcomes of unassisted-discovery tasks should not be surprising; Hake (2004) referred to such methods as *extreme* modes of discovery and pointed out that methods with almost no teacher guidance will, of course, be inferior to more guided methods. It does not seem that many researchers on either side of the argument would disagree with such a claim (Tobias & Duffy, 2009). Nonetheless, it seems that many of Mayer’s (2004) concerns are justified. Unassisted-discovery tasks appear inferior to more instructionally guided tasks, whether explicit instruction or enhanced discovery. Mayer’s concern that unassisted-discovery tasks do not lead learners to construct accurate understandings of the problem set illustrates the potential disconnect between activity and constructivist learning. As Mayer points out, it has been the accepted practice to consider hands-on activities as equivalent to constructivism but active instructional methods do not always lead to active learning, nor do passive methods always lead to passive learning (Mayer, 2009).

Recently, Chi (2009) outlined the theoretical and behavioral differences between learning tasks that require the learner to be active and learning tasks that require the learner to be constructive, and emphasized that the two are not one in the same. Although a meta-analysis of Chi’s claims would be optimal to support her outline, she nonetheless has provided tentative explanations that are useful fodder and seemingly in agreement to some extent with the points of Mayer (2004). She explained that although activities requiring hands-on active participation from learners guarantee a level of engagement greater than passive reception of information, these activities do not guarantee that learners will be engaged to the extent necessary to make sense of the materials for themselves. From Chi’s perspective, learning activities entailing true constructivism should require learners not only to engage in the learning task (e.g., manipulate objects or paraphrase) but also to construct ideas that surpass the presented information (e.g., to elaborate, predict, reflect). Chi’s emphasis that constructivism should require learners to achieve these *higher-order objectives* - similar to those outlined by Fletcher (2009) that include analysis, evaluative abilities, and creativity - illustrates that the objectives of constructivism are at least in part, present within the learning activity itself.

Perhaps the completely unguided discovery activities objected to by Mayer (2004) were too ambiguous to allow learners to transcend the mere activity and reach the level of constructivism intended. Through more guided tasks, the learner is liberated potentially from high demands on working memory and executive functioning abilities (Chi, 2009; Kirschner, Sweller, & Clark, 2006; Mayer, 2003; Rittle-Johnson, 2006; Sweller, 1988; Sweller, Kirschner, & Clark, 2007) and can therefore direct his/her efforts toward more creative processes (e.g., inference, integration, and reorganization) as outlined by both Chi (2009) and Fletcher (2009). Our finding that generation is not an optimal form of enhanced discovery may illustrate this claim. The generation conditions required learners to generate rules, strategies, or images, or to answer questions about the information but there was little consistency in the extent to which learners had to go beyond the presented information to do so. Of the three types of enhanced discovery, generation required the least engagement of learners with respect to the types of activities that Chi identified as constructive.

The finding that enhanced forms of discovery are superior to unassisted forms also calls into question ecological perspectives of learning inherent within discovery pedagogy and perhaps constructivism more generally. While it seems reasonable to expect learners to be able to construct their own understandings with minimal assistance because they do so on a daily basis in the context of everyday activities, perhaps the content and context of formal education are extraordinary (Geary, 2008) and consequently require more assistance to arrive at accurate constructions, understandings, and solutions (Sweller, Kirschner, & Clark, 2007). It is also possible that people often learn what they do within daily life activities through forms of guided participation (Rogoff, 1990).

The Potential of Teaching Discovery

In light of the previous discussion of Mayer (2004) and Chi (2009), we should return to the possibility that it might serve educators and students alike to spend time learning the procedures of discovery (Ausubel, 1964; Bielaczyc, Pirolli, & Brown, 1995; Bruer, 1993; Dewey, 1910; Karpov & Haywood, 1998; King, 1991; Kozulin, 1995; Kuhn, Black, Keselman, & Kaplan, 2000). Teaching learners first to be discoverers (e.g., how to navigate the problem solving space, use limited working memory capacities efficiently, and attend to relevant information) could *prepare* them (Bruner, 1961) for active learning demands as outlined by Chi (2009), and perhaps provide some of the needed curricular focus and necessary structure to discovery tasks as emphasized by Mayer (2004). Furthermore, by having learners better familiarized with the processes of discovery, the cognitive load demands (Kirschner, Sweller, & Clark, 2006; Rittle-Johnson, 2006; Sweller, 1988) might be reduced. Consequently, this might allow learners to engage with the learning tasks not only in active ways, but also constructively (i.e., in the ways outlined by Chi, 2009) to allow them to go beyond the presented information. Bruner (1961, pp. 26) emphasized that discovery encourages learners to be constructivists and that practice in discovering teaches the learner how best to acquire information to make it more readily available. Again, Bruner implied that the act of discovering is one that requires practice to be of value.

Bruner also warned that the learner's mind has to be prepared for discovery. The preparation that Bruner emphasized was not merely an existing knowledge base regarding the domain of study; he also emphasized that learning by discovery does not necessarily involve the acquisition of new information. Bruner claimed that discovery was more often the result of a learner gaining insights that transform their knowledge base through new ways of organizing the previously learned information. Furthermore, the prepared mind for Bruner was one with experience in discovery itself.

> It goes without saying that, left to himself, the child will go about discovering things for himself within limits. It also goes without saying that there are certain forms of child rearing, certain home atmospheres that lead some children to be their own discoverers more than other children (pp. 22).

Bruner (1961), like Vygotsky (1962), suggested that the narrative of teaching is a conversation that is appropriated by the learner who can subsequently use that narrative to teach himself/herself. Bruner emphasized that opportunities for discovery might facilitate this process. Consequently, it seems reasonable to conclude that discovery might itself be a scripted tool (i.e., a narrative) for making sense of materials on one's own (Arievitch & Stetsenko, 2000; Kozulin, 1995; Stetsenko & Arievitch, 2002; Wertsch, 1981). The steps and procedures of that script are not innate to the learner but need to be presented by teachers, or parents as emphasized by Bruner, because they are part of a culture (e.g., the culture of formal education). Thus, if learning through discovery is superior to other forms of instruction, then it might serve educators and students alike to spend time learning the procedures of discovery (Ausubel, 1964; Bielaczyc, Pirolli, & Brown, 1995; Bruer, 1993; Dewey, 1910; Karpov & Haywood, 1998; King, 1991; Kozulin, 1995; Kuhn, Black, Keselman, & Kaplan, 2000). Generally, teaching the procedures of discovery to learners might provide some of the needed curricular focus and necessary structure to discovery instructional methods (concerns raised by Mayer, 2004). It might also reduce the cognitive demands of discovery learning tasks and make such methods more easily employed (a concern raised by Kirschner et al., 2006; Sweller, Kirschner, & Clark, 2007).

Although we have suggested teaching learners how to discover, we do not mean to imply that we have arrived at some oversimplified strategy for discovery that can bridge all domains or learning tasks. On the contrary, directly instructing learners on problem solving skills, analogies, and other cognitive processes should not be expected to lead learners to generalize those skills to all other areas of learning (Klahr, 2009; Sweller, Kirschner, & Clark, 2007; Wise & O'Neill, 2009). However, providing ample opportunities for learners to discover when and where those processes are appropriate, could lead learners to such discovery-based constructivism only after those processes have been taught directly within the contexts of their appropriate domains.

More generally, teaching students how be constructive learners might begin with more basic preparation. Perhaps many learners are not prepared for such activities and that educational reform needs to focus first at the level of reading comprehension, to teach students how to make sense of new information (Herman & Gomez, 2009) because domain-relevant information might be essential for successful construction of novel understandings during instruction, particularly in ill-structured domains (Rosenshine, 2009; Spiro & DeSchryver, 2009). Herman and Gomez have outlined several *reading support tools* (p. 70) designed to help students understand science texts in meaningful and useful ways. Although these tools need first to be taught explicitly, they could provide self-guidance while reading science texts thereafter. Perhaps similar reading support tools need to be developed for other texts as well so that students can come to view textbooks as helpful resources within their environments that they are able to interact with in meaningful ways to reach objectives, the definition of learning as proposed by Gresalfi and Lester (2009). These tools could establish foundations for learning that might not be readily generalizable from the moment that they are mastered but can be after practice, experience in different contexts, and in the presence of scaffolding and feedback (Wise & O'Neill, 2009).

Conclusion

Overall, the effects of unassisted-discovery tasks seem limited, whereas enhanced-discovery tasks requiring learners to be actively engaged and constructive seem optimal. Based on the current analyses, optimal approaches should include at least one of the following: 1) guided tasks that have scaffolding in place to assist learners, 2) tasks requiring learners to explain their own ideas and ensuring that these ideas are accurate by providing timely feedback, or 3) tasks that provide worked examples of how to succeed in the task. Opportunities for constructive learning might not present themselves when learners are left unassisted. Perhaps the findings of these meta-analyses can help to move the debate away from issues of unassisted forms of discovery and towards a fruitful discussion and consequent empirical investigations of how scaffolding is best implemented, how to provide feedback in classroom settings, how to create worked examples for varieties of content, and when during the learning task direct forms of instruction should be provided.

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Table 1

Categories of Each Moderator

Moderator	Categories
Publication rank	Journal impact factor of 1.5 +
	Journal impact factor below 1.5
	Book chapters
	Unpublished/dissertations
Domain	Math/numbers
	Computer skills
	Science
	Problem solving
	Physical/motor skills
	Verbal/social skills
Age	Children: under 12 y/o
	Adolescents: between 12 and 18 y/o
	Adults: 18 y/o +
Dependent measure	All post-tests scores, error rates, rates of error detection
	Acquisition scores
	Reaction time scores
	Self-ratings
	Peer ratings
	Mental effort/load ratings

Moderator	Categories
Unassisted discovery	Unassisted, teaching oneself, practice problems
	Invention
	Other: matched guidance/probes in both discovery and comparison conditions
	Simulation
	Work with a naïve peer
Enhanced discovery	Generation
	Elicited explanation
	Guided discovery
Comparison condition	Direct teaching
	Feedback
	Worked examples with solutions provided
	Baseline
	unassisted: no exposure nor explanation
	enhanced: unassisted discovery or textbook only
	Explanations provided
	Other: study-specific condition

Table 2

Summary of Effect Sizes

Unassisted Discovery	Level of Analysis	Cohen's *d*	95% CI	Z	*p*-value (Z)	N	*Q*	*df* (*Q*)	*p*-value (*Q*)
	Studies								
	Fixed	-.30	[-.36, -.25]	-10.62	0.00	5,226	522.11	107	0.00
	Random	-.38	[-.50, -.25]	-5.69	0.00	5,226			
	Comparisons								
	Fixed	-.30	[-.32, -.27]	-23.08	0.00	25,986	3,490.42	579	0.00
	Random	-.38	[-.44, -.31]	-11.40	0.00	25,986			

Enhanced Discovery	Level of Analysis	Cohen's *d*	95% CI	Z	*p*-value (Z)	N	*Q*	*df* (*Q*)	*p*-value (*Q*)
	Studies								
	Fixed	.26	[.20, .32]	8.39	0.00	4,243	260.14	55	0.00
	Random	.30	[.15, .44]	4.10	0.00	4,243			
	Comparisons								
	Fixed	.24	[.21, .26]	18.61	0.00	25,925	2,037.19	359	0.00
	Random	.30	[.23, .36]	9.12	0.00	25,925			

Table 3

Samples Included in the Unassisted Discovery Meta-analysis

Author(s)	Year	Discovery *n*	Comparison *n*	*Cohen's d*	Domain	Age	Journal rank
Alibali	1999	26	29.25	-0.89	math/numbers	children	journal ≥ 1.5
Anastasiow, Sibley, Leonhardt, & Borich	1970	6	6	-0.06	math/numbers	children	journal < 1.5
Bannert	2000	37	35	0.74	computer skills	adults	journal < 1.5
Belcastro	1966	189	189	-0.26	math/numbers	adolescents	journal < 1.5
Bobis, Sweller, & Cooper E1	1994	15	15	1.07	math/numbers	children	journal < 1.5
Bobis, Sweller, & Cooper E2	1994	10	10	1.11	math/numbers	children	journal < 1.5
Bransford & Johnson E1	1972	10	10	-0.63	verbal/social skills	adolescents	journal ≥ 1.5
Bransford & Johnson E2	1972	17	17.5	-0.60	verbal/social skills	adults	journal ≥ 1.5
Bransford & Johnson E4	1972	9	11	-0.50	verbal/social skills	adolescents	journal ≥ 1.5
Brant, Hooper, & Sugrue	1991	33	35	0.55	science	adults	journal < 1.5
Brown, Kane, & Long E3	1989	21	16	-0.17	problem solving	children	journal < 1.5
Butler, Pine, & Messer	2006	34	28	-0.01	math/numbers	children	unpub/diss
Cantor, Dunlap, & Rettie	1982	24	24	-0.46	math/numbers	children	journal < 1.5

Author(s)	Year	Discovery *n*	Comparison *n*	Cohen's *d*	Domain	Age	Journal rank
Carroll E1	1994	16.8	16.8	-0.89	math/numbers	adolescents	journal ≥ 1.5
Carroll E2	1994	12	12	-2.05	math/numbers	adolescents	journal ≥ 1.5
Charney, Reder, & Kusbit	1990	20	45	-0.33	computer skills	adults	journal < 1.5
Craig	1965	30	30	-0.11	math/numbers	adults	journal < 1.5
Danner & Day	1977	20	20	-0.86	science	adolescents	journal ≥ 1.5
Destrebecqz E1	2004	20	20	-0.56	problem solving	adults	journal < 1.5
Destrebecqz E2	2004	12	12	-2.36	problem solving	adults	journal < 1.5
Elias & Allen	1991	37.86	34.43	-0.01	problem solving	children	journal < 1.5
Elshout & Veenman E1	1992	4.5	4.25	-0.19	science	adults	journal < 1.5
Elshout & Veenman E2	1992	4.4	5	-0.24	science	adults	journal < 1.5
Fender & Crowley E2	1992	12	12	-1.04	science	children	journal < 1.5
Guthrie	1967	18	18	-0.64	problem solving	adults	journal ≥ 1.5
Hendrickson & Schroeder	1941	30	30	-0.32	physical/motor skills	adolescents	journal ≥ 1.5
Hendrix	1947	13	13.5	0.51	math/numbers	adults	journal < 1.5
Hodges & Lee	1999	8	8.5	0.39	physical/motor skills	adults	journal < 1.5

Author(s)	Year	Discovery *n*	Comparison *n*	Cohen's *d*	Domain	Age	Journal rank
Howe, McWilliam, & Cross E2	2005	36	36	0.43	science	children	journal < 1.5
Howe, McWilliam, & Cross E3	2005	36	36	0.29	science	children	journal < 1.5
Jackson, Fletcher, & Messer	1992	36	24	-0.23	math/numbers	children	journal < 1.5
Jimenez, Mendez, & Cleeremans	1996	6	6	0.00	verbal/social skills	adults	journal ≥ 1.5
Kalyuga, Chandler, & Sweller E1	2001	9	8	-0.78	math/numbers	adults	journal < 1.5
Kalyuga, Chandler, & Sweller E2	2001	9	8	-0.28	math/numbers	adults	journal < 1.5
Kalyuga, Chandler, Tuovinen, & Sweller E1	2001	12	12	-0.53	computer skills	adults	journal ≥ 1.5
Kalyuga, Chandler, Tuovinen, & Sweller E2	2001	12	12	0.70	computer skills	adults	journal ≥ 1.5
Kamii & Dominick	1997	16.29	16.71	0.21	math/numbers	children	journal < 1.5
Kelemen	2003	12	11	-0.82	science	children	journal ≥ 1.5
Kersh	1958	16	16	-0.18	math/numbers	adults	journal ≥ 1.5
Kersh: Article 2	1962	10	10	0.50	math/numbers	adolescents	journal ≥ 1.5
King	1991	8	7.5	-0.58	problem solving	children	journal ≥ 1.5
Kittell	1957	45	43.5	-0.78	verbal/social skills	children	journal ≥ 1.5
Klahr & Nigam	2004	52	52	-1.14	science	children	journal ≥ 1.5

Author(s)	Year	Discovery *n*	Comparison *n*	Cohen's *d*	Domain	Age	Journal rank
Kuhn & Dean	2005	12	12	-1.18	science	children	journal ≥ 1.5
Lawson & Wollman	1976	16	16	-0.82	science	adolescents	journal < 1.5
Lazonder & van der Meij	1993	30	34	0.67	computer skills	adults	journal < 1.5
Lazonder & van der Meij: Article 2	1994	21	21	0.05	computer skills	adults	journal < 1.5
Lazonder & van der Meij: Article 3	1995	25	25	-0.44	computer skills	adults	journal < 1.5
Lee & Thompson	1997	66	64	-0.92	computer skills	adults	journal < 1.5
Leutner E1	1993	16	16	-0.09	problem solving	adolescents	journal < 1.5
Leutner E2	1993	19	19	-0.36	problem solving	adults	journal < 1.5
Leutner E3	1993	20	20	-0.38	problem solving	adolescents	journal < 1.5
McDaniel & Pressley E1	1984	16.6	17.6	-1.21	verbal/social skills	adults	journal ≥ 1.5
McDaniel & Pressley E2	1984	21	21	-1.06	verbal/social skills	adults	journal ≥ 1.5
McDaniel & Schlager E1	1990	31	29.5	0.00	problem solving	adults	journal < 1.5
McDaniel & Schlager E2	1990	60	60	0.42	problem solving	adults	journal < 1.5
Messer, Joiner, Loveridge, Light, & Littleton E1	1993	14	13	0.32	science	children	journal < 1.5
Messer, Joiner, Loveridge, Light, & Littleton E2	1993	18	20	-1.14	science	children	journal < 1.5

Author(s)	Year	Discovery *n*	Comparison *n*	Cohen's *d*	Domain	Age	Journal rank
Messer, Mohamedali, & Fletcher	1996	21	20	0.34	problem solving	children	journal < 1.5
Messer, Norgate, Joiner, Littleton, & Light E1	1996	11.75	10.5	-0.89	science	children	journal < 1.5
Messer, Norgate, Joiner, Littleton, & Light E2	1996	16	15	0.43	science	children	journal < 1.5
Morton, Trehub, & Zelazo E2	2003	15.29	16.14	-2.19	verbal/social skills	children	journal ≥ 1.5
Mwangi & Sweller E1	1998	9	9	-0.46	math/numbers	children	journal < 1.5
Nadolski, Kirschner, & Van Merriënboer	2005	11	12	0.09	problem solving	adults	journal < 1.5
O'Brien & Shapiro	1977	15	15	-0.15	math/numbers	adults	journal < 1.5
Paas	1992	13	15	-2.25	math/numbers	adolescents	journal ≥ 1.5
Paas & Van Merriënboer	1994	30	30	-0.77	problem solving	adults	journal ≥ 1.5
Pany & Jenkins	1978	6	6	-1.93	verbal/social skills	children	journal < 1.5
Peters	1970	30	30	0.25	math/numbers	children	journal < 1.5
Pillay E1	1994	10	20	-1.09	problem solving	adolescents	journal < 1.5
Pillay E2	1994	10	20	-0.78	problem solving	adolescents	journal < 1.5
Pine, Messer, & Godfrey	1999	14	14	-0.74	science	children	journal < 1.5
Quilici & Mayer E1	1996	27	54	0.92	math/numbers	adults	journal ≥ 1.5
Quilici & Mayer E2	1996	18	18	-1.69	math/numbers	adults	journal ≥ 1.5

Author(s)	Year	Discovery *n*	Comparison *n*	Cohen's *d*	Domain	Age	Journal rank
Radziszewska & Rogoff	1991	20	20	-1.25	problem solving	children	journal ≥ 1.5
Rappolt-Schlichtmann, Tenenbaum, Koepke, & Fischer	2007	27	37	-0.61	science	children	journal < 1.5
Reinking & Rickman	1990	45	15	-1.09	verbal/social skills	children	journal < 1.5
Rieber & Parmley	1995	25	27.5	-0.65	science	adults	journal < 1.5
Rittle-Johnson	2006	21	21.5	-0.23	math/numbers	children	journal ≥ 1.5
Salmon, Yao, Berntsen, & Pipe	2007	16	16	-1.66	verbal/social skills	children	journal < 1.5
Scandura E2	1964	23	23	0.00	math/numbers	children	journal < 1.5
Shore & Durso	1990	60	60	-0.14	verbal/social skills	adults	journal ≥ 1.5
Shute, Glaser, & Raghavan	1989	10	10	0.42	math/numbers	adults	book chapter
Siegel & Corsini	1969	12	12	-0.90	problem solving	children	journal ≥ 1.5
Singer & Gaines	1975	19	18	-0.27	physical/motor skills	adults	journal < 1.5
Stark, Gruber, Renkl, & Mandl	1998	15	15	-0.54	math/numbers	adults	journal < 1.5
Strand-Cary & Klahr	2008	29	32	-0.85	science	children	journal < 1.5
Sutherland, Pipe, Schick, Murray, & Gobbo	2003	12	11.5	-0.10	verbal/social skills	children	journal < 1.5
Swaak, deJong, & van Joolingen	2004	67	55	-0.56	science	adolescents	journal < 1.5
Swaak, van Joolingen, & de Jong	1998	21	21	-0.44	science	adults	journal < 1.5

Author(s)	Year	Discovery *n*	Comparison *n*	Cohen's *d*	Domain	Age	Journal rank
Sweller, Chandler, Tierney, & Cooper E1	1990	16	16	0.20	math/numbers	adolescents	journal ≥ 1.5
Sweller, Chandler, Tierney, & Cooper E3	1990	12	12	-1.78	math/numbers	adolescents	journal ≥ 1.5
Tarmizi & Sweller E3	1988	10	10	0.20	math/numbers	adolescents	journal ≥ 1.5
Tarmizi & Sweller E4	1988	10	10	0.28	math/numbers	adolescents	journal ≥ 1.5
Tarmizi & Sweller E5	1988	10	10	-0.71	math/numbers	adolescents	journal ≥ 1.5
Trafton & Reiser	1993	20	20	0.39	computer skills	adults	journal < 1.5
Tunteler & Resing	2002	18	18	-2.19	problem solving	children	journal < 1.5
van der Meij & Lazonder	1993	13	12	1.03	computer skills	adults	journal < 1.5
van hout Wolters	1990	24	24	-0.54	science	adolescents	book chapter
Veenman, Elshout, & Busato	1994	15	14	-0.49	science	adults	journal < 1.5
Ward & Sweller E1	1990	21	21	-1.07	science	adolescents	journal < 1.5
Ward & Sweller E2	1990	16	16	-1.52	science	adolescents	journal < 1.5
Ward & Sweller E3	1990	17	17	0.25	science	adolescents	journal < 1.5
Ward & Sweller E4	1990	15	15	-0.42	science	adolescents	journal < 1.5
Ward & Sweller E5	1990	15.5	15.5	-0.47	science	adolescents	journal < 1.5
Wittrock	1963	67	75	-0.84	verbal/social skills	adults	journal ≥ 1.5

Author(s)	Year	Discovery *n*	Comparison *n*	Cohen's *d*	Domain	Age	Journal rank
Worthen	1968	216	216	0.08	math/numbers	children	journal < 1.5
Zacharia & Anderson	2003	13	13	4.62	science	adults	journal < 1.5

Table 4

Effect Sizes by Domain for Unassisted Discovery

Domain	Cohen's *d*	95% CI	*Z*	*k*	*N*	*Q*
Math/numbers	-.16	[-.30, -.03]	-2.38*	129	6,639	
Computer skills	.07	[-.11, .23]	0.75	72	3,627	
Science	-.39	[-.53, -.24]	-5.27**	117	4,399	
Problem solving	-.48	[-.60, -.36]	-7.73**	154	5,637	
Physical/motor skills	-.01	[-.39, .38]	-0.02	15	520	
Verbal/social skills	-.95	[-1.11, -.79]	-11.66**	87	5,164	
Between-classes effect				5	25,986	91.75**

*$p < .05$, **$p < .01$

Post-hoc comparisons (*Q)*

Domain	Math/numbers	Computer skills	Science	Problem solving	Physical/motor skills
Math/numbers					
Computer skills	4.72				
Science	6.09	16.64***			
Problem solving	13.65***	28.29***	0.88		
Physical/motor skills	0.63	0.11	3.67	5.95	
Verbal/social skills	50.03***	58.17***	22.65***	18.35***	14.87***

***$p < .003$ (adjusted for post-hoc comparisons)

Table 5

Effect Sizes by Age for Unassisted Discovery

Age	Cohen's *d*	95% CI	*Z*	*k*	*N*	*Q*
Children	-.44	[-.56, -.32]	-7.11**	163	8,784	
Adolescents	-.53	[-.66, -.40]	-8.01**	148	5,556	
Adults	-.26	[-.35, -.16]	-5.28**	266	11,646	
Between-classes effect				2	25,986	12.29*

**p* < .05, ***p* < .01

Post-hoc comparisons (*Q*)

Age	Children	Adolescents
Children		
Adolescents	1.51	
Adults	5.00	10.41***

****p* < .017 (adjusted)

Table 6

Effect Sizes by Dependent Measure for Unassisted Discovery

Dependent measure	Cohen's *d*	95% CI	*Z*	*k*	*N*	*Q*
Post-test scores	-.35	[-.42, -.28]	-9.30**	430	20,070	
Acquisition scores	-.95	[-1.16, -.74]	-8.93**	54	2,059	
Reaction times	-.21	[-.39, -.02]	-2.20*	69	2,632	
Self-ratings	.07	[-.39, .54]	0.31	9	668	
Peer ratings	-.32	[-1.12, .49]	-0.77	2	306	
Mental effort/load	-.16	[-.64, .32]	-0.66	10	251	
Between-classes effect				5	25,986	37.38**

**p* < .05, ***p* < .001

Post-hoc comparisons (*Q*)

Dependent measure	Post-test scores	Acquisition scores	Reaction times	Self-ratings	Peer ratings
Post-test scores					
Acquisition scores	28.14***				
Reaction times	1.98	23.84***			
Self-ratings	3.30	15.89***	1.28		
Peer ratings	0.01	1.88	0.06	2.70	
Mental effort/load	0.60	7.82	0.04	1.99	0.14

****p* < .003 (adjusted)

Table 7

Effect Sizes by Type of Unassisted Discovery

Type of Discovery	Cohen's *d*	95% CI	*Z*	*k*	*N*	*Q*
Unassisted	-.41	[-.48, -.34]	-11.15**	476	21,832	
Invention	-.34	[-.60, -.08]	-2.52*	38	1,191	
Matched probes	.19	[-.26, .64]	0.84	13	303	
Simulation	-.13	[-.42, .15]	-0.92	29	1,652	
Work with a naïve peer	-.47	[-.81, -.13]	-2.72**	19	1,008	
Between-classes effect				4	25,986	10.02*

**p* < .05, ***p* < .01

Post-hoc comparisons (*Q*)

Type of Discovery	Unassisted	Invention	Matched probes	Simulation
Unassisted				
Invention	0.23			
Matched probes	6.57	7.06		
Simulation	3.35	0.95	1.56	
Work with a naïve peer	0.13	0.35	4.37	2.23

****p* < .005 (adjusted)

Table 8

Effect Sizes by Comparison Condition for Unassisted Discovery

Comparison condition	Cohen's *d*	95% CI	*Z*	*k*	*N*	*Q*
Direct teaching	-.29	[-.38, -.20]	-6.10**	272	14,145	
Feedback	-.46	[-.64, -.29]	-5.11**	74	2,578	
Worked examples	-.63	[-.76, -.50]	-9.70**	150	5,319	
No exposure / pre + post	.21	[-.14, .56]	1.18	17	881	
Explanations provided	-.28	[-.47, -.08]	-2.77*	59	2,927	
Other	.02	[-.84, .87]	0.04	2	136	
Between-classes effect				5	25,986	32.31**

*$p < .05$, **$p < .001$

Post-hoc comparisons (*Q*)

Comparison condition	Direct teaching	Feedback	Worked examples	No exposure / pre + post	Explanations provided
Direct teaching					
Feedback	3.27				
Worked examples	18.98***	1.57			
No exposure / pre+post	8.70	9.15***	13.70***		
Explanations provided	0.01	1.80	6.99	5.00	
Other	0.62	1.05	1.56	0.13	0.44

***$p < .003$ (adjusted)

Table 9

Studies Included in the Enhanced Discovery Meta-analysis

Author(s)	Year	Discovery *n*	Comparison *n*	Cohen's *d*	Domain	Age	Journal rank
Amsterlaw & Wellman	2006	12	12	1.11	verbal/social skills	children	journal < 1.5
Anastasiow, Sibley, Leonhardt, & Borich	1970	6	6	-0.08	math/numbers	children	journal < 1.5
Andrews	1984	25	28	1.27	science	adults	journal < 1.5
Bielaczyc, Pirolli, & Brown	1995	11	13	0.95	computer skills	adults	journal < 1.5
Bluhm	1979	20	17	1.44	science	adults	journal < 1.5
Bowyer & Linn	1978	312	219	0.20	science	children	journal < 1.5
Butler, Pine, & Messer	2006	32	31	-0.02	math/numbers	children	unpub/diss
Chen & Klahr	1999	30	30	-0.07	science	children	journal ≥ 1.5
Chi, de Leeuw, Chiu, & LaVancher	1994	14	10	0.94	science	adolescents	journal ≥ 1.5
Coleman, Brown, & Rivkin	1997	14	14	0.61	science	adults	journal < 1.5
Crowley & Siegler	1999	57	57	-0.25	problem solving	children	journal ≥ 1.5
Debowski, Wood, & Bandura	2001	24	24	1.07	computer skills	adults	journal ≥ 1.5
Denson	1986	45	34	0.10	science	adults	unpub/diss
Foos, Mora, & Tkacz E1	1994	78	90	0.53	science	adults	journal ≥ 1.5

Author(s)	Year	Discovery *n*	Comparison *n*	Cohen's *d*	Domain	Age	Journal rank
Foos, Mora, & Tkacz E2	1994	25	25	0.71	science	adults	journal ≥ 1.5
Gagne & Brown	1961	11	11	1.41	math/numbers	adolescents	journal ≥ 1.5
Ginns, Chandler, & Sweller E1	2003	10	10	-0.67	computer skills	adults	journal < 1.5
Ginns, Chandler, & Sweller E2	2003	13	13	0.67	math/numbers	adolescents	journal < 1.5
Grandgenett & Thompson	1991	72	71	0.05	computer skills	adults	journal < 1.5
Greenockle & Lee	1991	20	20	0.48	physical/motor skills	adults	journal < 1.5
Hiebert & Wearne	1993	24	21.25	0.70	math/numbers	children	journal < 1.5
Hirsch	1977	61	76	0.56	math/numbers	adolescents	journal < 1.5
Howe, McWilliam, & Cross E1	2005	31	30	0.15	science	children	journal < 1.5
Howe, McWilliam, & Cross E2	2005	35	36	0.15	science	children	journal < 1.5
Howe, McWilliam, & Cross E3	2005	35.5	36	0.34	science	children	journal < 1.5
Jackson, Fletcher, & Messer	1992	12	24	0.01	math/numbers	children	journal < 1.5
Kasten & Liben	2007	34	99	0.42	problem solving	children	journal ≥ 1.5
Kersh	1958	16	16	0.12	math/numbers	adults	journal ≥ 1.5
Kersh: Article 2	1962	10	10	-0.10	math/numbers	adolescents	journal ≥ 1.5

Author(s)	Year	Discovery	Comparison	Cohen's	Domain	Age	Journal rank

		n	*n*	*d*			
Kuhn, Black, Keselman, & Kaplan	2000	21	21	0.29	science	adolescents	journal < 1.5
Lamborn, Fischer, & Pipp	1994	113	113	1.06	verbal/social skills	adolescents	journal ≥ 1.5
Murphy & Messer	2000	41	40.5	0.46	science	children	journal < 1.5
Mwangi & Sweller E3	1998	12	12	-0.04	math/numbers	children	journal < 1.5
Ohrn, van Oostrom, & van Meurs	1997	11	12	0.99	science	adults	journal ≥ 1.5
Olander & Robertson	1973	190	184	-0.02	math/numbers	children	journal < 1.5
Peters	1970	30	30	-0.09	math/numbers	children	journal < 1.5
Pillow, Mash, Aloian, & Hill	2002	15	15	0.44	verbal/social skills	children	journal < 1.5
Pine & Messer	2000	40	44	0.55	science	children	journal < 1.5
Pine, Messer, & Godfrey	1999	14	14	-0.35	science	children	journal < 1.5
Ray	1961	45	45	0.44	math/numbers	adolescents	journal < 1.5
Reid, Zhang, & Chen	2003	20	18	0.16	science	adolescents	journal < 1.5
Rittle-Johnson	2006	22	21	0.19	math/numbers	children	journal ≥ 1.5
Rittle-Johnson, Saylor, & Swygert	2007	36	18	0.81	problem solving	children	journal < 1.5
Scandura E1	1964	23	23	0.00	math/numbers	children	journal < 1.5

Author(s)	Year	Discovery *n*	Comparison *n*	Cohen's *d*	Domain	Age	Journal rank
Singer & Pease	1978	16	16	2.62	physical/motor skills	adults	journal < 1.5
Stark, Mandl, Gruber, & Renkl	2002	27	27	0.94	math/numbers	adults	journal < 1.5
Stull & Mayer E1	2006	51	52.5	-0.60	science	adults	unpub/diss
Stull & Mayer E2	2006	38	39	-1.14	science	adults	unpub/diss
Stull & Mayer E3	2006	33	32.5	-1.10	science	adults	unpub/diss
Tarmizi & Sweller E2	1988	12	12	-0.08	math/numbers	adolescents	journal ≥ 1.5
Tenenbaum, Alfieri, Brooks, & Dunne	2008	32	30.5	0.20	verbal/social skills	children	journal < 1.5
Tuovinen & Sweller	1999	16	16	-0.67	computer skills	adults	journal ≥ 1.5
Vichitvejpaisal et al.	2001	40	40	-0.28	science	adults	journal ≥ 1.5
Zhang, Chen, Sun, & Reid E1	2004	13	13.67	-0.16	computer skills	adolescents	journal < 1.5
Zhang, Chen, Sun, & Reid E2	2004	14	16	0.36	computer skills	adolescents	journal < 1.5
Zimmerman & Sassenrath	1978	119.67	119.67	0.51	math/numbers	children	journal < 1.5

Table 10

Effect Sizes by Domain for Enhanced Discovery

Domain	Cohen's *d*	95% CI	*Z*	*k*	*N*	*Q*
Math/numbers	.29	[.18, .40]	5.24**	116	9,100	
Computer skills	.64	[.44, .84]	6.26**	36	1,379	
Science	.11	[.02, .20]	2.30*	152	12,164	
Problem solving	.20	[-.08, .47]	1.40	14	1,723	
Physical/motor skills	1.05	[.80, 1.30]	8.25**	23	896	
Verbal/social skills	.58	[.26, .90]	3.51**	13	663	
Between-classes effect				5	25,925	65.53**

*$p < .05$, **$p < .001$

Post-hoc comparisons (*Q*)

Domain	Math/ numbers	Computer skills	Science	Problem solving	Physical/motor skills
Math/numbers					
Computer skills	12.14***				
Science	6.69	18.65***			
Problem solving	0.84	5.55	0.31		
Physical/motor skills	34.59***	4.96	41.67***	15.73***	
Verbal/social skills	3.59	0.04	6.67	3.51	3.48

***$p < .003$ (adjusted)

Table 11

Effect Sizes by Age for Enhanced Discovery

Age	Cohen's *d*	95% CI	*Z*	*k*	*N*	*Q*
Children	.24	[.14, .33]	4.94**	157	16,556	
Adolescents	.19	[.04, .33]	2.50*	71	3,420	
Adults	.44	[.33, .55]	7.97**	129	5,949	
Between-classes effect				2	25,925	10.68*

*$p < .05$, **$p < .001$

Post-hoc comparisons (*Q*)

Age	Children	Adolescents
Children		
Adolescents	0.02	
Adults	7.64***	5.37

***$p < .017$ (adjusted)

Table 12

Effect Sizes by Dependent Measure for Enhanced Discovery

Dependent measure	Cohen's *d*	95% CI	*Z*	*k*	*N*	*Q*
Post-test scores	.28	[.22, .33]	8.38**	303	22,636	
Acquisition scores	.54	[.35, .74]	5.50**	34	2,205	
Reaction times	-.72	[-1.07, -.37]	-4.04**	11	668	
Self-ratings	1.25	[.84, 1.65]	6.02**	7	384	
Mental effort/load	-1.01	[-2.22, .19]	-1.65	0	32	
Between-classes effect				4	25,925	64.60**

**$p < .001$

Post-hoc comparisons (*Q*)

Dependent measure	Post-test scores	Acquisition scores	Reaction times	Self-ratings
Post-test scores				
Acquisition scores	6.73			
Reaction times	31.61***	10.19***		
Self-ratings	29.68***	6.66	5.18	
Mental effort/load	5.94	4.68	0.03	21.33***

***$p < .005$ (adjusted)

Table 13

Effect Sizes by Type of Enhanced Discovery

Discovery	Cohen's *d*	95% CI	*Z*	*k*	*N*	*Q*
Generation	-.15	[-.28, -.02]	-2.32*	87	3,905	
Elicited explanation	.36	[.26, .47]	6.93**	128	7,037	
Guided discovery	.50	[.40, .59]	9.96**	142	14,983	
Between-classes effect				2	25,925	65.00**

**p* < .05, ***p* < .001

Post-hoc comparisons (*Q*)

Discovery	Generation	Elicited explanation
Generation		
Elicited explanation	33.20***	
Guided discovery	57.43***	3.86

****p* < .017 (adjusted)

Table 14

Effect Sizes by Comparison Condition for Enhanced Discovery

Comparison condition	Cohen's *d*	95% CI	*Z*	*k*	*N*	*Q*
Direct teaching	.26	[.15, .37]	4.74**	123	13,668	
Worked examples	.06	[-.21, .32]	0.41	22	634	
Unassisted / pre + post	.33	[.25, .42]	7.48**	190	10,280	
Explanations provided	.33	[.06, .60]	2.39*	19	1,238	
Other	1.30	[.40, 2.20]	2.82*	1	105	
Between-classes effect				4	25,925	9.12

**p* < .05, ***p* < .001