Concrete Instantiations of Mathematics: A Double-Edged Sword

Jennifer A. Kaminski, Vladimir M. Sloutsky, and Andrew F. Heckler
The Ohio State University

What factors affect transfer of knowledge is a complex question. In recent research, we demonstrated that concreteness of the learning domain is one such factor (Kaminski, Sloutsky, & Heckler, 2008). Even when prompted and given no time delay, participants who learned a concrete instantiation of a mathematical concept failed to transfer their knowledge to a novel analogous situation. At the same time, those who learned a generic instantiation successfully transferred.

In a recent article, we (Kaminski, Sloutsky, & Heckler, 2008) reported the results of a study that investigated the effects on analogical transfer of learning one generic instantiation versus learning one or more concrete instantiations. Undergraduate students learned either a generic instantiation of a mathematical concept, or they learned one or multiple concrete instantiations of the same concept. The concrete instantiations, like many concrete examples that are given to students in mathematics class, situated the mathematics in contexts in which answers may seem sensible and learning would likely be easy. For mathematical groups of order 3 (the concept used in the research), such contexts exploit the cyclic nature of groups of this order. When later presented with a novel isomorphic instantiation, those who learned the generic instantiation ably transferred their knowledge to the new situation, whereas those who learned one or more concrete instantiations performed little or no better than chance guessing. Because the concept learned was unfamiliar to our participants and involved basic number properties (e.g., commutativity, associativity, identity, and inverses), and because the test questions were novel and complex, we argue that our findings could likely be generalized to other areas of mathematics. This generalization suggests that students who acquire mathematical knowledge through traditional symbolic formats may be better able to transfer that knowledge to novel isomorphic situations than students who acquired the knowledge through concrete, contextualized examples.

In his commentary, Jones (2009) questions our conclusions on two grounds. First, he argues that our selection of learning material might have favored the generic condition. Second, he suggests that our design taps only into near transfer, "which may be too narrow an instructional outcome to be useful for most mathematics educators" (p. 80). In this response, we address both comments.

Jones brings up a well established fact in research on transfer: Transfer to similar domains is more likely to occur than transfer to dissimilar domains (Gentner, Ratterman, & Forbus, 1993; Holyoak & Koh, 1987; Holyoak & Thagard, 1997; Ross, 1987, 1989). He believes that the generic instantiation used in our research was more similar to the transfer domain than were the concrete instantiations. We disagree. First, we asked participants to rate the similarity of the generic and transfer domains and also the concrete and transfer domains. As with other research, similarity ratings are intended to reflect superficial features of the domains. Therefore, participants' ratings were based on reading short descriptions of the domains and not explicit in-depth training of the domains. No differences in similarity ratings were found.

Jones believes that similarity differences exist because the concrete instantiations depict situations involving remainders that could be interpreted as quantities, whereas the generic instantiation does not. He suggests that differences in transfer performance across learning conditions could be attributed to these characteristics of the learning domains. Although this possibility is real, we have much evidence that it is not the case. If transfer difficulties stemmed from a mismatch between a quantitative instantiation (used in learning) and a nonquantitative instantiation (used in transfer), then transfer from a domain involving only numbers and arithmetic should be at least as poor as transfer from the concrete instantiation. However, we have data documenting successful transfer when the concept is instantiated with numbers.

Jones further brings up relevant points that there are multiple forms or dimensions of transfer. We concur that all or many are important goals of education. As educators, we would like to see facts from the lecture transfer to the test. This is transfer across time and perhaps location. Furthermore, tests may present novel, perhaps more complex, questions. That is also transfer. It would clearly be more difficult than mere recall of facts but would be within the same learning domain. For example, a physics lecture may cover Newton's Second Law of Motion, but test questions may not have been covered in the lecture. Another form of transfer, which was investigated in our research, is transfer to an entirely different, yet isomorphic, domain. This is the type of transfer we see, for example, when students apply knowledge acquired in statistics class to their biology class or perhaps at their future job. With these types of transfer in mind, important clarifications need to be made to Jones's discussion.

First, Jones (2009) believes that our transfer test is a test of near transfer. We disagree. He states that near transfer is a test of *replicative* knowledge, whereas far transfer is "more *applicative* knowledge . . . , or application to problems with different surface characteristics" (p. 84). Our transfer domain had completely different surface characteristics than the learning domains. So it meets the definition of far transfer. But even if our transfer were near, does Jones suggest that the concrete learners who failed our test of transfer would likely succeed on a test of farther transfer?

Jones writes that performance on tests of near and far transfer may vary depending on learning conditions, arguing that in some cases students may perform better on tests of far transfer than on tests of near transfer. He supports his arguments by citing a previous study investigating the effects of note taking on students' subsequent test scores (Peper & Mayer, 1986). In that study, note takers performed worse on tests of near transfer and better on tests of far transfer than non-note takers. Taken at face value, this finding suggests that perhaps learners can perform better on tests of farther transfer than on near transfer. If Jones is applying this possibility to our design, is he suggesting that participants in our concrete learning conditions may outperform those in the generic condition in other possible far-transfer or farther-transfer scenarios even though the reverse pattern of performance was found in our study? In order to consider this possibility, it is important to point out details of the note-taking study and categorical differences between it and ours. First, their tests of near transfer involved syntactic and semantic verbatim recognition and fact retention, whereas the test of far transfer involved problem solving. However, remembering and generalizing are different tasks relying on different processes (cf. Knowlton & Squire, 1993), and it is not obvious as to why the former is identified as near and the latter as far transfer. Furthermore, unlike our study, both tests involved the same domain.

However, even if we take the distinction at face value, Peper and Mayer (1986) state, "The failure to obtain differences between the groups on near transfer tests may be due to ceiling effects" (p. 36). This is a very different pattern of data than ours. The transfer performance for participants in our concrete learning condition was no better than chance guessing. Therefore, it seems extremely unlikely that learners could completely fail to transfer under one set of conditions, but would successfully transfer under conditions of farther transfer.

Jones also argues that our interpretation is distorted because we did not consider more dimensions of transfer. Specifically, our transfer task immediately followed our learning task, and we prompted our participants to transfer. We concur that prompting and lack of time delay will likely improve the possibility of transfer. However, given that participants in all conditions were prompted and received no delay, we ask, how do prompting and lack of time delay explain the transfer differences we found? Is Jones suggesting that explicit prompting and immediate transfer testing act as aids only for generic learners and act as transfer obstacles for concrete learners? Is there a supporting theory for such a possibility?

Our data demonstrate that *even with prompting and no time delay*, those in the concrete learning conditions failed to transfer. At the same time, those in the generic learning condition successfully transferred. When interpreting our study, it is important to remember its goal. The goal was not to test the effects of prompting or the effects of delayed testing. It was to test the effects of concrete versus generic learning instantiations on analogical transfer. We tested a theoretically driven hypothesis that learning a single generic instantiation may result in better knowledge transfer than learning multiple concrete, contextualized instantiations. What we found was that concreteness may be a double-edged sword. The very concreteness or contextualization that may help learners make sense of a given situation may actually hinder their ability to apply the mathematics to novel isomorphic situations.

We believe that the question of what factors affect transfer is a very complex one, and we thank Jones for raising the issue. At the same time, understanding of learning and transfer can be advanced only by sound theory and by theory-grounded empirical findings.

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Authors

Jennifer A. Kaminski, Research Scientist, Center for Cognitive Science, Ohio State University, 208A Ohio Stadium East, 1961 Tuttle Park Place, Columbus, OH 43210; kaminski.16@osu.edu

Vladimir M. Sloutsky, Professor, Center for Cognitive Science and School of Teaching & Learning, Ohio State University, 208C Ohio Stadium East, 1961 Tuttle Park Place, Columbus, OH 43210; sloutsky.1@osu.edu

Andrew F. Heckler, Assistant Professor, Department of Physics, Ohio State University, 191 West Woodruff Avenue, Columbus, OH 43210; heckler@mps.ohio-state.edu