Does Calling it 'Morgan's Way' Reduce Student Learning? Evaluating the Effect of Person-Presentation During Comparison and Discussion of Worked Examples in Mathematics Classrooms

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Abstract

Mathematics textbooks sometimes present worked examples as being generated by particular fictitious students (i.e., *person-presentation*). However, there are indicators that person-presentation of worked examples may harm generalization of the presented strategies to new problems. In the context of comparing and discussing worked examples during extended classroom instruction, the current study compared the impact of person-presentation to strategy labels on students' posttest accuracy and ratings of strategy generalizability. Five Algebra teachers and their 168 students used worked examples either presented using fictitious students or with a strategy label during a multi-week unit on equation solving, with teachers randomly assigned to condition. All students compared and discussed the worked examples. In this context, we found no effect of condition on student accuracy at posttest, nor on their ratings of the generalizability of the presented strategies. We discuss why previously found negative effects of person-presentation may not have extended to this context.

Keywords: mathematics instruction, generalization, worked examples, comparison

Introduction

When learning something new, studying examples is a logical first step. Knowing what to apply from examples and how to apply it to different situations, however, is where learning often breaks down. Generalizing knowledge from examples to new contexts and problems (i.e., knowledge generalization) is critical for it to be useful, yet failures of knowledge generalization are common (i.e., learning information but failing to use that information in a different context) (Gick & Holyoak, 1980, 1983). Knowledge generalization in part depends on how tightly knowledge is tied to specific features of examples, constraining how broadly it is applied to new examples and problem conditions (Chi, Bassock, Lewis, Reimann, & Glaser, 1989; Siegler, 1994). What instructional practices promote the acquisition of generalizable knowledge from examples and what practices impede knowledge generalization? Despite decades of laboratory-based research on example-based learning, too little theory and evidence is available on how instructional practices are best combined to support knowledge generalization, especially in educational settings. In the current study, we contrast the effect of presenting example strategies as being generated by particular, fictitious students (person-presentation) to using strategy labels without reference to a person (strategy-label presentation) on students' accuracy on a posttest. The impact was tested in the context of students comparing and discussing multiple examples, which are well-established instructional practices to facilitate learning from examples (Alfieri, Nokes-Malach, & Schunn, 2013; Chi, de Leeuw, Chiu, & LaVancher, 1994; McEldoon, Durkin, & Rittle-Johnson, 2013), and in the context of extended classroom instruction implemented by teachers.

Costs and Benefits of Person-Presentation of Worked Examples

Designing example-based classroom materials highlights decisions that must be made to support effective learning. In problem-solving domains such as mathematics, examples are often text-based worked examples, which demonstrate a correct strategy for solving a particular problem step-by-step and which lead to more efficient and deep learning than independently solving equivalent problems (Atkinson, Derry, Renkl, & Wortham, 2000; Renkl, 2014). The current study addresses an important and practical decision when designing text-based worked examples for classroom instruction: should they be presented with or without attribution to particular students? On the one hand, person-presentation of worked examples is often found in textbooks, reflecting perceived advantages of its use in educational materials. For example, presentation of worked examples as being generated by a fictitious student occurred in many U.S. middle school mathematics textbooks (Riggs, Alibali, & Kalish, 2015) and was common when multiple strategies for solving a problem were presented in Japanese 5^{th} and 7^{th} grade textbooks (Rittle-Johnson, 2019). In contrast, in research studies with text-based worked examples, they are usually presented as didactical strategies not generated by particular individuals, and consideration of person-presentation has not been highlighted in past reviews of design principles for text-based worked examples (e.g., Renkl, 2014). Thus, empirical evidence on the use of person-presentation is needed to inform design principles for creating text-based worked examples.

In the current study, we tested whether person-presentation impacts knowledge generalization when text-based worked examples are compared and discussed as part of extended classroom instruction. We consider potential costs and benefits of person-presentation in turn and then provide details on the instructional context in which we tested the impact of person-presentation.

Potential Costs. Emerging research suggests that person-presentation may harm a particular aspect of learning - knowledge generalization. When information is presented as specific to a particular individual rather than a general category, learners are less likely to generalize the information to new situations (e.g., less likely to say that something learned about a specific person is also true of another person in that same social category) (Cimpian & Erickson, 2012; Riggs, Kalish, & Alibali, 2014). Further, two recent studies indicate that person-presentation of worked examples can harm knowledge generalization in a problem-solving context. In one study, undergraduates studied a worked example of a mathematics problem-solving strategy that was either presented as belonging to a specific individual or not (see Figure 1). Person-presentation of the strategy reduced participants' transfer of the demonstrated strategy to new problems (i.e., strategy generalization) relative to no person-presentation (Riggs et al., 2015). In particular, the worked example illustrated a multiplicative strategy for solving an algebra word problem about constant change, which is a more efficient, but less commonly used strategy. There were four person-presentation conditions, which varied in the level of detail provided about the person (e.g., a photograph and/or background information) and two non-person-presentation conditions that varied in whether the strategy was labeled (i.e., "the continuous strategy") or not labeled. After studying the worked example, participants solved five transfer problems, which had different cover stories and varied wording that suggested a continuous or additive rate of change. Whether students used the demonstrated multiplicative strategy, the more common additive strategy, or another strategy was coded based on their written work; whether the strategy was implemented correctly and whether a correct answer was given was also coded. The four person-presentation formats did not differ from each other nor did the two non-person presentation conditions differ from each other on any outcome. However, participants in the person-presentation conditions were more likely to correctly use the demonstrated multiplicative strategy across the five transfer problems. The study focused on learning of the demonstrated strategy, and there were not differences by condition on accuracy of their answers, as problems could be solved correctly using a different strategy. A second study with middle-school students used similar materials and the same basic design, with a person-presentation condition and a non-person-presentation, strategy-label condition; students worked individually on study materials during a math class. Person presentation of a worked example had a similar negative effect on strategy generalization for middle-school students, measured both in terms of attempted use of the demonstrated strategy and accurate use of the demonstrated strategy on four transfer problems (Riggs, Alibali, & Kalish, 2017). Thus, holding all other design features constant, presenting a worked example as being used by a particular individual to solve a problem reduced strategy generalization to new problems relative to presenting the same worked example without attributing it to a person.

Learners seem to view strategies that are presented as belonging to specific individuals as less generalizable to new problems. In the study with middle-school students, students rated the perceived generality of the studied strategy by indicating the likelihood that another student, a teacher, and the students themselves would be to use the strategy in the future using a 5-point Likert scale. Students rated the strategy as less generalizable in the person-presentation condition, and the generalization ratings partially mediated the effect of person-presentation on students' attempts to transfer the strategy to new problems. Thus, person-presentation seemed to harm attempted transfer in part by reducing students' evaluations of the generalizability of the strategy.

This research supports a reduced-generalization hypothesis, such that person-presentation of examples can harm knowledge generalization relative to no person-presentation, in part because it can reduce learners' evaluations of the generalizability of the strategy. However, past experimental research on person-presentation was conducted in a single session under lab-like conditions, presenting a single worked example and focused on strategy transfer. There was no reported difference in overall accuracy, at least in part because people often already know a correct, albeit less efficient strategy, for the target topic. Nevertheless, the reduced-generalization hypothesis has potential practical significance for mathematics instruction using worked examples, as a primary goal of using worked examples is for students to learn and use the demonstrated strategies, with the expectation that this will improve accuracy on assessments.

Further, aids to better support learning from the worked example, such as including multiple examples, comparison or explanation prompts, were not included. Comparison and explanation might play a protective role because they support knowledge generalization. Thus, it is important to test the impact of person-presentation in more supportive and more authentic educational contexts, using more typical classroom assessments, before making instructional recommendations.

Potential Benefits. In contrast to a reduced-generalization hypothesis, research on social learning theory and motivation theory suggest potential benefits of using person-presentation of worked examples. Specifically, person-presentation may support learning by engaging social learning processes and increasing situational interest. First, Bandura's social learning theory (1977) suggests that people learn from modelling (a model person performing a task). Videos and animations of a person demonstrating and explaining a strategy promote learning and self-efficacy, especially if the model is perceived as similar to the observer (e.g., Braaksma, Rijlaarsdam, & van den Bergh, 2002). Studies using text-based worked examples have begun to explore the implications of social learning theory by using person-presentation (including people who the text-based worked examples are attributed to) and manipulating characteristics of the people (e.g., gender, expertise) (Boekhout, van Gog, van de Wiel, Gerards-Last, & Geraets, 2010). Although characteristics of the model person influence learning from live and video models (e.g., Schunk, Hanson, & Cox, 1987), characteristics of the person used in text-based worked examples have not been shown to influence learning, measured by accuracy on transfer problems, when the content of the worked example is held constant (Boekhout et al., 2010; Hoogerheide, Loyens, Jadi, Vrins, & van Gog, 2017). Unfortunately, this past research with text-based worked examples did not include a no-person-presentation condition. Nevertheless, person-presentation of worked examples has potential benefits because it could engage social learning processes, such as social comparison and vicarious learning.

Second, person-presentation may also promote situational interest. Situational interest can promote sustained attention to materials and improve learning (Hidi & Harackiewicz, 2000). For example, (a) situational interest is increased when there are characters in the text with whom the reader identifies (e.g., Jose & Brewer, 1984) and (b) increased situational interest improves text recall and reading comprehension (Hidi & Harackiewicz, 2000; Schraw, Bruning, & Svoboda, 1995). More generally, the design of educational materials impacts student motivation, which influences learning by increasing or decreasing cognitive engagement with the materials (e.g., Moreno, 2006). For example, decorative illustrations accompanying geometry problems increased situational interest and enhanced accuracy on far transfer problems (Magner, Schwonke, Aleven, Popescu, & Renkl, 2014). Although this research has not be done in the context of worked examples, it suggests a potential benefit of person-presentation of worked examples if it promotes situational interest. Research on designing worked examples has too rarely focused on features of worked examples that promote learning by increasing situational interest or other aspects of motivation. Increasing interest and motivation of students to engage in a task is especially relevant in classroom contexts (Hidi & Harackiewicz, 2000). Overall, potential benefits may counteract or outweigh potential costs in some contexts. Further, this may be particularly true when the outcome measure includes accuracy on transfer problems, as they have in relevant past research on social learning and situational interest.

In the current study, we contrasted the effect of person-presentation to using strategy labels without reference to a person (i.e., strategy-label presentation) on students' accuracy on a comprehensive posttest. Past research on worked examples most often presented the examples either without labeling the strategy, using a generic label (e.g., "Correct solution"), or using a strategy label (e.g., "Permutation without replacement"). As noted above, the one study that directly contrasted learning from a worked example with no label vs. a strategy label reported no effect of this manipulation on outcomes (strategy generalization or accuracy) (Riggs et al., 2015). In the current study, using strategy labels was an appropriate control condition given the need for teachers and students to refer to and distinguish between the different presented strategies when comparing and discussing them.

Context: Comparison and Discussion of Multiple Strategies in Classrooms

Past experimental research on person-presentation was conducted in a context that does not follow recommendations for promoting learning from worked examples. Providing multiple worked examples and prompting students to generate explanations while studying the examples are both considered core components of effective use (Atkinson et al., 2000; Paas & van Gog, 2006; Renkl, 2014). In contrast, in previous research on person-presentation, a single strategy was presented and there were no prompts to try to explain the strategy or the underlying concepts (Riggs et al., 2015, 2017). In the context of comparing and explaining multiple strategies, potential negative effects of person-presentation of strategies could be counteracted by comparison and explanation which aid strategy generalization and support learning more broadly. Thus, it is important to test effects of person-presentation on knowledge generalization in a context that is more supportive of learning from worked examples.

Comparison and discussion of multiple examples are well-established instructional practices to facilitate learning from examples, especially worked examples (Alfieri et al., 2013; Chi et al., 1994; McEldoon et al., 2013) and are recommended instructional methods in mathematics education (NCTM, 2014; Woodward et al., 2012). In particular, they can aid strategy generalization by supporting the development of broadly applicable strategy knowledge. Comparing similarities and differences between two example strategies typically improves generalization of the strategy relative to studying single examples (Gentner, Loewenstein, & Thompson, 2003; Star & Rittle-Johnson, 2009). Comparison of strategies is theorized to support generalization by focusing attention on underlying problem structure and away from surface features (Gentner & Medina, 1998; Gick & Holyoak, 1983). In mathematics, comparison of worked example pairs (two step-by-step solutions for how to solve the same problem shown side-by-side) leads to greater strategy transfer and flexibility (i.e., knowledge of multiple strategies and when to use each), and sometimes greater conceptual knowledge, relative to studying worked examples one at a time (Rittle-Johnson & Star, 2007; Star & Rittle-Johnson, 2009). In this past research on comparison of multiple strategies, strategies were presented as fictitious students' strategies (i.e., person-presentation) to align with best practices in mathematics education to compare multiple students' strategies for solving a problem (although those recommendations focused on real students' strategies; NCTM, 2000).

Explanation of worked examples also improves strategy generalization, broadening the knowledge of problem conditions under which strategies are applicable (Chi et al., 1989; Siegler & Chen, 2008). Further, providing prompts to explain worked examples leads to greater learning than studying worked examples without being prompted to generate explanations (e.g., Bielaczyc, Pirolli, & Brown, 1995; Chi et al., 1994). Discussions in which students generate explanations and teachers facilitate a discussion of different student responses further improves students' mathematics learning (Silver, Ghousseini, Gosen, Charalambous, & Strawhun, 2005; Stein, Engle, Smith, & Hughes, 2008). Thus, comparing and discussing multiple strategies aids strategy generalization and supports learning more broadly.

Unfortunately, comparison of multiple strategies is infrequent in mathematics textbooks (Rittle-Johnson, Star, Durkin, & Loehr, 2019) and is limited in frequency and effectiveness of use in U.S. mathematics classrooms (Star et al., 2015). To address this need**,** we have developed a *Comparison and Discussion of Multiple Strategies* instructional approach to support more frequent and effective use of comparison of multiple strategies in mathematics classrooms. Our approach includes supplemental Algebra I curriculum materials that present multiple strategies using worked examples and explanation prompts to explicitly compare the strategies along with supports for small-group and whole-class discussion of the comparisons. The worked examples always used person-presentation in past research. The effectiveness of our *Comparison and Discussion of Multiple Strategies* instructional approach has been demonstrated in small-scale, researcher led classroom-based studies, with evidence of promise when implemented by Algebra I teachers across the school year (Durkin, Star, & Rittle-Johnson, 2017; Star et al., 2015).

Current Study

The current study addresses an important and practical decision when designing text-based worked examples for instruction: should they be presented with or without attribution to fictitious students? In research studies, they are rarely presented as being generated by particular, fictitious students, but in mathematics textbooks, they sometimes are. We tested the impact of person presentation of worked examples vs. using strategy labels in the context of students comparing and discussing multiple strategies during regular classroom instruction. It was part of a larger project aimed at supporting the frequency and quality of teachers' use of comparison and discussion in their Algebra I instruction (Durkin et al., 2017; Star et al., 2015). Teachers were randomly assigned to one of two conditions when using our supplemental curriculum materials during a multi-week unit on linear equation solving. In the *person-presentation condition*, each worked example was labeled with a fictitious student's name (e.g., "Morgan's way"), appeared with a simple illustration of the student, descriptions of the solution steps were presented in first person, and explanation prompts used the student's name to refer to each strategy (see Figure 2). In the *strategy-label condition*, solution strategies were labeled with a general strategy label (e.g., "add up way") without illustrations, descriptions of the solution steps were presented in third person, and explanation prompts used the general strategy label to refer to each strategy. All teachers had students compare and discuss the worked examples individually, in small groups and as a class. Students also individually rated the generalizability of each example strategy. Students' conceptual and procedural knowledge as well as procedural flexibility was assessed at the beginning and end of the unit to measure student learning. This allowed us to test a reduced-generalization hypothesis, which predicts person-presentation of strategies could have negative effects on performance by reducing knowledge generalization, in part by reducing perceived generalizability of the strategies, in the context of students comparing and discussing worked examples of multiple strategies throughout an instructional unit.

Method

Participants

Teachers and their students were recruited from a larger study investigating the efficacy of a comparison and discussion of multiple strategies approach to learning algebra. Ninth-grade teachers who had participated in the project the previous school year and were continuing on the project were recruited. Five of the six returning 9^{th} grade Algebra I teachers and their 184 students from two schools in suburban Massachusetts volunteered to participate as part of their regular math instruction. Data from 16 students were excluded (9 were missing pretest data, 7 were missing posttest data). Thus, the final sample included 168 students. The study was deemed exempt from consent by our Institutional Review Board (IRB), which is common when research is implemented by teachers in their classrooms. Therefore, we were unable to obtain student-level demographics and instead report demographics for the schools that students attended. At one school (n = 116 students), 11% of students were eligible for free and reduced-price lunch, 79% were White, 13% Asian, 3% Hispanic, and 3% African American. At the second school (n = 52 students), 6% of students were eligible for free and reduced-price lunch, 89% were White, 4% Hispanic, 3% Asian, and 1% African American.

Design and Procedure

Teachers were randomly assigned to condition. We used a matched randomization method to account for the fact that some teachers taught algebra at different paces (slower paced vs. faster paced) and some teachers taught multiple sections. This resulted in assigning two teachers and their 76 students to the *person-presentation condition* and three teachers and their 92 students to the *strategy-label condition*. Table 1 provides teacher-level information for the characteristics that we attempted to match teachers on in each condition (number of sections, number of students, and course pace).

Teachers in both conditions supplemented their regular math instruction with an example-based curriculum consisting of 9 worked example pairs (WEPs) and materials to support comparison and discussion of them. The only difference between the two conditions was how the strategies were labeled and shown in the WEP materials that the teachers were given. No researchers were present in the classroom for any part of the study.

Teachers administered the same pretest and posttest assessment at the beginning and end of a multi-week unit on solving linear equations. Teachers used the WEPs throughout the unit, spending approximately 20 minutes on each. Suggestions for when to use each WEP were provided based on the teacher's curriculum, and we asked teachers to use at least 8 of the 9 WEPs. After comparing and discussing each WEP, students individually rated the generalizability of each of the two strategies on a worksheet. De-identified student assessments and worksheets were collected from teachers after they had completed the unit. Teachers in the person-presentation condition used an average of 7.2 WEPs (range 6-8) and teachers in the strategy-label condition used an average of 7.9 WEPs (range 6-9).

Supplemental Curriculum

Nine WEPs were designed to facilitate comparison of two strategies by presenting them side-by-side. An example WEP from each condition is provided in Figure 2. Each WEP illustrated the mathematical problem-solving steps for two example strategies to solve linear equations and included prompts for explanation that focused on one of four overarching comparison goals. First, Which is better? WEPs (n = 3) showed the same problem solved using two different, correct strategies, with the goal of understanding when and why one strategy is more efficient or easier than another. Second, Which is correct? WEPs (n = 3) showed the same problem solved with a correct and incorrect strategy, with the goal of understanding and avoiding common errors. Third, Why does it work? WEPs (n = 2) showed the same problem solved with two different correct strategies, but with the goal of illuminating the conceptual rationale in one strategy that is less apparent in the other strategy. Fourth, one How do they differ? WEP showed two different problems solved in related ways, meant to illustrate what the relationship between problems and answers of the two problems revealed about an underlying mathematical concept. Thus, some comparisons focused more on supporting conceptual knowledge (i.e., Why does it work? and How do they differ?), whereas others focused more on supporting procedural knowledge and flexibility (i.e., Which is better?). Which is correct? WEPs likely support both conceptual and procedural knowledge (Durkin & Rittle-Johnson, 2012). Three types of explanation prompts were provided. First, *Understand* prompts (e.g., "How did Riley and Gloria solve the equation?") focused on understanding each strategy in preparation for comparing them. Second, *Compare* prompts (e.g., "Which method is better?") focused on comparing similarities and differences between the two strategies. These first two prompt types prepared students to engage in productive reflection on the final *Make Connections* prompts (e.g., "Come up with another problem where Gloria's method will work. Then solve it using the distributive property. Which method is better?"). Students responded to explanation prompts individually, in small groups, and as part of whole-class discussions. Understand and Compare prompts were discussed as a whole class, and a 'think-pair-share' method was used for the

Discuss Connections prompts. The Discuss Connections prompt appeared on a student worksheet where students wrote down their thinking individually (‘think’) before sharing their ideas with a partner (‘pair’). The ‘share’ phase was a whole-class discussion addressing the Discuss Connections prompt. Each WEP activity ended with a take-away page that provided a short, explicit summary of the instructional goal. Figure 3 provides an example Discuss Connections worksheet and take-away page from the person-presentation condition for the WEP in Figure 2. Teachers participated in a one-week summer professional development institute that introduced them to the supplemental curriculum materials and provided training in the intended use of these materials (see Newton & Star, 2013).

In the person-presentation condition, the strategies were presented as specific examples of how two fictitious students (e.g., Riley and Gloria) solved a given problem (see Figure 2). The strategies were labeled (above each strategy and in discussion questions) using their names and a simple drawing of them was included underneath their strategy. Dialogue depicting each students’ thinking while solving the problem appeared next to their strategy. In the strategy-label condition, the strategies were presented generally as examples of how two students solved the problem. Mathematical labels (e.g., “distribute first”) were used to refer to the strategies and a general description of each problem-solving step was provided. No illustrations were included in this condition.

Measures

Assessment. The researcher-designed algebra assessment consisted of 16 questions (13 multiple-choice, 3 short constructed-response) testing students’ knowledge of solving linear equations. Portions of the assessment had been used in prior studies of comparison learning in algebra (Rittle-Johnson & Star, 2007, 2009; Rittle-Johnson, Star, & Durkin, 2009), but most items were written by the authors or modified to align with the supplemental curriculum. Five questions tested procedural knowledge (e.g., how to solve a linear equation), six questions tested procedural flexibility (e.g., selecting the best first step in a solution), and five questions tested conceptual knowledge (e.g., finding equivalent expressions and like terms). The procedural knowledge and flexibility items were designed to measure transfer of the strategies from the WEPs to new problems. Thus, equations differed from those used for the WEPs, and more than half included novel problem features that required adaptation of the example strategies. An example item from each knowledge sub-scale is provided in Table 2.

All questions were scored as correct or incorrect. The 3 short-constructed response questions were all procedural knowledge questions and requested a numeric answer, which were scored as correct if the student provided the correct answer. Each question on the assessment was worth one point and contributed an equal amount to the overall total score (out of 16). The assessment demonstrated good internal consistency at posttest (Cronbach's α = .78) but was less reliable at pretest (Cronbach's α = .54). The subscales for each type of knowledge at posttest achieved sufficient internal consistency (Cronbach's α > .5, see Table 2) for making group comparisons (Thorndike & Thorndike-Christ, 2010). Thus, we conducted an exploratory analysis examining differences in procedural knowledge and flexibility separately from conceptual knowledge to focus on strategy generalization.

Generalization ratings. Students answered three generalization questions about each of the two strategies presented in each of the WEPs. Specifically, students rated how likely they themselves, another high school student, and a teacher would be to use the strategy in the future on a scale from 1 (*Very Unlikely)* to 5 (*Very Likely),* as in Riggs et al. (2017). The average of the three generalization ratings (yourself, another student, a teacher) was calculated for each strategy, and then averaged across all strategies in the WEPs. However, one WEP was not used by any teacher in the person-presentation condition, so generalization ratings for this WEP were dropped. Three students (2 in the person-presentation condition and 1 in the strategy-label condition) were missing generalization ratings for all WEPs and were omitted from analyses on this measure. One teacher forgot to hand out the generalization questions worksheet for one WEP, so those ratings are missing for these students. When generalization ratings were missing for students, ratings were averaged across available ratings. Cronbach's Alpha was .88. Three WEPs compared a correct and incorrect strategy, so we also calculated the average generalization ratings for the three incorrect strategies separately (Cronbach's α = .72) and compared them to the remaining correct strategies (Cronbach's α = .90).

Data analysis. Our primary analysis tested whether person-presentation impacted knowledge generalization as measured by our posttest assessment. We examined posttest total scores using an ANCOVA model, with condition as the between-subject factor and pretest total scores as a covariate to reduce error variance. We verified that the assumptions of the ANCOVA were met. First, there was no reliable difference in pretest total scores between the two conditions, $t(166) = -.86$, $p = .39$, Cohen's $d = .13$, indicating the covariate was independent of the experimental effect. Second, posttest total scores did not interact with total pretest scores, $F(1, 164) < .001$, $p = .98$, $\eta_p^2 < .001$, indicating that the assumption of homogeneity of within group regression slopes was not violated. Third, QQ plots revealed that posttest total scores within groups were normally distributed. Finally, the error variance in posttest total scores was equal across groups, indicating the assumption of homogeneity of variance was not violated, Levene's test, $F(1, 166) = .10$, $p = .75$. In addition, the data were examined by estimating a Bayes factor, comparing the fit of the data under the null hypothesis to the alternative hypothesis to test for the absence of an effect of person-presentation. As stated in the introduction, potential negative effects of person-presentation of strategies could be counteracted in the context of the current study which supported learning more broadly. A JZS Bayes factor ANCOVA (JASP Team, 2019; Wagenmakers et al., 2018) was estimated using default prior scales (r scale prior width = 0.5).

To test for an effect of person-presentation specifically on strategy generalization, we conducted an exploratory analysis on procedural knowledge and flexibility subscores separately from conceptual knowledge subscores. These analyses on posttest subscores were very exploratory and must be interpreted with great caution.

Finally, we conducted t-tests to examine differences in strategy generalization ratings which were given immediately after studying each target strategy. This allowed us to test for an effect of person-presentation on students' perceived generalizability of the strategies.

Results

Descriptive statistics for students' scores on our assessment and their generalization ratings in each condition are presented in Table 3. Students' posttest scores were similar in each condition. An ANCOVA revealed no statistically significant effect of condition on posttest total scores controlling for pretest total scores, $F(1, 165) = .003$, $p = .96$, $\eta_p^2 < .001$. Pretest total scores were a statistically significant predictor of posttest total scores, $F(1, 165) = 44.23$, $p < .001$, $\eta_p^2 = .21$. An estimated Bayes factor of .18 suggested that the data were 5.56 times more likely to occur under the null hypothesis than the alternative hypothesis. Thus, the data provide substantial evidence (Jeffreys, 1961) in support of the null hypothesis that there was no effect of person-presentation vs. strategy labels on knowledge generalization as measured by posttest total scores.

Exploratory ANCOVA models revealed no statistically significant effect of condition on conceptual knowledge sub-scores, $F(1, 165) = 1.21$, $p = .27$, $\eta_p^2 = .007$, procedural knowledge sub-scores, $F(1, 165) = .11$, $p = .74$, $\eta_p^2 = .001$, or procedural flexibility sub-scores, $F(1, 165) = .56$, $p = .46$, $\eta_p^2 = .003$, controlling for pretest total scores. Pretest total scores did not interact with condition for any of the measures, $F(1, 164) = .00 – .97$, *p's* > .66, $\eta_p^2 < .001$.

We next tested whether person-presentation impacted students' generalization ratings immediately after studying of the studied strategies. First, we compared average generalization scores between the two conditions. As shown in Table 3, students' overall ratings of the generality of the strategies in the person-presentation condition did not reliably differ from students' ratings in the strategy-label condition, $t(159) = -1.00$, $p = .32$, Cohen's $d = .15$. Next, we compared generalization ratings for correct and incorrect strategies separately. Students rated the generality of correct strategies reliably higher than incorrect strategies, $t(156) = 22.14$, $p < .001$, Cohen's $d = 2.45$. There was no statistically significant difference between the two conditions in generalization ratings for correct strategies, $t(159) = 1.64$, $p = .10$, Cohen's $d = .28$, nor incorrect strategies, $t(155) = .22$, $p = .83$, Cohen's $d = .03$. Thus, we also found no negative (or positive) effect of person-presentation on student ratings of the generality of strategies.

Discussion

The current study addressed an important and practical decision when designing worked examples for classroom instruction: should they be presented with or without attribution to particular students? In research studies, they are rarely presented as being generated by particular, fictitious students, but in mathematics textbooks, they sometimes are (Riggs et al., 2015). Recent research indicated that person-presentation of strategies in worked examples can reduce students' perceived generalizability of the strategy and harm strategy transfer, at least when only one strategy is presented in a short-term, controlled setting (Riggs et al., 2015, 2017). In the current study, we explored this reduced-generalization hypothesis in a new context and found that person-presentation vs. strategy labels did not impact student accuracy at posttest or the perceived generalizability of studied strategies.

Previous research on person-presentation of examples suggested that it can harm strategy transfer relative to no person-presentation, in part by reducing the perceived generalizability of a novel strategy (Riggs et al., 2017). Person-presentation of examples may lead students to perceive the strategy as belonging idiosyncratically to someone else and potentially not applicable or useful for the students themselves. This aligns with more general findings that when information is presented as specific to a particular individual rather than a general category, learners are often less likely to generalize the information to new situations (Cimpian & Erickson, 2012; Riggs et al., 2014). Contrary to this prediction, in the current study, students in the person-presentation condition did not perceive the strategies as less generalizable or have reduced accuracy on a posttest.

It is possible that comparison and discussion of multiple strategies played a protective role against potential harmful effects of person-presentation. Studying multiple examples leads to better knowledge generalization than studying a single example (Gentner & Namy, 1999) and direct comparison of the multiple examples supports generalization by highlighting structural similarities of the examples (Gentner & Medina, 1998). Similarly, generating explanations while studying examples results in generalizable knowledge that is not as constrained to specific features of the examples (Siegler & Chen, 2008). Thus, comparison and explanation of multiple examples increases knowledge generalization, which could counteract reduced knowledge generalization that person-presentation might induce. Future research is needed to directly test whether person-presentation has different effects on strategy generalization in different educational contexts and whether attention to surface features plays a role in this effect (e.g., using eye-tracking and measures of memory for irrelevant details).

Potential advantages of person-presentation may also have counteracted potential negative effects of person-presentation. For example, person-presentation of text-based worked examples could enhance educational materials by engaging social learning processes and increasing situational interest, which are particularly relevant and important in classroom contexts. We used fictitious, adolescent-aged students as our model people (rather than teachers, experts or younger children) to increase perceived similarity; perceived similarity to the learner increases situational interest and social learning processes such as social comparison (e.g., Braaksma et al., 2002; Hidi & Harackiewicz, 2000). Anecdotally, both teachers and students have expressed great enthusiasm for the characters, suggesting they do promote situational interest. Future research should include measures of students' interest in the learning materials and of their engagement in social comparison to provide more direct evidence for this hypothesis. It should also investigate whether person-presentation impacts teachers' instruction using the materials.

Alternatively, study design features might account for why we failed to find an effect of person-presentation in the current study. First, the condition manipulation may not have been strong enough. Although we removed all references to particular students, materials in the control condition contained some reference to generic students. Removal of all reference to students as the ones who generated the solutions, as done in Riggs et al. (2015, 2017) could be necessary to reveal an effect of person-presentation. Second, we used sketches of the students (which is common in Japanese textbooks when presenting multiple strategies (Rittle-Johnson, 2019)), but past research sometimes used photographs of individuals (Riggs et al., 2015, 2017). Students in the current study may have been less influenced by person-presentation because they perceived the individuals as being fictitious characters instead of real people. Contrary to this possibility, the negative effect of person presentation was present even when the photograph was omitted (Riggs et al., 2015), suggesting this is not a substantial factor. Third, characteristics of the 5 fictitious individuals in the current materials (adolescent male and female characters from different ethnic backgrounds) may have influenced how much students identified with them, which can influence situational interest and learning from models (e.g., Braaksma et al., 2002; Hidi & Harackiewicz, 2000).

Another possibility for why we failed to find an effect of person-presentation in the current study is that compared to past research, our outcome assessment was not tightly focused on transfer of a specific example strategy to specific problem types, so we may not have detected very specific deficits. In Riggs et al. (2015, 2017), the dependent variable was use of the model strategy on new problems, whereas our measure was answer accuracy. In the current study, too few students showed their work to code for strategy use. Indeed, the condition manipulations did not impact answer accuracy in Riggs et al. (2015). Thus, person-presentation may impact more subtle issues of strategy generalization that do not necessarily lead to differences in test accuracy. In addition, randomization occurred at the teacher level rather than the individual student level, and only five teachers participated in the current study, so we were not able to use multi-level modeling to account for potential teacher-level effects. Differing quality of the teachers' instruction could be more influential on student learning than person-presentation on a subset of classroom materials. More broadly, the practical setting of the study may have introduced too much error to detect the effect of the manipulation, a problem exacerbated by measures of particular constructs (e.g., procedural flexibility) with only moderate reliability. Many of these issues illustrate why it is difficult to establish whether cognitive science research conducted in controlled settings generalizes to the much less controlled setting of classroom instruction implemented by teachers on outcomes typically used by teachers.

It is also possible that idiosyncratic design features of the two previous studies manipulating person presentation of example strategies led to their condition effects. For example, perhaps the model person was not presented in an effective way or even in a harmful way (e.g., causing a split-attention effect). Or perhaps students' generalization ratings simply reflected lower confidence in posttest performance. Generalization ratings were given after solving the posttest problems in Riggs et al. (2017), whereas students in the current study gave generalization ratings after studying each pair of examples, well before completing the posttest. Further, the negative effects of person-presentation have only been identified with text-based examples, not live or video modeling by real people. Thus, it is important to replicate negative person presentation effects on both perceived generalizability of the novel strategy and actual knowledge generalization when the manipulation is implemented in different ways.

Conclusion

Overall, our findings reduce concerns about the potential negative effects of person-presentation of text-based worked examples on students' test performance. We found person-presentation of worked examples was equivalent to using strategy labels without reference to a person on a math test when effective instructional supports were in place (i.e., comparison and discussion of multiple strategies) and integrated into classroom instruction.

Practically, the current findings suggest that person-presentation of example strategies in math textbooks may not influence student learning. It could influence other important outcomes, such as motivation, interest and self-efficacy, which merits further research. Person-presentation may be particularly common in textbooks when multiple strategies for solving the same problem are presented (Rittle-Johnson, 2019), which is a similar context to the one used in the current study. The research outlined above is needed before making additional recommendation for the use of person-presentation in instructional materials. More generally, there is a need for more research examining example-based learning techniques in classroom settings using broad learning outcomes.

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Table 1

Characteristics Used to Match Teachers for Matched Randomization to Condition

Teacher	Pace of Course	Number of Sections	Number of Students
Person-Presentation			
Teacher A	medium-fast	3	53
Teacher B	slow	2	23
Strategy-Label			
Teacher C	medium-fast	2	50
Teacher D	medium-fast	2	37
Teacher E	slow	1	15

Table 2

Example Item from the Assessment and Reliability Information for Each Knowledge Type

Item Type	Cronbach's Alpha at Posttest	Example Item
Conceptual Knowledge	.51	Look at this pair of equations. **Without solving the equations,** decide if these equations are equivalent (have the same answer). $34 = 8(x + 1) + 6(x + 1)$ $34 = 14(x + 1)$ **A) YES (same answer)** B) NO (different answer) C) CAN'T TELL without doing the math D) CAN'T TELL because I need more information
Procedural Knowledge	.60	Solve the equation below for x. $45 = 2(x + 8) + 7(x + 8)$
Procedural Flexibility	.55	On a timed test, which would be the BEST way to start solving the equation below? $8(n + 1) = 2(n + 1) + 12$ A) Gabriella's way: $4(n + 1) = (n + 1) + 6$ B) Jamal's way: $8n + 8 = 2n + 14$ **C) Nadia's way: $6(n + 1) = 12$**

Note. Correct answers are bolded for multiple-choice items.

Table 3

Descriptive Statistics by Condition for Accuracy on the Algebra Assessment and Generalization Ratings

	Condition	
	Person-Presentation *M* (*SD*)	Strategy-Label *M* (*SD*)
Assessment		
Pretest	5.66 (2.70)	5.32 (2.44)
Posttest	8.54 (3.59)	8.35 (3.45)
Conceptual Knowledge	2.76 (1.34)	2.48 (1.40)
Procedural Knowledge	3.22 (1.51)	3.23 (1.41)
Procedural Flexibility	2.55 (1.76)	2.64 (1.48)
Generalization Ratings		
Overall	3.72 (0.42)	3.66 (0.38)
Incorrect Strategies	2.41 (0.86)	2.43 (0.72)
Correct Strategies	4.05 (0.43)	3.93 (0.43)

Juan has been working at a cheese factory in Hilmar, California for 15 years. His job is to fill the cheese vats with milk. He has to be able to figure out how many total gallons of milk have been pumped into a vat. Over a period of 12 minutes, the rate at which milk is pumped into the vat increases steadily over the interval, from 7 gallons per minute to 128 gallons per minute. Juan has to know how many gallons of milk he pumped in total over the 12 minute interval. Here's Juan's strategy:

Steps:
1. Find the average rate. To do this, add the initial rate (rate at minute 1) to the final rate (rate at minute 12) and divide by 2.
2. Multiply the answer from step 1 by the number of minutes.

$$\frac{7 + 128}{2} \times 12 = \mathbf{810} \text{ gallons of milk}$$

Fig. 1. Person-presentation example including name, picture, and background information.

Figure 1. Person-presentation example from Riggs, A. E., Alibali, M. W., & Kalish, C. W. (2015). Leave her out of It: Person-presentation of strategies is harmful for transfer. *Cognitive Science*, *39*(8), 1965–1978. https://doi.org/10.1111/cogs.12224

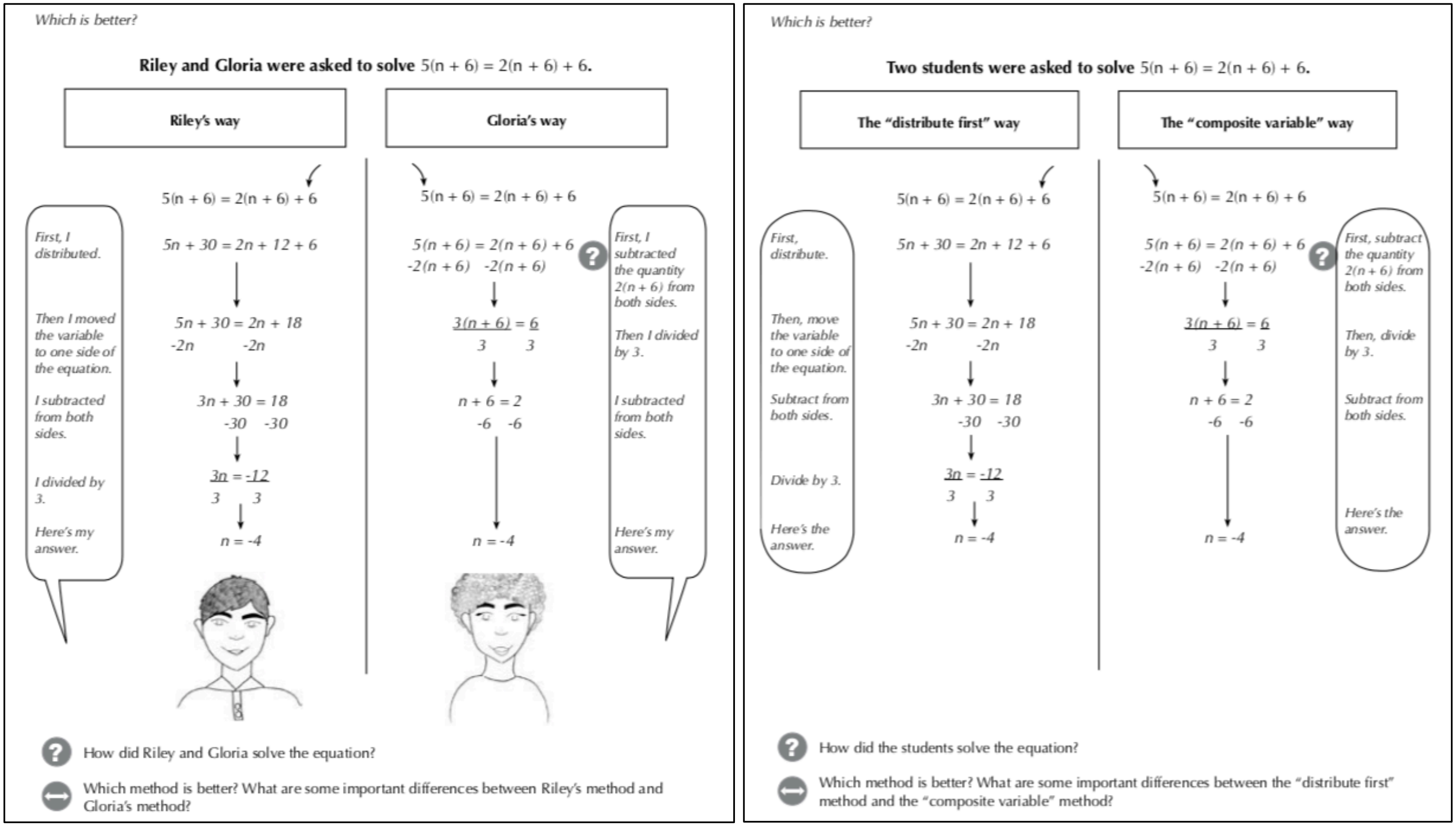

Figure 2. A sample worked example pair from the supplemental curriculum materials used by teachers in the person-presentation condition (left) and no person-presentation condition (right).

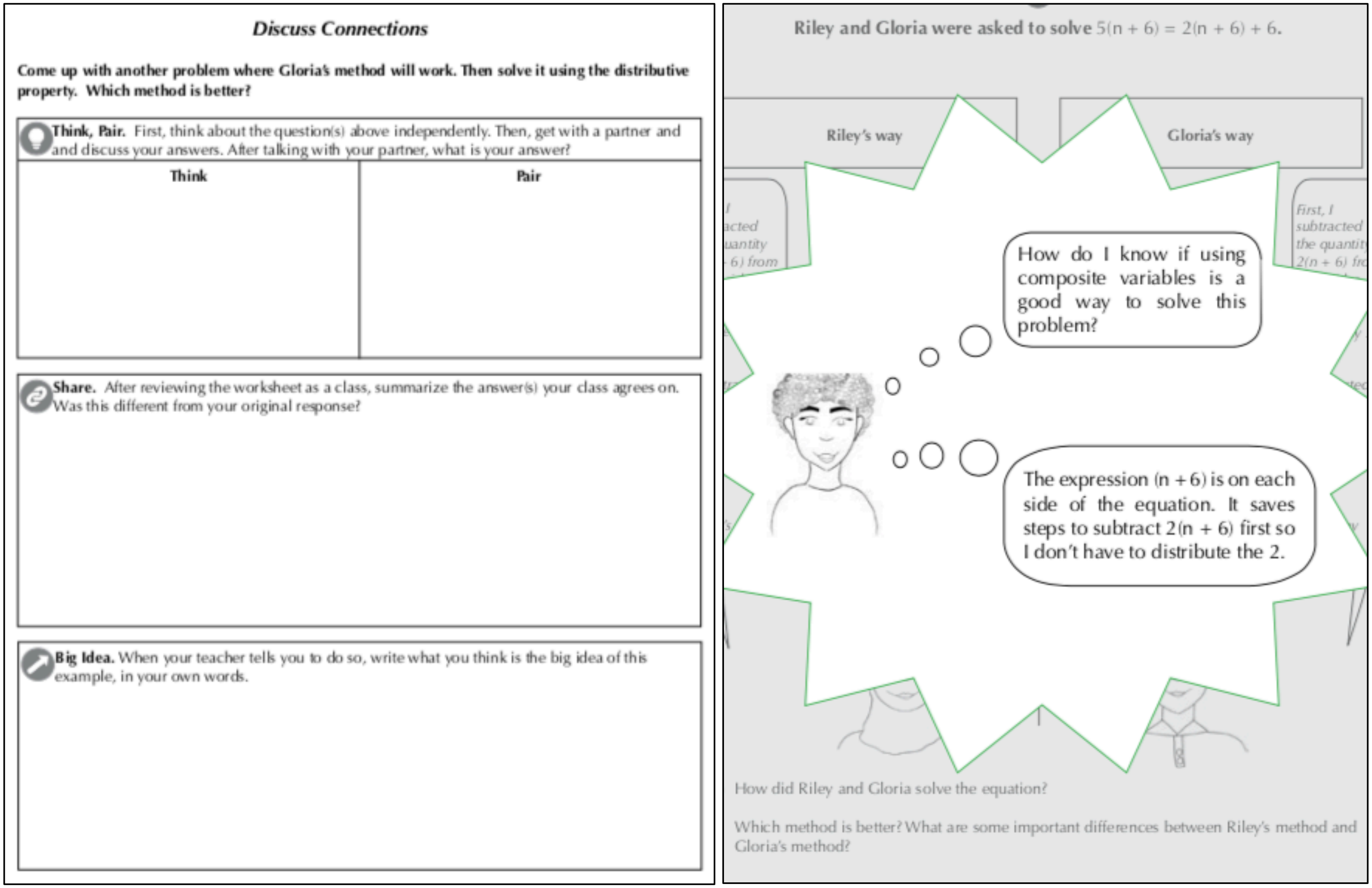

Figure 3. The Discuss Connections worksheet (left) and take-away page (right) from the person-presentation condition for the WEP in Figure 2.