WHEN CONCEPTS ACT CONCRETELY: EXPLORING THE NOTION OF CONCEPTUAL CONCRETENESS IN A NOVEL MATHEMATICAL DOMAIN

by

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Dissertation

Submitted to the Faculty of the

Graduate School of Vanderbilt University

in partial fulfillment of the requirements

for the degree of

DOCTOR OF PHILOSOPHY

in

Psychology

May, 2010

Nashville, Tennessee

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ACKNOWLEDGEMENTS

First and foremost, I would like to thank Bethany Rittle-Johnson for taking a chance on me. I applied to Vanderbilt as the equivalent of an intellectual vagabond with no real experience in the field of psychology. Moreover, it became clear from very early on that Bethany’s and my interests diverged in some important ways. Still, she took me in and managed to give me the structure to help ground my natural tendencies toward the fanciful all while allowing me the freedom to find my fit in the field. For this I will be forever grateful.

Next, I would like to thank my past and present committee members. Georgene Troseth has always been supportive in the extreme and introduced me to many of the works on symbols that provided the foundation for my emerging research program. Meg Saylor has provided invaluable guidance for everything from prepping for my qualifying exams to critiquing my proposed experimental designs. Leona Schauble has encouraged me to think broadly, making sure that I continued to see the value in that unruly side of me that tends toward the fanciful. Finally, Tom Carr has always pushed me to think hard about what exactly a particular manipulation stands to show (or if it actually stands to show anything at all). I have a long way to go before I measure up to the models that they provide, but each should know that I have listened carefully and aspire someday to meet their standards.

I owe thanks to so many of the faculty members of Peabody’s Department of Psychology and Human Development, that there’s hardly room. Dave Cordray has been helpful in so many ways. Teddi Walden has been a great source of advice and humor.

John Reiser has helped me feel like a member of the club and kept me humble by killing me at racquetball. Dan Levin and Julia Noland have always freely given of their time to help me think through problems. Laura Novick helped expand the way that I think of representations. Georgine Pion, Jim Steiger, Howard Sandler, Kathy Hoover-Dempsey, and David Cole have all contributed hours of sage advice that contributed to my professional development. Lest I forget, the administrative staff – in particular Tanya Motley, Sharone Hall, Gemma Bridges-Lyman, Sharana Jones, Donna Baldwin, and Holly Harris – have done much to make my journey smoother than I had any right to expect.

Thanks to all of the graduate students who I've stopped in the hall and asked to lend an ear. Particular thanks goes to my office mates Stephen Killingsworth, Mark Somanader, Gabrielle Strouse, and Nancy Carlisle for being forced to lend an ear over and over again. Their contributions cannot be overstated.

Many other people have helped support this research. Thanks to Vladimir Sloutsky and Jennifer Kaminski for coming up with one of the more versatile experimental tasks I've ever seen. Thanks to Rob Goldstone for the droodles. I am also particularly thankful to two funding institutions. The research reported here was supported in part by the Institute of Education Sciences, U.S. Department of Education, through Grant #R305B040110 to Vanderbilt University and in part by Vanderbilt University Provost's Fellowship.

Finally, I would like thank my friends and family. Aristotle held that man is a polis-animal, that he can never truly be complete outside of society. This is certainly true of myself, and the fact that I can call any of you at a moment's notice be it to laugh, to vent, or just say hello is as fundamentally important to my function as the air that I breathe. To Ms. Julia Michel, special thanks for putting up with me when this thing got me in a mood. I look forward to the years ahead. As for my last thank you, that one goes to Bobbi Cole. What can I possibly say? Thanks Ma. I dedicate this to you.

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CHAPTER I

INTRODUCTION

Learning is often plagued both by a lack of connected understanding and by the inability to transfer knowledge to novel problems. Understanding the processes that affect knowledge change is central both to theories of learning and to the development of effective strategies for overcoming these problems. Much research has attempted to address this problem by investigating different instructional strategies. One alternative approach is to begin with the view that most of the information we seek to communicate to learners is conveyed by symbols. It follows that our choice of symbols may directly affect cognition independently of the instructional strategies that we apply.

The current research starts from this perspective and investigates how manipulating dimensions of the symbols used to instantiate knowledge in a novel domain can affect learning and transfer. Specifically, the current series of experiments investigates an information-based account of concreteness in a complex mathematical domain. Briefly, on this account concreteness is defined as the information – both perceptual and conceptual – that a symbol communicates independently of its currently intended use. Each experiment follows a general theme in which participants are taught a set of abstract rules using various symbol sets and are tested to ascertain the extent to which symbol choice affects learning and transfer. In this case, learning is defined as the ability to successfully perform tasks with the same symbols used for initial training, and transfer is defined as the ability to successfully perform similar tasks using new symbols.

In particular, these experiments evaluate whether conceptual information associated with a symbol can have effects for learning and transfer that parallel those of perceptually concrete symbols (see page 4 for a description).

In this introductory chapter, I first will lay out the basics of this information-based account of concreteness. Then I will briefly review the current literature on perceptual concreteness. In the process I will raise some questions about the concept of conceptual concreteness, putting forth some specific hypotheses that will be addressed by the experiments that follow. Please see Chapter 6 for more in-depth reading on the theoretical framework employed.

The Information-based account of concreteness

There is ongoing debate on the tradeoffs between using abstract versus concrete symbols as tools to promote the building of knowledge structures. Interestingly, this debate often takes place in the absence of a definition for either of the terms 'abstract' or 'concrete'.[1] On the one hand, the idea that concrete materials benefit children's learning – based upon the Piagetian notion that children's thinking is inherently concrete – has a long history in developmental psychology and education (Goldstone & Son, 2005; Uttal, Scudder, & DeLoache, 1997). Bruner expanded the applicability of this concrete to abstract shift to include the thinking novices more generally, including adult novices (see McNeil & Uttal, 2009). Counter to this current, some researchers have argued that concrete symbols may be ill suited to serve as teaching aids when compared to abstract symbols in certain contexts. Though details of the arguments questioning the value of

[1] Please see page 3 for the definitions of 'abstract' and 'concrete' used in the current work.

concrete symbols vary, they converge on the concern that properties of concrete symbols that are not part of the to-be-learned knowledge can impede or corrupt the building of new knowledge structures (Goldstone & Son, 2005; Leslie, 1987; McNeil & Jarvin, 2007; Novick, 1988; Sloutsky, Kaminski, & Heckler, 2005; Uttal et al., 1997).

In investigating concreteness, researchers have focused on how concrete objects or examples affect learning and transfer, at times arguing that concrete symbols can aid learning, and at others arguing that concrete symbols impede learning and transfer. Interestingly, these authors almost never address the question of what concreteness *is*. Indeed, a comprehensive survey of the literature reveals that *concrete* and *concreteness* remain vaguely defined terms. Concrete has alternatively been taken to refer to: a) symbol's physicality as counterposed to the more mentalistic nature of a referent (McNeil & Jarvin, 2007; Uttal et al., 1997); b) the high degree of iconicity of a given symbol in contrast with a more abstract alternative (Goldstone & Sakamoto, 2003); c) the degree of perceptual richness of a given symbol relative to others(Sloutsky et al., 2005); and d) the degree to which a symbol is embedded or situated within a particular context (Gentner & Medina, 1998; Goldstone & Son, 2005; Koedinger, Alibali, & Nathan, 2008). To be clear, these different conceptions of concreteness are not all given as explicit definitions but instead often lie implicit in the writings of various authors, with the operating definitions to be extracted from usage in context. Hence, the construct *concrete*, so frequently conceived of as an important explanatory variable often goes without explicit definition. This means that problems of construct validity are endemic to discussions about the merits and demerits of using concrete symbols to promote learning. It would be helpful to find a definition of concreteness that can help bridge these various conceptions.

One definition that might fulfill this bridging function is that of Kaminski (2006c), who offers a comprehensive definition of concreteness. She uses the term concrete not necessarily to imply tangible, physical objects, but rather as a way to describe something about the degree of contextualization of alternative representations of a given concept:

> "concrete versus abstract is not a dichotomy; it is a continuum where concrete instantiations provide the learner with more information than abstract instantiations. For a given concept, instantiation A is more concrete than instantiation B if A provides the learner with more information than B. Consider the increase in conveyed information as concreteness increases from a stick figure of a person to an elaborate drawing to a photograph to a real person. This conveyed information may be perceptual or conceptual in nature." (p4)

From this perspective, what makes a symbol concrete is its informational load relative to other symbols. On this view, concrete symbols are information rich, and abstract ones are information sparse. Although she notes that some versions of this interpretation of concreteness have been used by other researchers, I find her version to be a clearer, more explicit statement of what often remains implicit in the studies she cites (Gentner, Loewenstein, & Thompson, 2003; Goldstone & Sakamoto, 2003; Goldstone & Son, 2005). By focusing on informational load as the mechanism by which concreteness operates, this account fits squarely within information processing theories of cognition. It can be used to generate hypotheses about concreteness that can be unpacked according to the different types of information involved (e.g. sensory, perceptual, conceptual) and the different demands each type of information may impose.

The effects of perceptual concreteness

One important aspect of this information-based formulation is that it explicitly allows that concreteness – viewed as the informational load of a symbol independent of its present use – can be either perceptual or conceptual in nature. Several lines of research have explored the ways that perceptual information might contribute to concreteness. None, however have attempted to isolate the effects of the conceptual information borne by a symbol from the effects of perceptual information borne by that symbol. The present research is motivated by the question of whether such conceptual information can exert effects that parallel the effects of perceptual information (concreteness) with regard to learning and transfer.

Previous research has shown that perceptual concreteness can significantly affect learning and transfer. Moreover, the evidence suggests that the effects of concrete symbols on initial learning in a domain may differ from their effects on transfer. On the one hand, concreteness has often been shown to be detrimental to transfer. Its effects on initial learning, however, are more varied and seem to hinge upon whether the information communicated by the symbols is aligned with the content of the to-be-learned task.

One particularly instructive study that investigates this differences between the effects of perceptual concreteness on learning and its effects on transfer is Sloutsky et. al. (2005). The experimenters used different symbol sets to instantiate a novel rule-governed learning domain. The first set was labeled the abstract group and used perceptually sparse two-dimensional shapes to instantiate the rules of the task. The second group, the concrete group, instantiated the same rules using screen images of novel, color, 3-D objects (see Figure 1). The use of perceptually sparse 2-D symbols in contrast to perceptually richer screen images of novel 3-D symbols allowed the authors to manipulate the relative concreteness of the stimuli used. Again, concreteness was defined as the amount of perceptual information in the symbols. Both groups did equally well on the learning task, but differed in their abilities to transfer from one symbol set to the other. There was a transfer advantage for abstract symbols, whereby participants

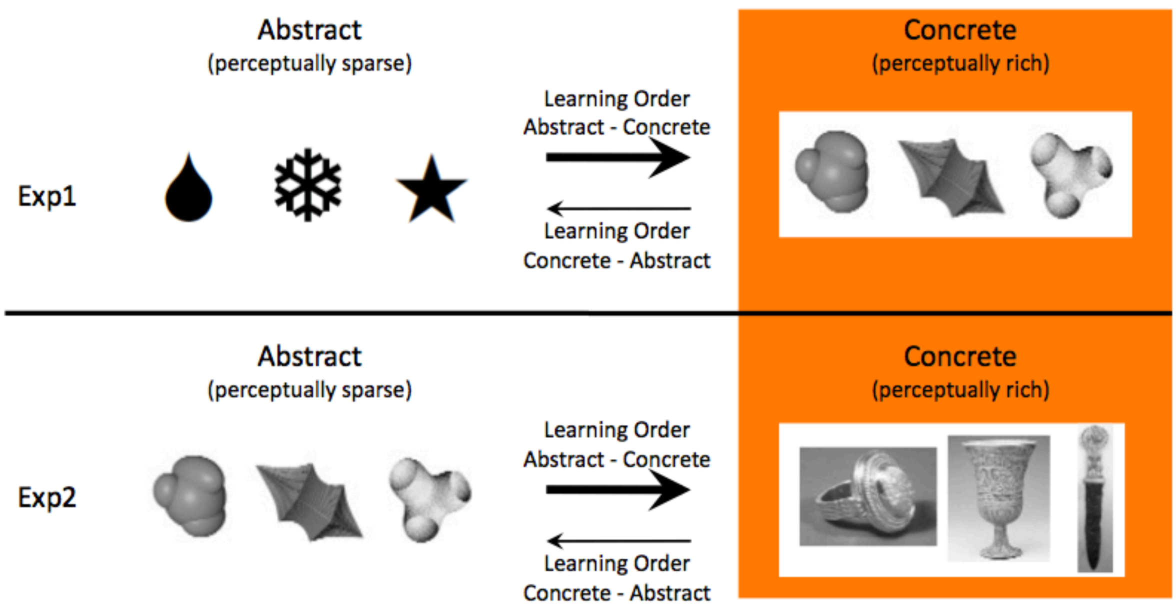


Figure 1. Stimuli from Sloutsky, Kaminski & Heckler, 2005

performed better on concrete symbols if they first learned the task using abstract symbols. By contrast, performance with abstract symbols was equivalent regardless of which symbol set was used for initial learning. Thus, perceptually sparse symbols led to superior transfer relative to more concrete perceptually rich symbols.

In a second experiment, the perceptually sparse group was replaced with specific and identifiable 3-D objects. The authors argued that, in this case, the group of identifiable objects was now relatively more concrete than the 3-D computer generated objects because it was perceptually richer. In this case, the transfer was also better from the more abstract symbols to the more concrete symbols. This finding was especially interesting, because it suggests that concreteness is relative: the same symbols that were more concrete in experiment 1 were more abstract in Experiment 2. These authors have used this paradigm in multiple ways to make a strong case that perceptually concrete symbols do not promote transfer as well as abstract ones (Kaminski, Sloutsky & Heckler, 2005; Kaminski, Sloutsky & Heckler, 2008, Kaminski, Sloutsky & Heckler, 2009). Other researchers have converged on this finding (Goldstone & Sakamoto, 2003; Novick, Catley & Funk, in preparation; see Table 1).

In contrast to the generally negative effects of concreteness on transfer, the effects of concreteness on initial learning in a domain are not so clear. Some have found that increasing the perceptual richness of a symbol can actually impede its ability to promote learning (McNeil, Uttal, Jarvin, & Sternberg, 2009; Sloutsky et al., 2005). For instance, the third experiment of Sloutsky et. al. found that increasing the perceptual richness of symbols impeded learning relative to perceptually sparse symbols of the same shape. Others have found that learning with perceptually concrete symbols can aid initial learning in certain contexts (Goldstone & Sakamoto, 2003; T. Martin & Schwartz, 2005). One factor that has been shown to effect whether or not perceptual concreteness aids learning is whether or not the concreteness is aligned with the to-be-learned task. For instance, using a modular arithmetic task, Kaminski, et. al. (2005; 2006a) showed that perceptually concrete symbols boosted initial learning relative to abstract symbols when those symbols were aligned with the structure of the to-be-learned task. More generally, it has been argued that concrete representations that communicate relevant aspects of a to-be-learned task can promote learning (Goldstone & Sakamoto, 2003).

The case for conceptual concreteness

One shortcoming of the research on concreteness in general is that it often fails to distinguish between the effects due to sensory information and the effects due to conceptual information. For instance, Sloutsky et. al. (2005) argued that the effects of concreteness were due to differences in the sensory aspects of the symbols – black and white 2-D symbols vs. colorful 3-D symbols. By contrast, the perceptually rich group in experiment 2 was deemed to be more concrete *because* the objects were recognizable. In this case, the 'percepts' of the ring, cup, and knife (unlabeled but clearly recognizable in Sloutsky et. al., 2005; see Figure 1) are what add information. It seems, however, that these percepts may have exercised their effects by activating conceptual information.

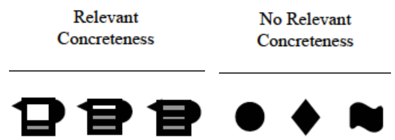
From the outset, a holistic view of perception might predict that the concepts associated with a symbol should affect concreteness. At the very least, it is clear that concepts and percepts can be tightly linked. This percept-concept link is evidenced by well-documented differences in perception of identical stimuli based on expertise (Chi & Ceci, 1987; Goldstone, 1994; Rosch, 1975). If percepts are more than mere sensation (Kellman et al., 2008) then we should expect that much of what affects the perception of a symbol might go beyond mere sensation and include prior knowledge. This may have been the case in Experiment 2 of Sloutsky et. al. (2005).

As an illustrative example, Goldstone and Sakamoto (2003) provides another notable case in which effects ascribed to 'perceptual' concreteness of symbols may have

Table 1

Articles Reviewed Investigating the Effects of Perceptual Concreteness

Author	Participants	Manipulation	Findings – Learning	Findings – Transfer
Goldstone & Sakamoto, 2003	Undergraduates	Exp I. Corresponding agents either the same color or cross-mapped Exp II. Blue marble in first simulation corresponds to either blue marble or black/white soccer ball in the next simulation Exp III. very similar to II. Exp IV. ants and food simulation was either abstract (dots, blobs) or concrete (ants, apples).	Exp IV. Low performers showed better initial learning with the concrete symbols	Exp I-III Low performers transfer better when receiving cross-mapped or dissimilar agents first. Exp IV Low performers transferred better when given the abstract version.
Goldstone & Son, 2005	Undergraduates	4 training conditions: (1) concrete, (2) abstract, (3) concreteness faded from abstract→concrete, and (4) concreteness faded from concrete→abstract	Initial Learning was best with concrete groups	Transfer was better with the abstract group. If training employed concreteness fading in either direction (either abstract→concrete or concrete→abstract) then transfer was improved
Kaminski, 2006c	Undergraduates	X manipulations of Many perceptual concreteness	Across X experiments, Abstract symbols generally lead to better learning. Abstract symbols always lead to better transfer.	
Kaminski, Sloutsky & Heckler, 2008	Undergraduates	Abstract vs. 3 alternative sets of concrete symbols	Initial Learning was equivalent across abstract and concrete symbols	Transfer was better when initial learning was with concrete symbols.

Kaminski, Sloutsky, & Heckler, 2005	Undergraduates	2X2 manipulation of relevant perceptual concreteness and perceptual richness	Initial learning was better with relevant concrete symbols than those with no relevant concreteness.	Symbols with no relevant concreteness supported better transfer. Perceptually sparse symbols with no relevant concreteness supported transfer best of all.
Kaminski, Sloutsky, & Heckler, 2006a	6th grade students	Abstract vs. Relevant Concrete Generic  	Initial learning was better with relevant concreteness.	Transfer was better with the abstract set.
Kaminski, Sloutsky, & Heckler, 2006b	Undergraduates	Exp I. Abstract vs. Relevant Concrete vs. Irrelevant Concrete Exp II. Abstract vs. Relevant Concrete Half the sample was given object correspondences between the learning and transfer instantiations  	Exp. I Initial learning with different tasks led to differences in recognition of the deep structure of the task.	Exp II. Those who learned with relevant concrete symbols needed to be given object correspondences to transfer. Those who learned with abstract symbols transferred regardless.
Martin & Schwartz, 2005	9 & 10 year old children	Physical materials vs. Pictorial materials	Learning was best with physical materials	N/A

McNeil, Uttal, Jarvin & Sternberg (2009)	4th 5th and 6th grade students	Exp 1: Perceptual rich dollar bill manipulatives vs. No manipulatives Exp 2: Perceptually rich vs. bland dollar bills vs. no manipulatives	Exp 1: No manipulatives group solved problems more accurately Exp 2: Performance was worse with rich manipulatives than other conditions. Perceptually rich symbols led to fewer *conceptual* errors relative to the other two.	N/A
Novick, Catley and friends (under review)	Undergradu ates	Photographs of recognizable species (concrete) vs. Written labels of novel biological species (abstract)	No differences in initial learning.	Transfer was better when initial learning was with abstract symbols
Petersen & McNeil, 2008	3 yr-olds	2 X 2 manipulation of familiarity and perceptual richness of the symbols used.	Found familiarity X richness interaction. Perceptual richness hurt performance with familiar objects, but improved performance with unfamiliar objects.	N/A
Sloutsky, Kaminski, & Heckler, 2005	Undergradu ates	Exps 1 & 2: Perceptually concrete or abstract symbols given for training, and learning and transfer from one type to the next is assessed for various presentation orders. Exp 3: Perceptually sparse vs. Perceptually rich symbols	Exp 1: Learning was better with more abstract groups relative to concrete ones. Exp 3: Learning was superior with perceptually sparse symbols	Exp 1: Transfer was better from more abstract to more concrete. Exp 3: N/A

actually emerged from conceptual knowledge activated by those symbols. In this experiment, participants learned a novel competitive specialization procedure using identical cover stories about ants foraging for food. The process was instantiated using either concrete symbols that used iconic pictures of ants and fruit or abstract symbols that represented ants as dots and food as nondescript blobs (see Figure 2).

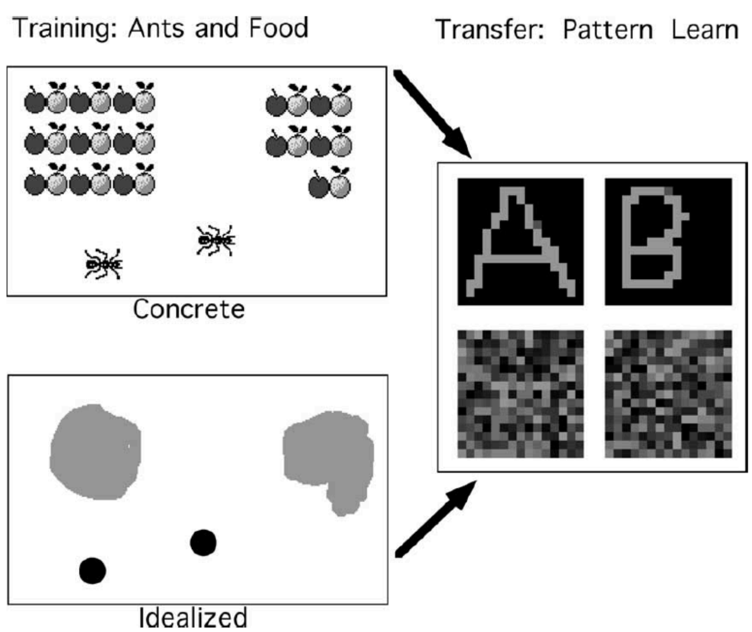


Figure 2. Stimuli from Goldstone & Sakamoto (2003).

In this experiment, the concrete instantiation supported better initial learning, but the abstract instantiation supported better transfer to a novel context governed by the same processes. The researchers concluded that learning with concrete symbols encouraged learners to develop context bound interpretations that impeded transfer to new situations Notably, participants in the concrete training condition were more likely to give domain-specific, anthropocentric interpretations of the ants' behavior (e.g. "one ant scares the other away" or "the ants are tempted by both food piles" for the concrete group versus "animals move quickly to food they are close to" or "It helps to make an ant move quickly at first and then more slowly" for the abstract group). These analyses support the contention that students were using conceptual information contained in the symbols (thinking in terms of little ant communities) when trained using concrete versus abstract symbols. It appears that the perceptual information that counted in this case was the degree of symbolic iconicity with real ants and food. This apparently operated via activating the anthropomorphic ant concepts instead of via some difference in raw sensory processing between instantiations.

Overview of current experimental questions

The current experiments were designed to isolate the effects of conceptual information in arbitrary symbols from perceptual information using the modular arithmetic task from Sloutsky, Kaminski & Heckler (2005). Specifically, three hypotheses were tested:

Hypothesis 1: Concreteness refers to the content communicated by a symbol, and this content can be either perceptual or conceptual in nature. Prior research has demonstrated the effects of perceptual information. The current research sought to isolate the effects of the conceptual information contained in symbols over and above the perceptual information that they contain. If the conceptually concrete symbols in these experiments show parallel patterns for supporting learning and transfer relative to abstract symbols (i.e. aiding initial learning but harming transfer relative to abstract symbols) then there is a face-valid argument that conceptual information acts similarly to perceptual information in this context (see Son & Goldstone, 2009). The implication would be that such conceptual information is indeed functionally concrete. The three experiments below tested this hypothesis by comparing the efficacy with which various conceptually concrete symbol sets promote learning and transfer with the Sloutsky task relative to abstract symbol sets.

Hypothesis 2: Alignment of conceptual concreteness with to-be-learned content is an important factor. One major factor determining the effects of conceptual concreteness should be the degree to which relevant associated knowledge is aligned with the to-be-learned structure. To the degree that the information is aligned with structure, then it should facilitate learning (see Bassok, Chase, & S. A. Martin, 1998; Kaminski et. al., 2005, 2009) At the same time, this alignment is expected to effect transfer negatively because it is expected to lead learners to focus on surface knowledge instead of on the deep structure of the task (see Kaminski et. al., 2006b). To the degree that such information is actually misaligned or directly contrary to the to-be-learned content, then there should be competition between the to-be-learned association and prior knowledge. In such a case, a symbol that brings to mind misaligned information may demand an extra sort of inhibition to support learning. Because this misalignment has not been tested before, there were no strong predictions relative to transfer. Experiments 1 & 2 tested this hypothesis using Arabic numerals.

Hypothesis 3 – Conceptual Concreteness should be manipulable. If concreteness depends in part on the strength of prior associations, then it follows that there should be an *a priori* expectation that it is manipulable. On the long-term view, such experience-based differences would be predicted by well-documented differences in perception of identical stimuli based on expertise (see Bransford, Brown & Cocking, 1999; Chi &

Ceci,1987). In the short term, it seems that contexts that either strengthen or weaken preexisting links between a given signifier and the content associated with it should modulate the effects of concreteness. This has not been explicitly studied before, but there is an *a priori* case for such effect that follows from an information-based conception of concreteness. Experiments 2 and 3 examined this prediction for relevant concreteness, both by manipulating what prior knowledge is activated using well-known symbols (exp 2) and how much prior knowledge is developed using novel symbols (exp 3).

CHAPTER II

EXPERIMENT 1

Experiment 1 sought direct evidence that conceptual content associated with a symbol could render a symbol concrete. Specifically, I investigated whether or not Arabic numerals function similarly to perceptually concrete symbols when teaching mathematically governed concepts. It is arguable that Arabic numerals are rendered concrete relative to many other symbols due to the information that they automatically communicate to adult learners. That such information is overlearned to the point of automaticity is evidenced by various examples of mathematical stroop effects (Bull & Scerif, 2001; Washburn, 1994) as well as neurological studies that show specialized brain activation in response to exposure to symbolic number (Cohen Kadosh & Walsh, 2009; Nieder & Dehaene, 2009). Theoretically, it follows that Arabic numerals may have acquired some experience-based concrete properties for adult learners. Experiments 1 & 2 explored this possibility, searching for similar effects on performance for numerals as were observed for perceptual salience in Sloutsky et. al. (2005). In particular, the pattern is for concrete symbols to support initial learning, but for there to be a transfer advantage for initially learning with abstract symbols compared to concrete ones. The design was geared to address the question whether manipulating a symbol's alignment with prior knowledge and usage affected learning and transfer over and above perceptual attributes.

The experimental methodology closely followed that of Sloutsky et. al. (2005). Instead of using stimuli that varied on perceptual richness to manipulate concreteness, the experiment used the conventional symbols 0, 1, and 2 as concrete symbols and the abstract symbols ⚑, ●, and ♦ as learning stimuli for instantiating the domain of addition modulo 3.[3] As explained above, the numerals were considered to be the more concrete of the two groups, because people's experience with them was expected to be very powerfully associated with prior mathematical knowledge, whereas the perceptually sparse shapes of the abstract set weren't expected to have any strong and necessary associative connections with information relevant to the task.

The domain was instantiated using the following three alternative symbol sets: an *aligned* concrete set (0, 1 & 2) with 0 defined as the identity; a perceptually identical *misaligned* concrete (2, 1 & 0), with 2 defined as the identity; and an *abstract* set (⚑,●, ◆), with ⚑ defined as the identity. The aligned set was so named because use of 0 as the identity element easily aligns with participants' prior integer addition schemas. The misaligned set's use of 2 as the identity was expected to compete with prior addition schemas (see figure 2). The abstract set was neither aligned nor misaligned with the to-be-learned domain. It was hypothesized that familiarity with the conventional uses of previously known symbols would distract learners from underlying structure, with diverging effects for learning and transfer.

Hypotheses

Learning. It was expected that the aligned symbol set would promote superior learning relative to both the abstract and misaligned sets. On its surface, modular arithmetic shares several similarities with integer arithmetic. Hence, the aligned set was

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[3] Mathematically speaking, a commutative group is a set on which a law of composition is defined, which is associative and has an identity element, and such that every element has an inverse (Artin, 1991).

expected to provide a learning boost by activating a preexisting addition knowledge, which communicates relevant aspects of the to-be-learned information (see Kaminski et. al., 2006a). The misaligned symbols were expected to impede learning relative to the aligned set. Although this set was also expected to activate integer arithmetic schemas, the object correspondences were misaligned between the set as used in the experiment and as used in typical integer addition (e.g. 2 is the identity so 2 and 0 yield 0 for the misaligned symbols,). This misalignment with the preexisting arithmetic schema was expected to require inhibition of prior knowledge, depressing learning. Finally, the abstract symbol set was not expected to activate any relevant knowledge, so it was anticipated to support initial learning superior to initial learning with misaligned symbols, though inferior to initial learning with the aligned concrete set.

Transfer. Transfer from the abstract to the aligned set was expected to be superior when compared to transfer from the aligned to the abstract set. Initial learning with the abstract group was expected to allow the learner to acquire rules in a more decontextualized manner, supporting knowledge acquisition more in tune with the deep structure of the task. In contrast, initial learning with the aligned symbols was expected to impede transfer relative to initial learning with the abstract set, because the knowledge generated was expected to be tied to knowledge imported from preexisting integer arithmetic schemas (Goldstone & Sakamoto, 2003; Sloutsky et al., 2005). Integer arithmetic conflicts in some aspects with the structure of modular arithmetic even for aligned numerical symbols (most notably, arithmetic modulo 3 has a cyclical nature and lacks a well defined property of ordinality). Hence, the learning gains due to alignment were expected to come at the expense of a somewhat impoverished understanding of the deep structure of the task, imposing a cost on transfer performance.

As for the transfer between abstract and misaligned symbols, predictions were less certain. Again, initial learning with the abstract group was expected to support knowledge acquisition that is not tied to the specific symbols and therefore more facilitative to transfer. On the other hand, it might be that those who proved to be successful at learning with misaligned symbols despite the misalignment might actually acquire a better understanding of the underlying structure due to the task difficulty. For these participants, the misaligned symbol set might even support transfer performance as well as abstract symbols.

Method

Participants

Consent was obtained from 56 adults from the metropolitan Nashville community recruited through Vanderbilt's SONA system. One participant was dropped from the study for failure to complete the experimental tasks. The final sample (n = 55) included many participants who were not Vanderbilt students (31%). Mean age for the sample 25.5 years (range 18 to 55, SD = 7.0). Participants were paid $10 for participation.

Design

The experiment was conducted in 5 phases presented over approximately one hour: 1) training with one symbol set (the learning set), 2) testing with that symbol set, 3) training with a second symbol set (the transfer set), 4) testing with that symbol set, and 5) a series of follow up questions about the correspondences between the first and second symbol sets and strategies used to produce answers.

The initial symbol set always served as the *learning* set for each participant, and the second set served as the *transfer* set. Participants were randomly assigned to one of four orders of symbol set presentation: abstract-then-aligned (n = 13); aligned-then-abstract (n = 14); abstract-then-misaligned (n = 14); or misaligned-then-abstract (n = 14).

Materials and Procedure

The training phase introduced the rules governing the symbol systems. The relations between the elements of each symbol set were governed by the rules of a commutative algebraic group of order three and isomorphic to the integers under addition modulo three. The rules of the various sets are depicted in Figure 3. The goal of training was to teach the explicit rules presented in Figure 3 and to provide implicit exposure to the mathematical properties governing the set (e.g., associativity, commutativity, and the existence of the identity element and of inverse elements). Training for each of the three symbol sets was similar.

The rules were introduced by fictional characters using separate cover stories for the abstract and the concrete symbol sets. The abstract set was presented as a symbolic language discovered on an archaeological search, per Sloutsky et. al. (2005). In this language, pairs of different symbols combined to yield a resulting symbol. Concrete (numeral) sets were presented as a new type of mathematics recently invented by a fictional mathematician. In this system, different pairs of numerals are "transformed" to yield a resulting numeral. Note that the concrete cover stories neither used typical names

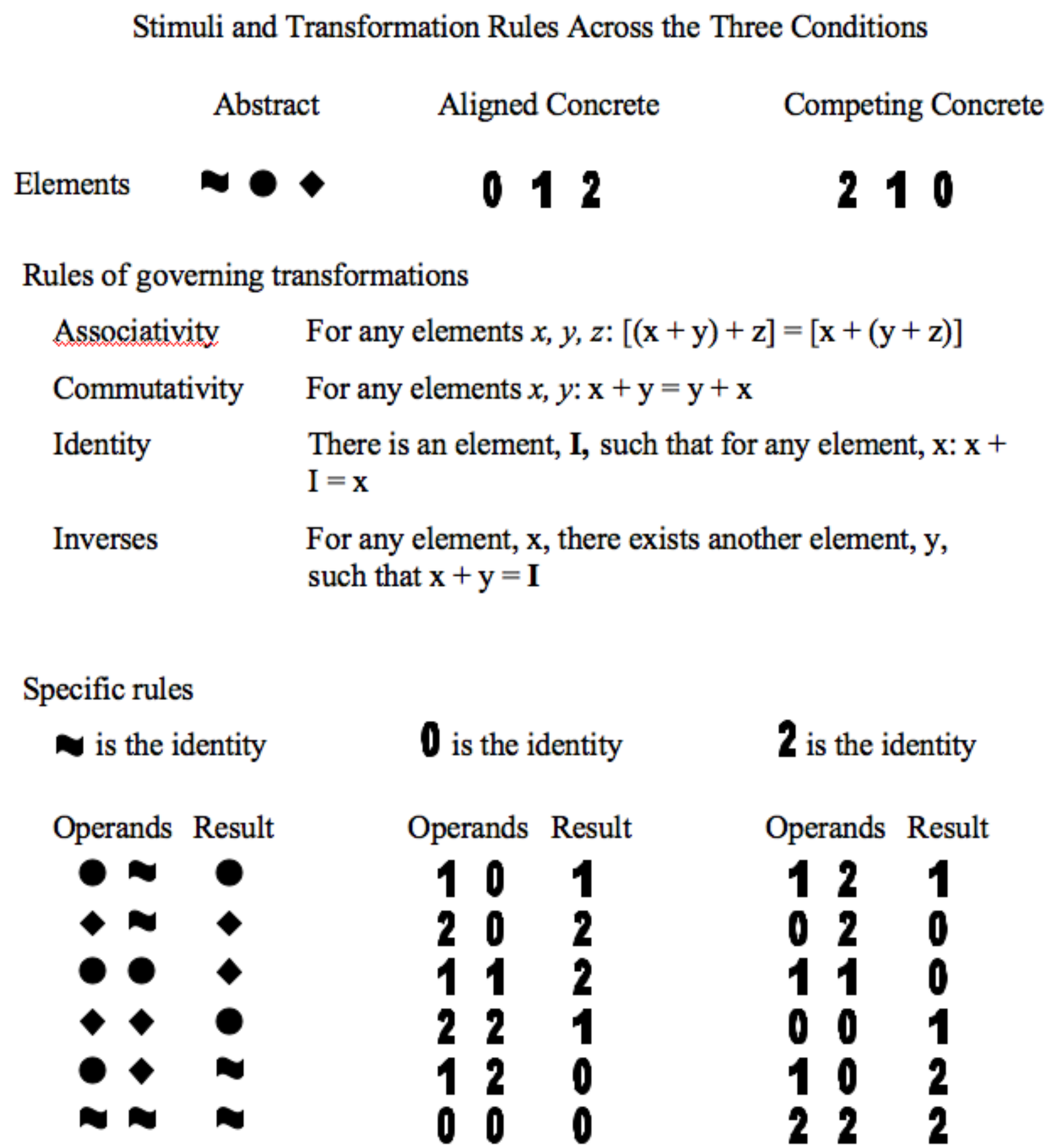


Figure 3. Rules governing the modular arithmetic task

for mathematical operations (e.g. add, subtract, etc.) nor used canonical symbols denoting mathematical operations (e.g. +, - , ∗, /, etc). Cover stories for aligned concrete and misaligned concrete sets were identical except for the object correspondences.

Participants completed the experiment individually, with all training and testing stimuli presented on a Mac PowerBook G4 laptop computer using Superlab 4 software (Cedrus Corporation, 2007). Participants progressed through the experiment in a self-paced manner. During the training phase, the governing rules were presented one at a time and stated explicitly. For instance, the abstract training told participants that combining the symbols ♦and ●always results in the symbol ⚑. Instead of explicitly mentioning mathematical properties of the operations (e.g. commutativity), the training session mentioned that ♦,● “gives the same result as” ●,♦. The concrete training sequence was very similar, with the differences note above (see Appendices A, B, and C for details).

Each test phase consisted of 23 multiple-choice problems that required participants to apply the previously learned rules (see Figure 4 for sample problems). Due to a programming error, accuracy data for one of the items was not collected for half of all participants, so data analysis was based on 22 questions. For all symbol sets, the test items were completely isomorphic and were presented in the same order. Responses for the test phase items were recorded by computer.

A series of follow up questions was asked at the conclusion of the second test phase. Students were asked a) to indicate the appropriate object correspondences between the two learned symbol sets, b) to explain how they managed to determine each object correspondence, c) to indicate whether or not the task generally reminded them of something they’d had experience with in the past, d) to indicate whether or not the task reminded them of arithmetic and arithmetic properties specifically. Responses for the follow up questions were recorded by paper and pencil.

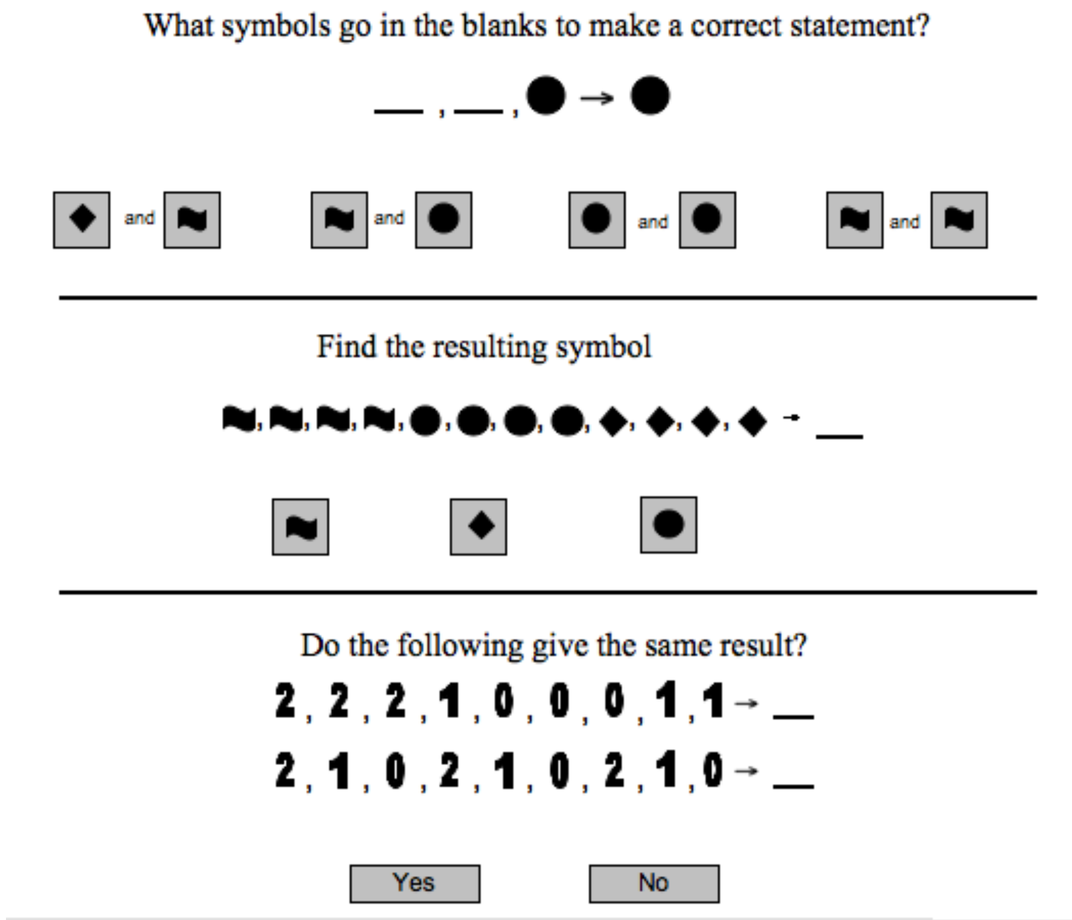


Figure 4. Sample problems used in the assessment

A series of follow up questions was asked at the conclusion of the second test phase. Students were asked a) to indicate the appropriate object correspondences between the two learned symbol sets, b) to explain how they managed to determine each object correspondence, c) to indicate whether or not the task generally reminded them of something they'd had experience with in the past, d) to indicate whether or not the task reminded them of arithmetic and arithmetic properties specifically. Responses for the follow up questions were recorded by paper and pencil.

Responses to the follow-up questions included in analysis were coded as described in the results section. Independent raters coded 20% of responses with 93% agreement.

Measures

The dependent variables were the numbers of questions answered correctly for each symbol set. Initial learning was indexed by overall accuracy on the 1st test block. Transfer was measured by examining order X symbol type interactions in a repeated-measures model with symbol type (abstract or concrete) as a within-subject factor and order (abstract first or concrete first) as a between-subjects factor. Such an interaction can indicate that there were differences between symbols regarding the incremental boost that learning with the first symbol set gave to accuracy for the symbol learned second, and follow-up tests are necessary to determine if the interaction indicates this specific effect. Main effects for symbol and order were of secondary interest. Symbol effects indicate differences in accuracy between symbol types when performance on each is collapsed across order. Existence of order effects would indicate that learning with a particular symbol type first boosts overall accuracy across both trials in a given learning order compared to the alternative learning order.

Results

In presenting the results, I first describe accuracy for initial learning with the different symbol sets. I follow this summary with an analysis of transfer. Finally, I explore the potential effects suggested by some of the exploratory follow-up questions.

Initial Learning

A univariate ANOVA was conducted to assess the effect of symbol type on initial accuracy. Initial learning score was the dependent variable and symbol type (abstract, aligned, and misaligned) served as the 3-level independent variable. Contrary to expectations, there were no differences in initial learning between groups $F(2,55) = .81$, $p = .45$, $\eta^2 = .03$. In fact, the nonsignificant mean difference that appeared between initial learning on abstract and aligned symbols was in the wrong direction, with highest accuracy in the abstract condition (see Table 2),.

Table 2.

Initial Learning Accuracy For Each Symbol Type

Initial Learning (full sample)			
1st Block	Abstract	Aligned	Misaligned
Mean (SD)	16.48 (5.20)	14.57 (5.64)	14.71 (5.43)

Transfer

Ceiling effects. There was an apparent problem with a ceiling effect for the measure that could potentially affect transfer scores. Sloutsky et. al. (2005) found similar results and performed a supplementary analysis with lower performers to account for the influence of ceiling effects, and found that the effects of the experimental manipulation were larger for lower performing participants than for the full sample.

To assess the issue in the current experiment, a conservative estimate of a 95% confidence interval around a ceiling score of 22 was created using a binomial approximation to the mean: $CI = p \pm 1.96\sqrt{\frac{p_0(1-p_0)}{n}}$. Here p_0 – the probability of getting the problem correct by chance – was set to .25 (This was a conservative estimate because three of the items on the assessment actually had $p_0 = .5$). This yielded a CI of 18-22 for a perfect score. In each of the four possible conditions, at least 43% of participants scored within the confidence interval for perfection (see Table 3). Thus, it does seem that the posttest had a compromised ability to show increases from the learning to transfer trials. To deal with the compressed variability on the top end of the spectrum, two sets of data analyses were conducted for transfer: one based on the full sample and one based on the lower performing participants after conducting median splits for initial learning accuracy for each of the four possible learning orders. This median split on initial learning scores is appropriate for analysis of transfer, because it amounts to selecting on an independent variable (initial learning accuracy), which does not introduce bias (King, Keohane & Verba, 1994). The transfer analyses reported below will generally be those conducted on the full sample with supplemental commentary on low performing sample where appropriate.

It was expected that the abstract symbol set would support better transfer to the aligned set than vice versa. It was less clear if the abstract symbols would support better transfer to misaligned symbols than vice versa. Because no participant saw both aligned and misaligned symbol sets, evaluation of transfer is broken down by type of concrete symbol learned. This ultimately results in analysis of two separate pairs of conditions: a) abstract-then-aligned vs. aligned-then-abstract; and b) abstract-then-misaligned vs.

misaligned-then-abstract. Two sets of repeated measures ANOVAs were conducted, using symbol type (abstract or concrete) as a within subjects factor and order (abstract first or concrete first) as a between subjects factor. These separate analyses paralleled those conducted in Experiments 1 and 2 of Sloutsky et. al. (2005).

Table 3.

Percent of Participants scoring ≥18 on Initial Learning Set by Condition

Order 1st 2nd	Abstract→ Aligned	Aligned→ Abstract	Abstract→ Misaligned	Misaligned→ Abstract
% Scoring ≥18	69.2	42.9	42.9	42.9
Median cut score	<20	<18	<18	<16
Proportion of sample remaining	6 of 13 46.2%	8 of 14 57.1%	8 of 14 57.1%	8 of 14 57.1%

Abstract vs. Aligned. Overall, there was no main effect for order or symbol, $F(1,25) = 1.32$, $p = .26$, $\eta^2 = .05$ and $F(1,25) = .41$, $p = .53$, p = .26, $\eta^2 = .30$, respectively. As predicted, there was a significant symbol X order interaction, $F(1,25) = 10.80$, $p < .01$, $\eta^2 = .30$. Follow up tests indicated that there were significantly larger differences in performance for aligned symbols as a function of learning order than for abstract symbols. The participants who learned with abstract symbols first performed significantly better on the aligned symbols than those who learned the aligned symbols initially, $t(25)$

= 2.39, p = .03 (see Table 4). In contrast, there was no difference in accuracy for abstract symbols between learning orders, $t(25) = .13$, $p = .90$. In summary, learning with abstract symbols first boosted later learning with aligned symbols, but learning with aligned symbols first did not aid later learning with abstract ones (see Figure 5). As expected, these effects were even stronger and in the same direction for lower performing participants.

Table 4.

Accuracy by Order for Different Learning Groups

Type of Concrete Symbol	Symbol Type	Order	
		Abstract 1st	Concrete 1st
Aligned	Abstract	17.15 (5.34)	17.43 (5.42)
	Aligned	19.08 (3.93)	14.57 (5.64)
Misaligned	Abstract	15.86 (5.19)	18.07 (3.75)
	Misaligned	16.43 (6.16)	14.71 (5.43)

The transfer results with the abstract-aligned pairing closely resemble the transfer results from the abstract-perceptually rich comparison of Sloutsky et. al., 2005. This serves as one instance in which the conceptual content tied to an arbitrary symbol (Arabic numerals) had very similar effects on learning and transfer as perceptual concreteness.

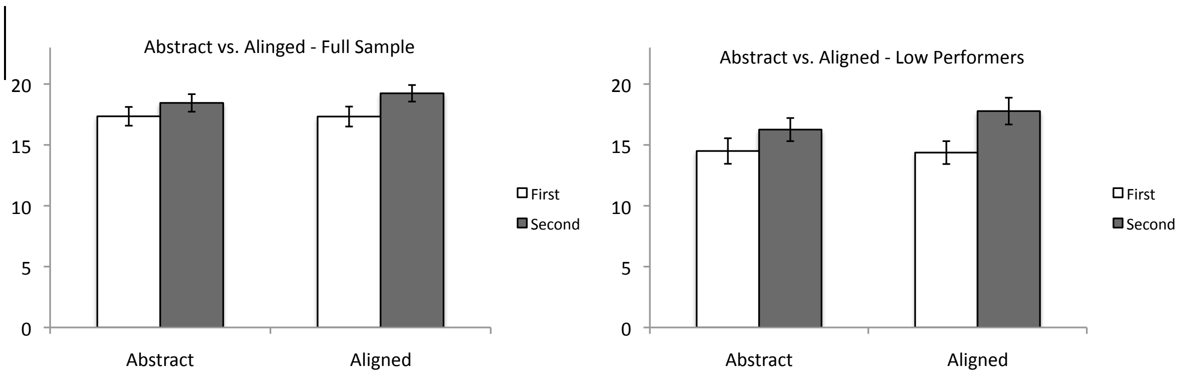


Figure 5. Transfer Results for Abstract vs. Aligned Symbols

Abstract vs. Misaligned. There was no main effect for order., $F(1,26) = .02$, $p = .89$, $\eta^2 < .01$. There was a nonsignificant trend for somewhat higher overall accuracy with abstract symbols than with misaligned symbols, $F(1,26) = 3.46$, $p = .07$, , $\eta^2 = .12$. Continuing the pattern of stronger effects for low performers, this trend toward an effect for symbol type reached significance with the lower performing groups, $F(1,14) = 5.87$, $p = .03$, $\eta^2 = .30$. This finding was in accord with the general hypothesis that should be a decrement for learning with misaligned symbols relative to abstract and aligned ones. There was also a significant symbol X order interaction, $F(1,26) = 6.87$, $p = .01$, $\eta^2 = .21$. Follow up tests, however, showed no large differences in performance for misaligned symbols as a function of learning order when compared to abstract symbols. Instead, accuracy was equivalent regardless of order both for abstract, $t(26) = 1.29$, $p = .30$, and for misaligned symbols $t(26) = .78$, $p = .44$ (see Figure 6). There was no differential transfer; instead both symbols equally supported transfer each to the other. It appears that the interaction emerged because performance on each symbol type was somewhat higher when it was learned second than when it was learned first.

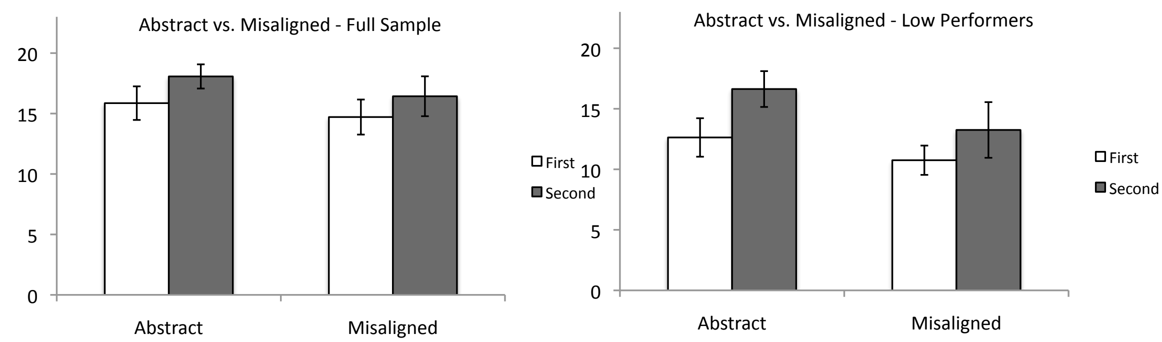


Figure 6. Transfer Results for Abstract vs. Misaligned Symbols

The results for the misaligned versus abstract pairing were different from those for the aligned versus abstract pairing. In particular, there was a transfer advantage for abstract symbols relative to aligned symbols, but there was no such advantage relative to misaligned symbols. The difference is striking because the aligned sets (0, 1, 2) and misaligned sets (2, 1, 0) were *perceptually identical.* If the effects of concrete versus abstract symbols could be accounted for by perceptual differences, then there should have been no differences between the aligned and misaligned symbols vis-à-vis abstract ones. Therefore, differences between pairings could only be due a) to prior knowledge that subjects imported into the learning situation and b) to the way that prior knowledge is activated.

Follow-up Questions

Of the series of paper and pencil follow up questions that each participant received, two items were added to the analysis for exploratory purposes. These were *remind-of-arithmetic,* the degree to which the task reminded participants of arithmetic as indicated on a likert scale from 1 (strongly disagree) to 4 (strongly agree); and *interchangeable,* whether or not participants explicitly recognized that the non-identity elements were interchangeable (1 = yes).

This second variable, *interchangeable*, provides some insight into the degree to which participants began to understand the deep structure of the task. Owing to the nature of modular arithmetic, the non-identity elements both a) generate each other (e.g. ●,● → [illegible] and [illegible], [illegible]→●) and b) act as inverses, yielding the identity when combined (e.g. ●, [illegible] →⚑). This means that the non-identity element in one set can be mapped interchangeably to the non-identity elements in another set. Hence, for the aligned set (0,1,2), the 1 can be mapped either to the ● or the [illegible]. By contrast, the 0 can only be properly mapped to the ⚑. Explicit recognition of this fact indicates a deeper understanding of the task structure than the belief that identity elements map in a strict one-to-one fashion.

As above, repeated measures ANCOVAs were conducted on assessment scores, using symbol type as a within subjects factor, presentation order as a between subjects factor, and *remind-of-arithmetic* and *interchangeable* as covariates. Preliminary analyses showed that *interchangeable* was the only of the two covariates to have significant effects for the Abstract vs. Aligned pairs. Both *interchangeable* and *remind-of-arithmetic* had significant effects for the Abstract vs. Misaligned pairs. The nonsignificant covariates were dropped from the analyses, and the ANCOVAs were rerun.

Abstract v Aligned Sets. Interchangeable was not contingent upon learning order, $\chi^2(1,27) = 1.45$, $p = .23$. It significantly predicted accuracy for the Abstract vs. Aligned groups, $F(1, 24) = 30.90$, $p < .01$, $\eta^2 = .56$, with participants who recognized that the mappings of the non-identity elements were interchangeable performing significantly better than those who did not (see Table 5). This effect was larger and in the same direction for low performers.

Table 5

Average Accuracy Across Trials by Recognition of Interchangeability

	Condition			
	Abstract-Aligned	Aligned-Abstract	Abstract-Misaligned	Misaligned – Abstract
Interchangeable	19.73 (2.38)	18.44 (3.02)	19.93 (1.59)	18.93 (2.47)
Not Interchangeable	9.25 (2.47)	11.60 (4.99)	12.36 (5.39)	13.86 (3.76)

Abstract vs. Misaligned Sets. There was also a main effect for *interchangeable* for the Abstract vs. Misaligned groups, $F(1, 24) = 14.21$, $p < .01$, $\eta^2 = .37$. Participants who recognized that the mappings of non-identity elements were interchangeable were more accurate than those who did not (see Table 5). There was also a main effect for *remind-*

of-arithmetic, $F(1, 24) = 8.53$, $p = .01$, $\eta^2 = .26$. Participants who were reminded of arithmetic were more accurate than those who did not. This difference was qualified by a *symbol* X *remind-of-arithmetic* interaction $F(1, 24) = 5.19$, $p = .03$, $\eta^2 = .18$. Further analysis indicated that participants who were more reminded of addition scored higher on misaligned symbols than those who were not, B = 2.68, t(24) = 3.67, $p < .01$, but equivalently on abstract symbols, B = 1.03, t(24)=1.41, $p =.17$. Again, effects were stronger and in the same direction for low performing participants.

Discussion

Contrary to my hypotheses, there were no differences in initial learning across symbol types. In fact, what difference there was between aligned and abstract symbol sets was even in the wrong direction. This may have resulted because the concrete symbols failed to adequately activate preexisting *addition* schemas. Experiment 2 explores this possibility. Although there were no difference in initial accuracy for different symbol types, there was a trend suggesting an overall learning decrement for misaligned concrete symbols when compared to learning for abstract symbols when performances with each were collapsed across the two different orders of learning. Although this result does not confirm the hypothesized decrement expected for the misaligned set on initial learning, it does suggest that the hypothesis may warrant further investigation.

Regarding transfer, abstract symbols seemed to support transfer to aligned symbols better than vice versa. As predicted, participants performed better with the aligned symbols after learning the abstract symbols first, but performed equivalently on the abstract symbols, regardless of presentation order. For the misaligned vs. Abstract groups, however, neither symbol type supported transfer better or worse to the other. Instead, learning with one symbol type was equally likely to boost later performance on the other.

Follow-up questions indicated that explicit recognition of the interchangeability of non-identity elements was a large predictor of performance. This was to be expected as such recognition indicated better appreciation for the deep structure of the task. Moreover, remindings of arithmetic boosted performance with misaligned symbols. This was somewhat surprising as the hypothesized decrement should be *due* to such prior knowledge. It bears mentioning that ‘arithmetic’ is not at all synonymous with ‘addition’, and it is possible that this distinction may in some way account for the finding.

The most important finding of this experiment bears repeating: There was a transfer advantage for abstract symbols relative to aligned symbols, but not relative to misaligned symbols, even though the aligned symbols (0, 1, 2) and misaligned symbols (2, 1, 0) were *perceptually identical.* The only difference between the sets lay in the way that rules governing the sets were relatively aligned or competing with established arithmetic schemata. These transfer performance differences between the aligned and misaligned symbol sets suggest that symbolic concreteness is indeed affected by prior experience with a given symbol.

CHAPTER III

EXPERIMENT 2

Experiment 1 provided evidence that alignment of numerical symbols with the structure the learning task could affect transfer for the modular arithmetic task. I hypothesized that this was because the presence of numerals activated participants' preexisting integer arithmetic schemas, and that this activated prior knowledge acted as a form of concreteness – that is, information communicated by the symbols – that affected performance. In mirroring the results obtained by manipulating perceptual richness in Sloutsky et. al., (2005) and Kaminski et. al. (2006a), Experiment 1 provided evidence that under certain circumstances, the conceptual information associated with a symbol can operate similarly to perceptual concreteness in supporting initial learning while impeding transfer when compared to abstract symbol sets. Although the results of Experiment 1 directly support the importance of alignment, they only provide circumstantial evidence of the proposed activation mechanism as a driver of the effects. The purpose of experiment 2 was to provide further evidence that the activation of prior knowledge is indeed a factor in determining how effectively concrete a symbol is in any given context. This experiment manipulated an initial warm-up to activate or deactivate participants' prior knowledge of integer arithmetic to modulate the effects of concreteness observed in Experiment 1. Beyond providing evidence for the role of alignment in conceptual concreteness, activation by brief practice could provide evidence that the effects of concreteness on learning and transfer are manipulable in the immediate short term.

Hypotheses

It should be noted that neither the addition warm-up nor the font comparison warm-up was conceived to duplicate Experiment 1. In the case of the addition warm-up, activation of preexisting addition schemas was expected to be higher, increasing the effects of conceptual concreteness. On the other hand, the font comparison warm-up was expected to deactivate mathematical knowledge generally, thereby decreasing the effects of conceptual concreteness. This new warm-up manipulation yielded expectations that diverged a bit from those of Experiment 1.

Learning. It was hypothesized that activating conventional addition schemas with an addition warm-up would improve initial learning with aligned symbols and impede initial learning with misaligned symbols while leaving initial learning with abstract symbols unaffected. Consequently, it was expected that initial learning with aligned symbols would be superior to that with abstract symbols, which in turn was expected to be superior to that with misaligned symbols (*aligned* > *abstract* > *misaligned*). This hypothesis was tempered by cognizance of the high number of participants scoring at ceiling in Experiment 1. Because of anticipated ceiling effects, it was expected that the instrument would not be able to detect differences between the initial learning with aligned and abstract symbol sets, but should detect differences between those two sets and the misaligned set (*aligned* = *abstract* > *misaligned*). In contrast, the font comparison warm-up was expected to deactivate prior math knowledge, encouraging participants to view the numerical stimuli simply in terms of shape. Hence accuracy on initial learning was expected to be equivalent across groups (i.e. *aligned* = *abstract* = *misaligned*).

Transfer. It was further predicted that transfer effects from the abstract to aligned groups would be stronger following an addition warm-up. It was expected for transfer to be greater from the abstract condition to the aligned condition than vice versa, despite the fact that initial scores on aligned symbols was expected to be greater. In particular, I expected the activation of prior schema to further impede acquisition of the deep structure of principles governing the task when learning with aligned symbols, causing a transfer deficit relative to learning with abstract symbols. I expected that there would continue to be no differential transfer for the abstract vs. misaligned groups. Abstract symbols typically yield similar accuracy rates independent of learning order, and misaligned symbols are not expected to benefit much because of the competition instigated by the addition warm-up.

Following the font comparison warm-up, I expected for the transfer differences between abstract and aligned to be attenuated compared to following an addition warm-up. I expected that the misaligned vs. abstract comparison would continue to fail to show preferential transfer in any given direction. This was because the deactivation of numerical knowledge induced by the font comparison warm-up was expected to render the Arabic numeral sets relatively more abstract, yielding transfer that was roughly equivalent across symbol types.

Method

Participants

Participants were undergraduate students from Vanderbilt University recruited for course credit (n =134). Self-reported mean math SAT score was 675 (range 560 to 800, SD = 58.7) and mean math ACT was 30 (range 20 to 36, SD = 3.9).

Design and procedure

The procedure was identical to that of Experiment 1 with one exception. A warm-up phase was added prior to the initial training phase of the experiment. Students were randomly assigned to either an *addition* warm-up or to a *font comparison* warm-up. After completing the warm-up, participants in both the addition and font groups proceeded through a procedure identical to that of Experiment 1.

Participants in the *addition* group solved 8 sets of 15 two-addend addition problems (e.g. 2 + 0 = _ see Figure 7) prior to engaging in the modular arithmetic tasks. Problems were presented one at a time on a computer monitor, and participants entered answers via keystroke. Participants were allowed three seconds to solve each problem before the screen progressed to the next problem, but were encouraged to solve the problems as quickly and accurately as possible. All problems involved pairs of single digit addends from 0 to 9. Sixty percent of all trials involved 0, 1, or 2 as addends in order to ensure ample activation of these particular experimental stimuli as associated with addition. The *font comparison* warm-up employed the same pairs of digits as the addition problems, but instead of being asked to add, participants were asked to press a key indicating whether or not numerals were of the same font (e.g. decide if the following digits are displayed in the same font **2** 0). In sum, Experiment 2 crossed the *addition* vs.

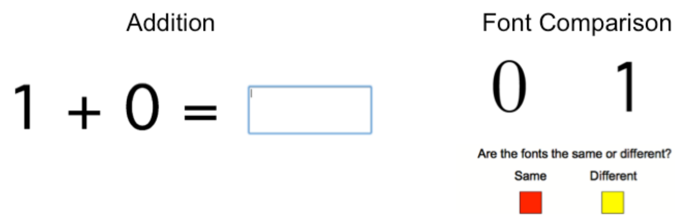


Figure 7. Addition and Font Comparison Warm-up Stimuli

font comparison warm-up dimension the four presentation orders from Experiment 1, *abstract-then-aligned* (n = 34); *aligned-then-abstract* (n = 33); *abstract-then-misaligned* (n = 33); and *misaligned-then-abstract* (n = 34).

All assessments, scoring methods, and coding schemes were identical to those of Experiment 1. Independent raters coded 20% of responses to the follow-up questions included in analysis with agreement ranging from 92% to 100%.

Results

First, I describe a manipulation check conducted in order to verify a) whether warm-up affected activation of prior arithmetic schemas and b) whether subjective reports of such activation were predictive of accuracy. Next, differences in initial learning with the different symbol sets are described, with attention to difference induced by the warm-up tasks. This summary is followed by an analysis of transfer effects. Finally, I examine potential differences in participant recognition of deep structure as suggested by answers to the exploratory follow-up questions.

Subjective reports of schema Activation

Although there was no direct measure for whether or not the warm-up activated the integer addition schema, one of the paper and pencil follow-up questions asked, "Did the task remind you of anything you've learned in the past?" Responses were dummy coded into the binary variable *reminded-addition* to indicate whether or not respondents answered that the task reminded them of addition. This open-ended question was asked upon completion of training and testing on both symbol sets. Although it is an imperfect indicator of schema activation, such a reflective self-report of subjective experience can provide tentative evidence for whether or not the warm-up manipulation worked as planned.

A Chi-square test was conducted to confirm whether or not warm-up influenced likelihood of a participant reporting being reminded of addition. As expected, the data suggest that the addition warm-up may have increased the likelihood that the task reminded the participants of addition when compared to the font comparison task, $\chi^2(1,134) = 11.94$, $p < .01$ (See Table 6).

Table 6

Participants Reporting Being Reminded of Addition by Warm-up

Warm-up	Yes	No
Font Comparison	7	58
Addition	25	44

As a secondary check, a one-way ANOVA was performed on the entire sample with initial learning score as the dependent variable and symbol type and *remind-addition* as the independent variables. There was a main effect for *remind-addition*, F(1,132) = 6.77, p = .01 η^2= .05, so that participants who reported being reminded of addition scored higher (M = 18.44, SD = 4.30) than those who did not (M = 15.77, SD = 5.26).

These data suggest that the manipulation did lead to differences in activation of participants' addition schemas. Moreover, the overall accuracy differences between those who were reminded of addition and those who were not support the view that activation does indeed affect performance on the task.

Initial Learning

Initial learning was expected to exhibit a pattern in which *aligned* = *abstract* > *misaligned* (assuming ceiling effects in the aligned and abstract conditions) after the addition warm-up and an alternative pattern in which *aligned = abstract = misaligned* after the font comparison warm-up. To examine these effects, a univariate ANOVA was conducted with accuracy on initial learning trials as the dependent variable, type of symbol as a 3-level independent variable (abstract, aligned, or misaligned), and type of warm-up as a 2-level independent variable (font comparison or addition).

Contrary to predictions, there was no main effect for warm-up, $F(1,128) = .84$, $p = .364$, $\eta^2 = .01$. There was also no main effect for symbol, though there was a nonsignificant trend in the predicted direction, $F(2,128) = 2.44$, $p = .09$, $\eta^2 = .04$. Because there was already a specific *a priori* prediction for a symbol effect, a planned contrast was analyzed to examine the relation further. The single degree of freedom contrast (.5*abstract, .5*aligned, -1*misaligned) was designed to test for a decrement due to misaligned symbols relative to the others. In particular, a contrastXwarm-up effect was expected whereby the contrast would not be significant for the font comparison warm-up, but would be significant for the addition warm-up.

Contrary to hypotheses, there was a main effect for the contrast $\Psi_{(.5, .5, -1)}$, $F(1,130) = 4.66$, $p = .03$, $\eta^2 = .04$, indicating that initial learning accuracy was lower for misaligned symbols than for abstract and aligned symbols. There was, however, no contrastXwarm-up interaction, $F(1,130)=1.95$, $p=.17$, $\eta^2 = .02$. This finding was curious given the patterns observed in the data (see Figure 8. Visual inspection suggests that performance across symbols was equivalent for those receiving the warm-up task, but different for those receiving the addition task. For purposes of exploration, separate univariate ANOVAs examining the effect of symbol type on learning were run, and these results also suggested effects for symbol with the addition group, $F(2,66)=3.45$, $p=.04$, $\eta^2 = .10$, but not for the font comparison group, $F(2,62) = .30$, $p=.74$, $\eta^2 = .01$. Thus, although there was no warm-upXsymbol interaction that emerged from the omnibus

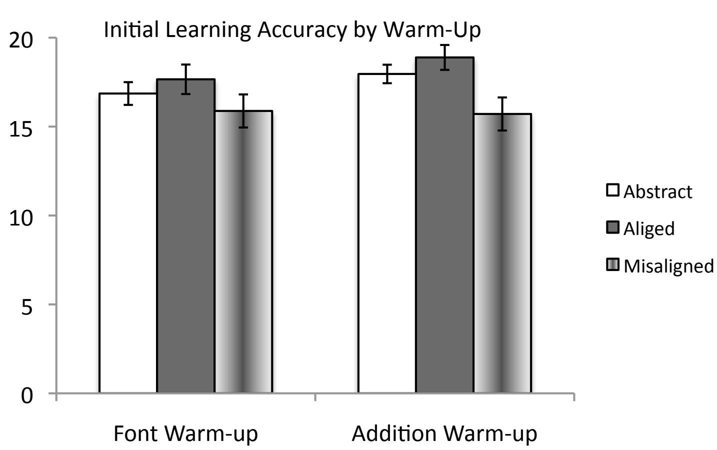


Figure 8. Initial learning performance by symbol type and warm-up

ANOVA, there do seem to be small differences in initial accuracy that stemmed from the warm-up manipulations. These relations warrant further study.

Transfer

As in Experiment 1, there was an apparent problem with ceiling effects for the assessment, especially with abstract and aligned symbols following the addition warm-up. A conservative estimate of a 95% confidence interval around a ceiling score of 23 was created using a binomial approximation to the mean: $CI = p \pm 1.96\sqrt{\frac{p_0(1-p_0)}{n}}$ where p_0 represents the probability of chance success, and n represents the number of trials in the assessment. For the confidence interval estimate, p_0 was set to .25 (This was a conservative estimate because three of the items on the assessment actually had $p_0 = .5$). This yielded a CI of 19-23 for a perfect score. In each of the four possible conditions, at least 35.3% of participants scored within the confidence interval for perfection (see table). As a result, the posttest had a compromised ability to show increases from the learning to transfer conditions. To deal with the compressed variability on the top end of the spectrum, a secondary data analysis was conducted for lower performing participants based on median splits for each of the four conditions as in Experiment 1. The transfer analyses reported below will generally be those conducted on the full sample with supplemental commentary on low performing sample where appropriate.

As in Experiment 1, the evaluation of transfer was broken down into analyses of pairs of learning order by concrete symbol learned. This ultimately resulted in analysis of two separate pairs of conditions: a) abstract-then-aligned vs. aligned-then-abstract; and b) abstract-then-misaligned vs. misaligned-then-abstract. Two sets of repeated measures ANOVAs were conducted, using symbol type as a within subjects factor and order (abstract first or concrete first) and warm-up type (addition or font comparison) as between subjects factors. Initial analyses indicated no significant effects for warm-up, so the variable was dropped from the model and the ANOVAs were rerun.

Abstract vs. Aligned. There was no main effect for order. Collapsing across symbol type, accuracy for the Aligned-then-Abstract group was equivalent to that for the Abstract-then-Aligned group, $F(1,65) = .16$, $p = .69$, $\eta^2 < .01$. There was no main effect for symbol, indicating that overall accuracy with aligned symbols was equivalent to overall accuracy with abstract symbols when scores were collapsed across the between subjects variable of order $F(1,65) = 1.49$, $p = .23$, $\eta^2 = .02$ (see Table 8).

There was a significant symbol X order interaction, $F(1, 63) = 23.21$, $p < .01$, $\eta^2 = .26$. As predicted, subsequent analysis showed somewhat larger differences in performance for aligned symbols as a function of learning order than for abstract symbols. There was a trend for participants who learned in the abstract-then-aligned conditions to perform significantly better on the aligned symbols than those who learned the aligned symbols initially, $t(65) = 1.79$, $p = .08$. In contrast, there was no difference in accuracy for abstract symbols across conditions, $t(65) = 1.04$, $p = .30$. This pattern emerged more strongly amongst lower performers with no difference for abstract scores, $t(35) = 1.25$, $p = .22$, but an increase in accuracy for aligned symbols when initial learning was abstract symbols, $t(35) = 2.36$, $p = .02$. Hence, learning with abstract symbols seemed to have somewhat boosted later learning with aligned symbols compared

Table 7.

Percent of Participants scoring ≥19 on Initial Learning Set by Condition

Warm-up	Order 1st 2nd	Abstract→ Aligned	Aligned→ Abstract	Abstract→ Misaligned	Misaligned→ Abstract
Font Comparison	% Scoring ≥19	37.5	56.3	37.5	35.3
	Median cut score	< 19	< 20	< 19	< 17
	Proportion of sample remaining after split	10 of 16 62.5%	10 of 16 62.5%	10 of 16 62.5%	10 of 17 58.8%
Addition	% Scoring ≥19	83.3	64.7	47.1	35.3
	Median cut score	< 20	< 20	< 19	< 16
	Proportion of sample remaining after split	8 of 18 44.4%	9 of 17 52.9%	9 of 17 52.9%	9 of 17 52.9%

Table 8.

Accuracy by Order for Different Learning Groups

Type of Concrete Symbol	Symbol Type	Order: Abstract 1st	Order: Concrete 1st
Aligned	Abstract	17.35	18.45
	Aligned	19.24	17.33
Misaligned	Abstract	16.21	17.67
	Misaligned	16.85	14.76

to learning with aligned symbols first. In contrast, learning with aligned symbols first did not aid later learning with abstract ones to the same extent (see Figure 9). As expected, these effects were even stronger and in the same direction for lower performing participants. As in Experiment 1, the transfer results with the abstract vs. aligned pairing parallel the transfer results from the abstract-perceptually rich comparison of Sloutsky et. al. (2005).

Abstract vs. Misaligned. There was no main effect for order., $F(1,65) = .07$, $p = .80$, $\eta^2 < .01$. There was a main effect for symbol such that there was higher overall performance with abstract symbols than with misaligned symbols, $F(1,65) = 7.28$, $p = .01$, $\eta^2 = .10$. Once again, this effect was stronger among low performers (see Figure 10).

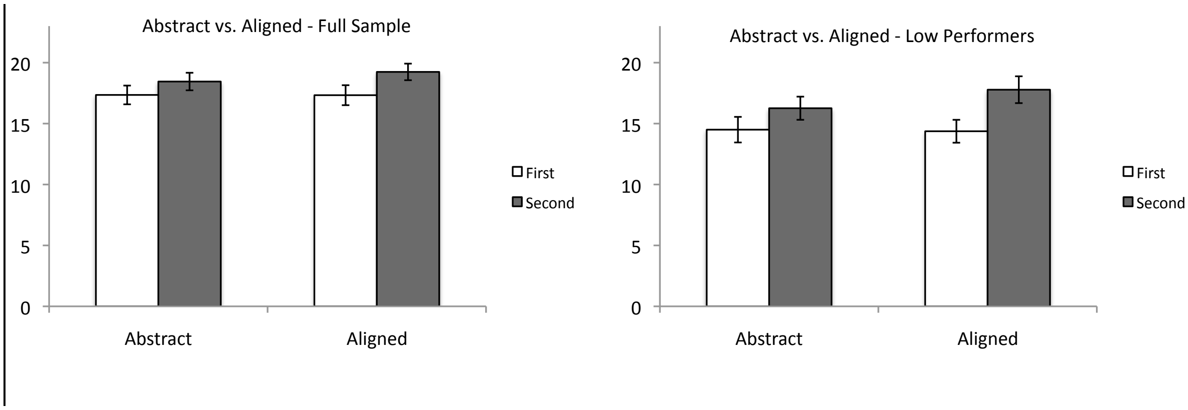


Figure 9. Transfer Results for Abstract vs. Aligned Symbols

There was also a significant symbol X order interaction, $F(1,65) = 7{,}28$, $p = .01$, $\eta^2 = .10$. Follow up tests, however showed that performance was equivalent regardless of order both with abstract symbols, $t(65) = 1.17$, $p = .25$, and with misaligned symbols, $t(65) = 1.61$, $p = .11$. As predicted, there was no differential transfer; instead both abstract and misaligned symbols equally supported transfer each to the other. It appears that the interaction emerged because performance on each symbol type was somewhat higher when it was learned second than when it was learned first.

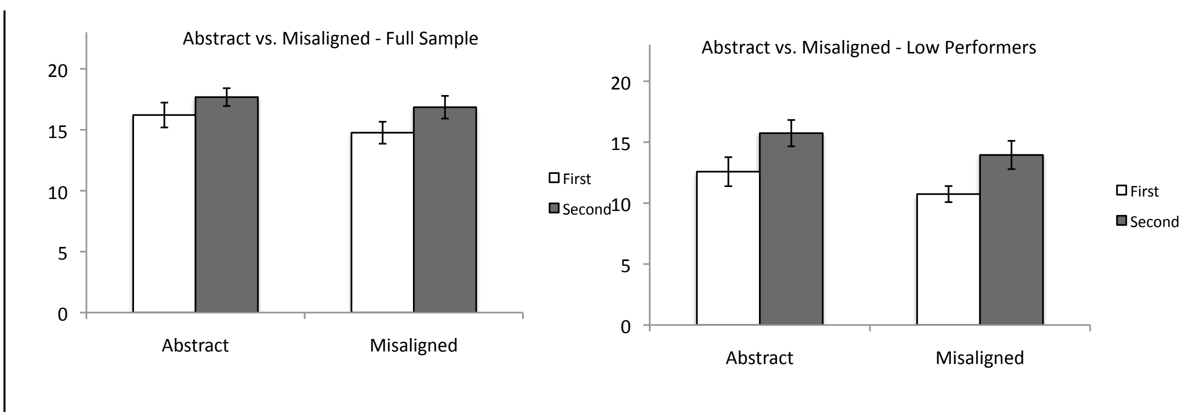


Figure 10. Transfer Results for Abstract vs. Misaligned Symbols

Follow-up Questions

The follow-up items *remind-of-arithmetic* and i*nterchangeable* were added to the analysis for exploratory purposes. As above, repeated measures ANCOVAs were conducted on assessment scores, using symbol type as a within subjects factor, presentation order as a between subjects factor, and *remind* and *interchangeable* as covariates. *Interchangeable* was the only covariate for which effects emerged, so *remind-of-arithmetic* were dropped from the model and the ANCOVAs were re-run.

Abstract vs. Aligned sets. For this group, the likelihood of recognizing nonidentity interchangeability was contingent upon learning order $\chi^2(1,67) = 9.34$, $p < .01$, (see Table). Those who learned on abstract symbols first were more likely to recognize interchangeability than those who first learned on aligned symbols. This means that participants who learned with aligned symbols first were less likely to explicitly note this aspect of the deep structure of the task than those who first learned with abstract symbols. This finding is in accord with the prediction that learning with aligned symbols would impede appreciation for the deep structure of the task. It may also help explain the relative transfer advantage for abstract symbols relative to aligned ones.

The contingency of *interchangeable* upon learning order presents the possibility that *interchangeable* may be causally related to learning order for the abstract vs. aligned sets. If this is the case, then collinearity between *interchangeable* and order complicates interpretation of the statistical tests. This covariate should be explicitly analyzed with a design aimed at unpacking its causal relations.

Abstract vs. Misaligned sets. The likelihood of recognizing nonidentity interchangeability was not contingent upon learning order for this set of symbols,

$\chi^2(1,67) = .13$, $p = .72$ (see Table 9). Participants were equally as likely to acknowledge this aspect of the deep structure of the task when they learned with misaligned symbols first as they were when they learned with abstract symbols first. This is consistent with the possibility that misalignment of the task with prior knowledge encouraged participants to abandon prior knowledge when trying to understand the task.

Table 9

Number of Participants Recognizing Interchangeability by Order of Learning

		Recognized Non-Identity Interchangeability	
		No	Yes
Abstract vs. Aligned	Abstract 1st	11	23
	Aligned 1st	23	10
Abstract vs. Misaligned	Abstract 1st	17	16
	Misaligned 1st	19	15

Inserting *interchangeable* into the transfer analysis for abstract vs. misaligned sets does not change the significance or direction of any of the effects of the original analysis. Thus, it is much more straightforward to interpret the analysis. Those who recognized the interchangeability of nonidentity elements scored higher than those who did not, $F(1,64) = 30.48$, $p < .01$, , $\eta^2 = .32$ (see Table 10).

Table 10.

Average Accuracy Across Trials by Recognition of Interchangeability

	Condition			
	Abstract-Aligned	Aligned-Abstract	Abstract-Misaligned	Misaligned – Abstract
Interchangeable	20.02 (1.75)	19.25 (3.82)	20.47 (2.16)	18.13 (3.70)
Not Interchangeable	14.68 (5.06)	17.30 (4.38)	12.82 (4.97)	14.71 (4.29)

Discussion

Initial learning was lower with misaligned symbols. Moreover this difference appeared to be greater for the addition warm-up condition than for the font comparison warm-up. This suggests that the activation of relevant prior knowledge modulated the effects of conceptual concreteness in this case.

The transfer finding were very similar to those in Experiment 1. Abstract symbols supported transfer to aligned symbols better than vice versa. As predicted, participants performed somewhat better with the aligned symbols after learning the abstract symbols first, but performed equivalently on the abstract symbols, regardless of presentation order. For the misaligned vs. Abstract groups, however, neither symbol type supported transfer better or worse to the other. Instead, learning with one symbol type was equally likely to boost later performance on the other.

Follow-up questions indicated that explicit recognition of the interchangeability of non-identity elements was a large predictor of performance. Moreover, recognition of this interchangeability was contingent upon learning order for the aligned vs. abstract groups. It seems that those who got aligned concrete symbols first were less likely to recognized this interchangeability than those in other conditions. Still performance was at least as high for these participants as it was for others. This finding was consistent with the findings of Kaminski et. al., (2006b), which found that those who learned with perceptually concrete symbols were less able to recognize the structure of the task in new forms after learning with aligned perceptually concrete symbols. This raises the possibility that the processes supporting accuracy with aligned symbols may be different from those supporting accuracy with abstract symbols.

CHAPTER IV

EXPERIMENT 3

The purpose of this experiment was to investigate the effects of irrelevant conceptual concreteness on learning and transfer. Experiments 1 & 2 provided evidence that the conceptual information associated with symbols could affect learning and transfer for the modular arithmetic task. Notably, the concrete symbols used for the task were Arabic numerals which contained conceptual information that was in some way relevant to the learning tasks – be it aligned or misaligned. This leaves open the question of whether irrelevant conceptual concreteness – that is conceptual information that is completely unrelated to the learning task – might have effects on learning and transfer. This experiment examined these relations, once again controlling for the perceptual input of the symbols while manipulating the conceptual information communicated by the symbols.

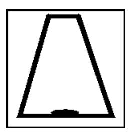
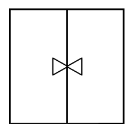
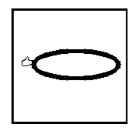
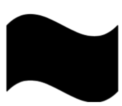
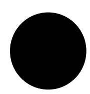
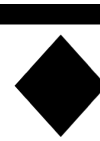
In this case the domain was instantiated using the two alternative symbol sets for learning – a *meaningful* concrete set and a perceptually identical *empty* concrete (see Table 11) – and using a set identical to the abstract sets used in Experiments 1 & 2 to measure *transfer*. The meaningful set was so named because it was expected that the label and interpretation of the symbols would imbue them with more conceptual information relative to the *empty symbols.*

Hypotheses

Learning. It was expected that the meaning associated with the solutions for the meaningful group would render the symbols effectively more concrete than in the empty condition. Likewise, stimuli in the empty condition, because they were not given labels, were expected to communicate less information and were therefore more abstract.

Table 11.

Abstract, Meaningful, and Empty Stimuli for Experiment 3

Symbol	Identity Element	Non-Identity Elements	
Learning Symbols			
Label for Meaningful Group	Hallway mirror as seen by a crawling baby	A man who's caught his bowtie in an elevator	An alien in a flying saucer signaling a left turn
Transfer Symbols			

Because the concreteness was not relevant to the learning domain, there should be no support to boost learning accuracy with meaningful symbols relative to the empty ones. In fact, the concreteness of the meaningful symbols might even impose an extra processing load that slow initial learning. Thus, it was expected that initial accuracy for the empty group would be greater than or equal to that of the meaningful group. This would parallel the finding in Experiment 3 of Sloutsky et. al. (2005) that increasing irrelevant perceptual concreteness can impose costs on learning.

Transfer. The empty symbols were expected to support transfer better than meaningful symbols because they are relatively more abstract. The names associated with the meaningful group were expected to make the knowledge supported by such symbols more context bound and thusly less transportable. On the other hand, the empty symbols – because they don't communicate extraneous information – were expected to allow participants to pay more attention to the deep structure of the task at hand.

Method

Participants

Consent was obtained from 50 undergraduate students from Vanderbilt University recruited for course credit. Of the 50, two were dropped from the analysis because they had participated in earlier versions of the experiment that employed tasks identical to the current transfer task. Mean math SAT score obtained from the registrar was 666 (range 560 to 800, SD = 54.8) and mean math ACT was 29 (range 18 to 35, SD = 3.6).

Design

The experiment was conducted in 6 phases presented over approximately one hour: 1) a brief introduction to the stimuli used in the initial learning task (functioned as the experimental manipulation), 2) training with one symbol set (the learning set), 3) testing with that symbol set, 4) training with a second symbol set (the transfer set), 5) testing with that symbol set, and 6) a series of follow up questions about the correspondences between the first and second symbol sets and strategies used to produce answers. Participants completed the experiment individually, with all training and testing stimuli presented on a computer in a self-paced manner.

Procedure

The introductory phase was intended to introduce the participants to the stimuli in a way that would seem independent of the training task to follow. Participants were randomly assigned to either receive *empty* (n = 25) or *meaningful* (n = 23) versions of the initial training stimuli. After assignment to group, participants were given four minutes to study eight novel stimuli adapted from Price (2000). The stimuli, known as 'droodles' (a combination of doodle and riddle), were novel ambiguous drawings that have associated names or solutions that make the drawings sensible (see example). Both groups studied stimuli that were identical with one exception: those in the *meaningful* group were asked to study pictures paired with their associated names, and those in the *empty* group were asked to study pictures without names. Participants in each group were instructed that they would later be asked to recall the stimuli studied from a larger series of stimuli.

Immediately after the introductory phase, participants completed a 13 trial recall task. The task was presented in Superlab on a Macintosh Powerbook G4, and answers were recorded using a combination of keystroke and paper and pencil responses. For each recall trial, groups were presented with a series of three stimuli with an additional 'None of the Above' option to choose from. Either zero or one of the stimuli from the eight presented in the memorization phase was presented among the answer choices. The remaining stimuli were taken from a list of widely recognized corporate logos (see Appendix E). Those in the *meaningful* group were first asked to choose which of the three stimuli (or none of the above) was presented as part of the memorization task and then asked to write the matching name of the image on the paper and pencil answer sheet. Those in the *empty* group were also asked to choose the previously presented image, but instead of being asked to name the (unnamed) droodle, were asked to name one of the corporate logo distractors to match the verbal load of the task performed by the *meaningful* group.

The training phases introduced the rules governing each symbol system. As in Experiments 1 & 2, the relations between the elements of each symbol set were governed by the rules of addition modulo three. Training for each of the symbol sets was nearly identical. The rules were introduced by fictional characters with separate cover stories for the abstract and the concrete conditions. The learning (i.e. droodle) sets were presented as a type of card game played by children in a foreign country. In this system, different children pointed to combinations of cards, and the child who was ‘it’ needed to figure out the winning card based upon the cards that others pointed to (adapted from Kaminski, 2006c). The transfer set was identical to the abstract set from Experiments 1 & 2..In this experiment, as counterposed to Experiments 1 & 2, the abstract symbol set always functioned as the transfer task.

Each training phase was immediately followed by a test phase. The test phase consisted of 23 multiple-choice questions that required participants to apply the previously learned rules. For all symbol sets, the test questions were completely isomorphic to the test phase of Experiments 2 and were presented in the same order.

A series of follow up questions identical to those asked in Experiments 1 & 2 was asked at the conclusion of the second test phase (see Appendix F for a detailed list). Independent raters coded 20% of responses to the follow-up questions included in analysis with 92% agreement.

Results

Initial Learning

An independent samples t-test was conducted to examine differences in initial learning. Score on the droodle set served as the test variable and condition (*meaningful* or *empty*) served as the grouping variable. Contrary to expectations, there was no difference in initial learning between the meaningful (M=9.35, SD = 2.08) and empty (M=10.36, SD=3.50) groups $t(46) = 1.21$, $p = .23$. The manipulation failed to make a difference for initial learning.

Indeed, it appears that there was very little initial learning to support transfer in the first place. The expected value of chance performance on the task is 7 (Five problems with 2 answer choices, and 18 problems with 4 answer choices). A conservatively constructed 95% confidence interval around the expected chance value (using a binomial distribution with 25% random probability of correct answer) yields the confidence interval $4.18 < x < 9.81$. This means that any score at below 10 is equivalent to chance performance. Overall, 52% of participants in the empty condition and 69.6% of participants in the meaningful condition scored at or below chance on the learning trials. Moreover, the mean initial learning scores in each condition were at chance levels (see

Table 12). There was no difference in the likelihood of scoring below chance in either condition, $\chi^2(1,48) = 1.55$, $p=.21$). Thus, it is arguable that the initial learning task was ineffective for assessing differential transfer.

Table 12.

Percent scoring at or below chance on initial learning set by condition

	Meaningful	Empty
Mean Initial Learning Accuracy (SD)	9.35 (2.08)	10.36 (3.50)
% Scoring ≤ 10	69.6	52

Transfer

To assess transfer we conducted a repeated-measures ANOVA, using trial (learning vs. transfer) as a within subjects factor and initial learning symbol (empty vs. meaningful) as a between subjects factor. There was a main effect for trial, $F(1,46) = 62.92$, $p < .01$, $\eta^2 = .59$, indicating that accuracy was higher for the transfer symbol set than for initial learning symbol sets. Contrary to expectations, there was no main effect for the manipulation of initial learning symbol, $F(1,46) = 1.46$, $p = .23$, $\eta^2 = .03$. There was no difference between accuracy in overall performance collapsed across trials for those in the empty vs. meaningful conditions. Finally, there was no trialXcondition interaction, $F(1,46) = .04$, $p = .85$, $\eta^2 < .01$, to suggest that accuracy on the transfer set varied in response to the experimental manipulation between groups that initially learned using the meaningful (M=15.17, SD = 5.13) and empty symbols (M=16.48, SD=5.30).

Follow-up Questions

Of the series of paper and pencil follow up questions that each participant received, two items found to be predictive in previous experiments were added to the analysis for exploratory purposes. These were *remind-of-arithmetic,* the degree to which the task reminded participants of arithmetic mathematics as indicated on a likert scale from 1 (strongly disagree) to 4 (strongly agree); and *interchangeable,* whether or not participants explicitly recognized that the non-identity elements were interchangeable. There were no significant effects for *remind-of-arithmetic,* so it was dropped from the model and analyses were rerun.

A repeated-measures ANCOVA was conducted using trial as a within subjects factor, condition as a between subjects factor, and *interchangeable* as a covariate. *Interchangeable* did predict overall performance collapsed across trials and conditions, $F(1,44) = 16.00$, $p < .01$, $\eta^2 = .27$, with those who explicitly recognized nonidentity interchangeability (M = 15.57, SD = 2.17) scoring higher than those who did not (M = 11.75, SD = 3.11). There was, however, no trialXconditionXinterchangeable interaction to suggest that recognition of nonidentity interchangeability modulated transfer supported by either condition, $F(1,44) = 1.47$, $p = .23$.

Discussion

There were no differences in initial learning or transfer between conditions. In fact, it seems that there was very little in the way of overall initial learning to begin with, as a large proportion of the sample scored at chance levels. Without evidence of initial learning, assessments of transfer are difficult.

Two likely explanations present themselves for explaining the low overall rates of accuracy on the initial learning trials. First, there is the perceptual concreteness associated with the droodles used for learning. Both sets pack more irrelevant perceptual concreteness than any of the symbols used in Experiments 1 & 2. Second, the way the task is introduced with these symbols is a bit different from the ways the task is introduced with other symbols. It may be that learning the task in terms of a children's pointing game is simply more difficult than other instantiations used, which both present the task as combining elements to yield a third.

CHAPTER V

GENERAL DISCUSSION

The current research examined some novel implications generated by an information-based account of symbolic concreteness. In particular, it was aimed at investigating whether or not the conceptual information associated with a symbol could function similarly to perceptual concreteness in a novel mathematical domain. This discussion begins with a consideration of three hypotheses that motivated the work: 1) That there should be a conceptual analog to perceptual concreteness; 2) that the alignment of conceptual information with the to-be-learned content should modulate its effects on learning and transfer; and 3) that the effects of conceptual concreteness should be manipulable by interventions that affect the activation of information associated with the symbol. It then briefly raises a key limitation of the current experiments and how that limitation might be addressed. Finally, it considers some general implications of the research when couched against the larger theoretical backdrop of the information-based account of concreteness.

A conceptual analog for perceptual concreteness?

When paired with abstract symbols in a way aligned with the structure of the learning task, Arabic numerals exhibited a pattern for learning and transfer that was quite similar to that exhibited by perceptually rich concrete symbols in other studies that employed the same task (Kaminski et. al., 2006a; Kaminski et. al., 2006b; Sloutsky et.

al., 2005). That is, there was a transfer advantage for learning with abstract symbols first relative to learning with Arabic numerals whose conventional usage was aligned with the task. This effect was found in both Experiments 1 and 2. These similar patterns of performance suggest a conceptual analog for perceptual concreteness.

Furthermore, it appears that initial learning with aligned symbols manifested some of the typical problems associated with learning using concrete symbols. As discussed above, one of the ways that concrete symbols are supposed to help learning is by aiding memory access, but they are thought to impair transfer by taking attention away from the deep structure of the task. This seems to have been the case with aligned symbols. Recall that participants in Experiment 2 were less likely to recognize the interchangeability of nonidentity elements when they initially learned with aligned symbols. This means they were less likely to see the deep structure of the task, despite the fact that they faired just as well on initial accuracy as those who learned abstract symbols first. The disconnect between learning and transfer with aligned symbols might be explained by the boost that prior knowledge gave to learning the rules. Two-thirds of the rules to be learned could simply be imported from prior arithmetic schemas (0+1, 0+2, 0+0, and 1+1), leaving only two rules to be memorized (1+1 and 2+2). If these rules were applied mechanically, learners could solve problems with a high degree of accuracy without noticing the deep structural elements of the task. Initial learning based on such simple memory aids would be expected to be tied to prior knowledge and not to transfer as well. The observed pattern of results seems consistent with this explanation and parallels the results obtained using relevant perceptual concreteness (Kaminski et. al., 2006b).

The alignment of conceptual information with the to-be-learned content should modulate its effects on learning and transfer

The results found with aligned numerals stood in contrast to the results with misaligned numerals. Although there was a transfer advantage for abstract symbols relative to aligned ones, no such transfer advantage existed for abstract symbols relative to misaligned symbols. Instead, transfer was roughly equivalent between abstract and misaligned symbols. Even though there was no transfer advantage for abstract symbols relative to misaligned ones, there a small overall advantage for performance with abstract symbols relative to misaligned symbols in Experiments 1 and 2. No such general performance difference emerged between abstract and aligned symbols. These differences all emerged because of the manipulation of symbolic alignment with the to-be-learned task.

Another important difference lay in the fact that contingency of recognizing interchangeability depended upon the alignment of the concrete symbols involved. Employing Arabic numerals in a way that was misaligned with prior knowledge allowed learners to recognize this element of deep structure just as frequently as learning with abstract symbols first. It may have been that the misalignment in some way rendered the numerals somewhat more abstract. It may be that the misaligned numerals imposed a higher overall memory load due to conflict with preexisting schemas. This additional load due to misalignment may have provided some desirable difficulty for transfer, whereby the incongruence of the task with preexisting addition schemas triggers an appreciation for the deep structure of the task (see also Bjork, 1994; Mannes & Kintsch, 1987). At the same time, it may have reduced overall accuracy.

The differential pattern between learning and transfer for abstract vs. misaligned symbols is also consistent with the Crowley, Shrager, & Siegler (1997) model of competitive negotiation between metacognitive and associative mechanisms in strategy discovery. On this model, the inadequacy of existing solution strategies tapped by associative mechanisms can lead to a more metacognitively guided search for new strategies. Although the prior addition schemas associated with numerals could help with learning in the aligned condition, the schemas were inapplicable in the misaligned case. This lack of a preexisting model for action may have lead to more careful analysis of the domain, producing a knowledge structure that was more in tune with deep structure.

Whatever the mechanisms involved, there were clear differences in performance based upon alignment. These differences are striking because the aligned sets (0, 1, 2) and misaligned sets (2, 1, 0) were *perceptually identical.* If the effects of concrete versus abstract symbols could be accounted for by perceptual differences, then there should have been no differences between the aligned and misaligned symbols vis-à-vis abstract ones.

Moreover, the manipulation of Experiment 3, which did not manipulate relevant concreteness, had no effects on performance. Unlike the situation with Arabic numerals, there was no difference in the effects of the concreteness manipulation with the droodles. This may have been because the conceptual knowledge associated with the droodles was irrelevant (i.e. neither aligned nor directly misaligned) to the learned task. The additional information associated with meaningful symbols may simply not have affected processing of the to-be-learned content.

There are two other possibilities, however, that make it difficult to ascertain the role of conceptual relevance to the null effects of Experiment 3. First, as discussed above, the method of introducing the rules of the system with droodles diverged significantly from the method used with other symbol types. It used a different syntax and a cover story that involved more perspective taking that may have complicated the task. These differences may have significantly impeded learning. Second, the knowledge attached to the droodles was not as tightly tied to the symbols as was the knowledge attached to Arabic numerals. The labels learned for the *meaningful* group were taught over a four-minute span whereas the information attached to Arabic numerals are taught, re-taught, and practiced in institutionalized settings over the course of years. Whatever the reason may be, Experiment 3 sought to induce irrelevant concreteness in a short time frame using linguistic labels and this manipulation failed to have the same effects as the intrinsically relevant concreteness of Experiments 1 and 2.

The effects of conceptual concreteness should be manipulable by interventions that affect the activation of information associated with the symbol.

Experiment 2 found that the effects of conceptual concreteness could be significantly modulated with a brief warm-up activity. When given a four-minute addition warm-up to activate preexisting addition knowledge, a significant decrement emerged for initial learning accuracy on misaligned symbols relative to abstract and aligned symbols. By contrast, there was no such decrement following a font comparison task, which encouraged learners to focus on the perceptual attributes of the symbols instead of on the prior mathematical knowledge associated with the symbols. By modulating activation of the prior conceptual knowledge hypothesized to render Arabic numerals as concrete, this manipulation underscored the fact that it was not the perceptual attributes of the symbols that were responsible for the effects found. Moreover the fact that these effects were manipulable at all implies that concreteness is not a static attribute of a symbol, but can be affected by contextual factors. Finally, viewed with an eye toward pedagogy, this is evidence that even subtle manipulations in context can affect the information that a chosen symbol communicates, with significant effects for the learning that the symbol supports.

Key Limitations

Ceiling effects. Perhaps the single largest limitation of the present investigation was the restriction of range on the outcome measures. In Experiments 1 and 2, more than 40% of the sample was at ceiling on the task following initial learning. The compressed variability due to these ceiling effects presents potentially serious problems for causal inference. In the present case, supplementary analyses were presented on low-performers, and those analyses were generally associated with stronger effects. Unfortunately, the smaller sample sizes for those supplemental analyses compromise confidence in the results of the statistical analysis. Future investigations that use the current assessments should establish inclusion criteria, anticipating the exclusion of those at ceiling. Consequently data should be collected on a much larger sample in order to ensure sufficient sample size after eliminating those who score above the inclusion threshold.

A more elegant solution for dealing with this ceiling effect – albeit one that would require considerably more sophistication than using exclusion criteria – would be to develop a scale that can account for a wider range of variability in participant abilities. The most pressing impediment for such a project is the need to develop a more coherent conception of the construct measured by the present assessment. As currently used, the assessment is good for measuring how manipulating dimensions of a variable affect task performance, but this performance is not directly conceived of in terms of a commonly measured theoretical construct (e.g. math ability, verbal ability, visuospatial ability, etc). Developing a more systematic conception of the construct would both allow better measurement and help lead to a theoretically based understanding of the variability exhibited in the population.

Maintenance over time. The current research explores learning and transfer measured over a very short time span (around 1 hour). This is very different from real world situations in which learning usually occurs over a much longer time course. Although learning with abstract symbols first may better allow participants to acquire deep structure in a one-shot learning situation, the current experiments have no measure for whether or not such learning is maintained over time or quickly forgotten.

If we take the proposition that concrete structure helps aid memory seriously, then learning based on such memories – even if less attentive to deep structure – may be maintained much longer over time. Further, some research has suggested that it may be possible to root initial learning in concrete symbols and to encourage later transfer by a process of concreteness fading (Goldstone & Son, 2005). Concreteness fading begins by representing to-be-learned content with concrete symbols and follows by representing the content with progressively more abstract symbols. It may be that such a paradigm could be used with the current task to provide more robust learning that can be maintained over time (Nicole McNeil, personal communication, February 19, 2010). On any account, the design of the present experiments would be much improved if it were extended to include multiple time points.

Implications for math education: Math without numbers?

Perhaps the most interesting result of these studies from a pedagogical perspective is that the task – one governed by well-defined mathematical rules – was on the whole learned better when initially instantiated with abstract shapes than with numbers. This is an important point, because modular arithmetic is not some random experimental task with no relevance to the real world. Instead, it is a foundational concept in number theory. Although this concept was not taught in depth in this experiment, few mathematical concepts ever are taught in depth at first pass. Even the simplest of mathematical concepts are quite complex, and people require much time and practice to develop familiarity with them (see Baroody & Dowker, 2003; Becker & Varelas, 1993; Greeno, Riley & Gelman, 1984; Rittle-Johnson & Siegler, 2008). The fact remains that in this introduction to modular arithmetic, deep structure was learned better with abstract symbols than with numbers.

One interesting proposition is that this may be a case in which prior knowledge of numbers are getting in the way of learning mathematics. Moreover, there may be more such cases. This should not be an entirely provocative statement. Other lines of research have shown that prior knowledge acquired in a mathematical domain can pose an impediment to developing more sophisticated knowledge in that domain. For instance, a long line of work has presented evidence that prior experience with arithmetic operations, while solidifying children's abilities to perform those operations, can lead to incorrect operational understanding of the equal sign (Knuth, Stephens, McNeil, & Alibali, 2006; McNeil & Alibali, 2005; Perry, 1991; Rittle-Johnson, Siegler & Alibali, 2001). It has also been shown that simple interventions that draw attention to the relations involved with the equal sign, as opposed to those that focus on numbers *per se*, can push the development of sophistication in the domain (Matthews & Rittle-Johnson, 2008; Rittle-Johnson & Alibali, 1999).

It is generally accepted that the acquisition of a robust sense of number is an essential first step on the road to mathematical competence (Landerl, Bevan & Butterworth, 2003; National Math Panel, 2008). The canonical 'sense of number' however, one associated with a number line that extends from zero to infinity in either direction, is not the beginning and end of mathematical understanding. Case in point, it is not exactly compatible with modular arithmetic, which is perhaps more accurately represented as a cyclical system than a linear one. Trying to build an understanding of modular arithmetic on top of a preexisting conception of linear magnitude is a tough enough proposition when we are self-consciously aware of what the undertaking involves. The insidious side of the problem, however, lies in the fact that we often use symbols without explicit awareness of how they compete with our intended aims (e.g. this may even bear on our difficulties in teaching children how to tell time, another system that is based on modular arithmetic). When choosing symbols for pedagogical purposes we should step back and ask a very important question: What do these symbols communicate, and might this information work at cross-purposes to our aims? It is not a simple question to answer, but the first step is generating awareness that it needs to be asked in the first place.

Implications for cognitive psychology: Concepts vis-à-vis percepts

Several studies have implicated perceptual richness as a primary factor determining concreteness (e.g. Goldstone & Sakamoto; Schwartz, 1995; Sloutsky et. al., 2005). One major purpose of the above series of experiments was to highlight another dimensions of symbols, beyond simple perceptual or sensory input, that might affect concreteness. Indeed, in this case, the conceptual knowledge tied to Arabic numerals led to effects on learning and transfer that paralleled those of perceptually concrete symbols. Highlighting the effects of the conceptual information contained in a symbol set such as Arabic numerals, however, is a far cry from completely separating those effects from the effects of perception. It has been argued before that that attempts to divorce concepts and percepts are largely in vain (Barsalou, 2008; Goldstone & Barsalou, 1998). The logic of the argument is rather powerful.

For instance, I argued that the 'perceptual' information communicated by the iconic ants in Goldstone & Sakamoto (2003) might communicate conceptual information that was not communicated by the more idealized blob versions. In this case, it took the visual recognition of the icon as an ant in order to activate the *conceptual* information associated with the symbol. Just the same, the conceptual information communicated by the numerals is inherently linked to the tendency of participants to visually recognize the symbols as numbers. Hence, the activation of conceptual knowledge in this case begins with perception. The take home is that perception is neither purely sensory nor purely conceptual, and focusing on the sensory aspect at the expense of the conceptual side impoverishes our abilities to appreciate what makes a symbol concrete in the first place.

Indeed, the current definition of concreteness demands that we look at the ways that sensory and conceptual information are bridged, creating a state of affairs in which neither can be completely divorced from the other. This perspective is consistent with views of interactive specialization (see Schlaggar & McCandliss, 2007), which hold that neural circuits associated with different processes (e.g. visual and linguistic) change and form specific links with practice and development. In this way, an arbitrary symbol like the character 'A' comes to be both easily recognized by the visual system and easily recognized as standing for certain phonemes. In this way, the symbol unites the sensory and the conceptual side of things with practice.

This union between percepts and concepts may be especially true in the case of number. Some have argued that our conceptual knowledge of numbers is rooted in the approximate number system, a phylogenetically ancient perceptual system that allows us (and squirrels and lions, for that matter) to discriminate between numerosities of different cardinal values (Dehaene, 1999; Dehaene & Cohen, 2007). Others have begun to present evidence that training using linear external representations of number can help push children's developing knowledge of number concepts (Ramani & Siegler, 2008; Siegler, 2009). Hence, it may be that our *concepts* of numbers are fundamentally rooted in percepts. This has the interesting implication that activating number concepts may automatically activate a sort of perceptual information, and this perceptual information may account for some of the effects presented in the experiments above.

Conclusion

Even if number presents a somewhat special case, and I believe it does, the case for conceptual concreteness does not begin and end with number. Findings from Bassok et. al. (1998) suggest that semantic alignments in word problems affect solution strategies (e.g. when thinking of ‘apples’ and ‘baskets’, learners will divide apples into baskets, but not vice versa). Further, Son & Goldstone (2009) have recently found that several manipulations that affect a learner’s perspective taking can lead to differences in learning and transfer that mimic perceptual concreteness. Most recently, Novick, Catley & Funk (in preparation) have found that the conceptual information associated with well-known biological organisms can function in ways that parallel perceptual concreteness as well.

This is all to say that the current project fits well alongside an emerging body of work suggest that concreteness is about more than what meets the eye. Taking a hard look at concreteness means taking a hard look at symbols and realizing that they bring much more to bear on our cognitive architecture than we casually appreciate. By attending to these issues, hopefully we can advance our understanding of this architecture and how best to build the knowledge structures that it supports.

CHAPTER VI

GETTING SOLID ON CONCRETENESS

For a word that is meant to connote something solid and definite, ‘concrete’ is pretty soft. Think for a minute about what might count as a concrete example: Is it something solid, that can be touched? Perhaps a visual depiction? An anecdote that one can easily relate to? Maybe just a simpler version of a formal equation? Each of these very different alternatives might be taken, in different contexts, to be concrete. Beyond vague appeals to intuition, we lack a clear picture of what the term *concrete* means precisely and what the construct offers. Yet the term persists, playing a role in many arguments about learning and cognition.

Despite this lack of clarity, there is currently much theorizing about what concrete representations may or may not be good for. In the realm of education, many champion the use of concrete manipulatives as key tools for promoting learning, while others urge caution, citing the limits and occasional pitfalls of concrete examples (Ball, 1992; Clements & McMillen, 1996). Similar concerns occupy developmental and cognitive psychologists, who currently explore the merits and drawbacks of abstract versus concrete materials for learning and transfer more generally (Koedinger, Alibali & Nathan, 2008; Sloutsky, Kaminski & Heckler, 2005; Uttal, Scudder & DeLoache, 1992). Consideration of concrete examples, however, has a reach that extends far beyond the rather exclusive realm of academic research. The recommended usage of concrete examples is a mainstay of guides to good communication: for example, the College

Board advises all SAT takers to "use concrete examples and avoid generalities".[4] Such lay arguments hold that concrete examples both grab the audience's attention and help provide a foothold upon which further understanding can be built. Unfortunately, few of the aforementioned researchers or lay advisers go so far as to explain what exactly constitutes a concrete example.

The current theoretical discussion seeks to delimit the concept of concreteness and the role that it plays in symbolization. I will argue that although concreteness is often referred to in an off-hand way, the construct stands to add considerable leverage to our understanding of how symbols function. In giving concreteness its due attention, I seek to show that a rigorous treatment of concreteness may call for significant reassessment of our current theories of symbolization. Moreover, such revisions hold implications for our understanding of how our choices of symbols used for teaching can affect learning and transfer.

First, I provide an overview of current thinking about symbols, arguing that concreteness is an overlooked but essential dimension of symbolic thought, and offer a working definition of symbolic concreteness. This definition explicates the construct, clarifying the stakes in various arguments about the benefits versus the drawbacks of concreteness. Then, I discuss the potential implications of concreteness for learning and educational science. This more nuanced understanding of concreteness should provide us with new tools for assessing ways that different pedagogical strategies may support or impede learning. Finally, I end with a discussion of what general leverage provided by an information-based view of concreteness.

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[4] http://www.collegeboard.com/student/plan/boost-your-skills/122.html

Concreteness: A Key Component of Symbolism?

The first step in delimiting concreteness is recognizing that the term *concrete,* as used in psychology and educational science, is fundamentally concerned with symbols or external representations. Although the details of the definition are often left implicit, *concrete* is almost always counterposed to the term *abstract.* Comparisons of concrete vs. abstract are concerned with alternative ways to represent or instantiate the same underlying concepts or principles. These various alternatives all *stand for something* – some specific referent (e.g. a particular object, a mathematical operation, familial relationship, etc). Thus, any discussion of concreteness is necessarily a discussion about symbols[5]. It follows that the concreteness with which this discussion is concerned is symbolic concreteness. With this in mind, I begin with a discussion of current theories about symbols to provide some context for the analysis of concreteness that follows.

The basics of symbols and symbolism

DeLoache (2002) provides a commonly accepted working definition of symbols: “A symbol is something that someone intends to represent something other than itself.” Although DeLoache goes on to list several key components for the understanding of symbolic relationships, we need only consider two in order to get the gist of symbolic relations. These are 1) the dual nature of symbols, and 2) the fact that this duality is established by a triadic relation in which human intention acts as the primary unifying force.

[5] External representations run the gamut from icons, which bear direct resemblance to their referents, to arbitrary symbols. When considered in pedagogical contexts, however, they are always used with the intent that they stand for a particular referent. In this sense, these external representations all fit the definition of symbol used throughout this discussion.

By duality, Deloache means that a symbol exists both in a primary sense as an object in its own right *and* in a secondary sense as a *signifier* or reference to some other object – its *referent* (see also Barthes, 1964; Peirce, 1995; Saussure, 1959). For the sake of simplicity, I primarily use the term 'symbol' instead of 'signifier' in what follows. Note that I sometimes use the term 'object' to refer to a given symbol, though it is clear that symbols need not be objects at all (e.g. spoken words are symbols).

As for the role of intention, the link between signifier and referent exists because someone wills it to be so at some point in time. Often in the case of learning, that someone is a third party, be it a teacher, text book writer, or even some collective social consciousness, as is the case with assigning meanings to words in a language. Hence, the argument puts forth a symbolic triad of *symbol, referent,* and *intention*: symbolization is the process by which an intentional agent forges a relationship between one entity (a symbol) and another (its referent). From this theoretical vantage point, human intention is cast as the primary factor to be considered when theorizing about symbols (see Figure 11). Note that I argue that while important, this focus on intention may obscure the central role that concreteness plays in symbolic relations.

To be sure, much important developmental work makes the case for intention's vital role in the understanding of symbols (Deacon, 1998; Huttenlocher & Higgins, 1978; Leslie, 1987; Tomasello & Rakoczy 2003;Vygotsky, 1986). DeLoache's (1987) proposed dual representation hypothesis is one widely cited perspective in this line. The dual representation hypothesis argues that a major step in the development of symbolic ability lies in the understanding of symbol-referent relations; to think symbolically, one must be

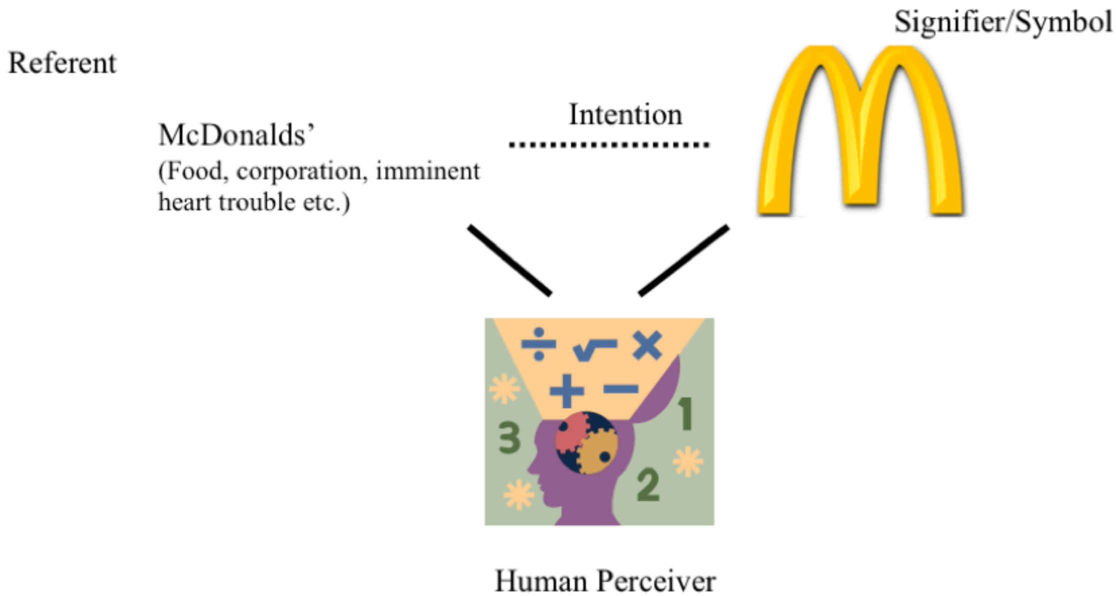

Figure 11. The symbolic triad. Note that intention is often that of a third party.

able to distinguish between symbol and referent. This ability demands that a perceiver be able to see a symbol not only as an object in its own right, but also as an intentional representation of some other referent (DeLoache, 2002).

Experiments using search tasks with scale models have provided compelling evidence for the validity of the dual representation hypothesis (DeLoache, 1987; Deloache, Miller & Rosengren, 1997). In the tasks, a child is shown, using a miniature replica of a room, where a doll is hidden in the real room. Typically, 30-month-old children fail to use the model to discover the hiding place of the doll in the real room, whereas 36-month-old children succeed in doing so. The dual representation view holds that the children fail because they do not understand the model of the room as a symbol that is *intended* to *stand for* the room. Prior to developing this representational insight, there can be no true symbolization, and these children cannot establish the link that imbues the model with the ability to serve as a proxy for the real room (DeLoache, DeMendoza & Anderson, 1999). The difference in performance between the age groups is attributed to the development of this ability. Using a particularly ingenious manipulation Deloache et. al. (1997) found that 30-month-old children successfully completed the task when tricked into believing that the scale model is actually the result of shrinking the real room. Because these children think the model is the room itself, they no longer have to deal with symbolic duality, so they can perform the task. Note that children experience such difficulties despite the fact that the scale model is an iconic replica. This provides a strong case for the importance of intention, even with iconic symbols.

The long line of work spawned by this research on dual representation foregrounds the importance of understanding intention, at the same time, it demands that we pay closer attention to other aspects of the symbolic triad of *symbol, referent,* and *intention* (DeLoache, 2000; DeLoache, 2002; DeLoache, et. al., 1997; Marzolf & DeLoache,1994; Troseth, Bloom Pickard & Deloache, 2007; Troseth & DeLoache, 1998). A significant part of the conversation concerns the idea that the choice of symbol might pose an obstacle to the establishment of the intended reference. Namely, any content or knowledge previously associated with a potential symbol might compete with its desired association with a new referent. For example, long ago Langer suggested that a peach would not make a good symbol because people care too much about peaches:

> A symbol which interests us also as an object is distracting. It does not convey its meaning without obstruction. For instance, if the word 'plenty' were replaced by a succulent, real, ripe peach, few people could attend to the mere content of the word…The more barren and indifferent the symbol, the greater its semantic power. Peaches are too good to act as words; we're too much interested in peaches themselves. (as cited in Shore, 1989, p. 177)

Something about our prior conceptions of peaches – as objects in their own right – would interfere with our abilities to use then as symbols for some other referent.

This example gets at the crux of my treatment of concreteness: symbols often communicate things other than those intended. That is, many objects that we intend to use as symbols for specific referents are already involved in prior relationships. When choosing a symbol for an intended referent, we must consider that there may already be other words, meanings, and experiences – prior knowledge or other content – associated with these candidate symbols. These previous relationships may, in turn, adulterate their involvements in any new symbolic relationships we wish to form. Langer's peach is but the simplest example. If I choose a stylized golden to stand for slope in a ninth grade algebra class, it may already be associated with McDonald's or even just the letter 'm' and its associated phoneme. Likewise, if I choose the letter 'n' in statistics class to represent a given probability, it may already be associated with the concept of sample size. The strength of these previous relationships may play a large part in determining how concrete a symbol is for a given observer. Thus, establishing an understanding of concreteness seems essential for proper understanding of symbols and symbolic thinking more generally.

Searching for a solid view of concreteness

A comprehensive survey of the literature reveals that 'concrete' and 'concreteness' remain vaguely defined terms. Concrete has alternatively been taken to refer to: a) symbol's physicality as opposed to the more mentalistic nature of a referent (Uttal et. al., 1997, McNeil & Jarvin, 2007), b) the high degree of iconicity of a given symbol in contrast with a more abstract alternative (Goldstone & Sakamoto, 2003), c) the degree of perceptual salience inherent in a given symbol relative to others (Sloutsky et. al., 2005), and d) the degree to which a symbol is embedded or situated within a particular context (Gentner & Medina, 1998; Goldstone & Son, 2005; Koedinger, Alibali & Nathan, 2007). These different conceptions of concreteness are not all given as explicit definitions but instead often lie implicit in the writings of various authors, with the operating definitions to be extracted from usage in context. These authors focus on how concrete objects or examples affect learning and transfer, sometimes arguing that concrete symbols can aid learning, and at other times arguing that concrete symbols impede learning and transfer. Interestingly, these authors rarely address the question of what concreteness *is*. Hence, the construct *concrete*, so frequently treated as an important variable influencing learning, often goes without explicit definition. As a consequence, problems of construct validity are endemic to discussions about the merits and demerits of symbolic concreteness.

One exception is the work of Kaminski (2006c), which offers a comprehensive definition of concreteness. She uses the term concrete not necessarily to imply tangible, physical objects, but rather as a way to describe something about the degree of contextualization of alternative representations of a given concept:

> Concrete versus abstract is not a dichotomy; it is a continuum where concrete instantiations provide the learner with more information than abstract instantiations. For a given concept, instantiation A is more concrete than instantiation B if A provides the learner with more information than B. Consider the increase in conveyed information as concreteness increases from a stick figure of a person to an elaborate drawing to a photograph to a real person. This conveyed information may be perceptual or conceptual in nature. (p. 4)

Kaminski’s information-based definition yields three implications that are critical to consideration of symbolic concreteness: 1) any object used in a symbolic relationship may have prior content that exists independently of that intended by any current intentional act of symbolization, 2) this content may be perceptual or conceptual in nature, and 3) this content may possibly either compete with or facilitate the intended symbolization process.

Kaminski’s formulation makes an important contribution to the field by offering a clear definition of the construct – one that we can revise and build upon. The logic of this knowledge-based construal of concreteness suggests the need for a corrective to the commonly held notion that a symbol equals a signifier plus a referent bound by intent, as this view somewhat overestimates the role of intention. It is certainly true that symbols result from an intentional link between a symbol and a referent. Because the symbol reader has to interpret the meaning of a symbol, however, his or her prior knowledge and perceptual apparatus mediate the symbolic triad and are, therefore, part and parcel of the

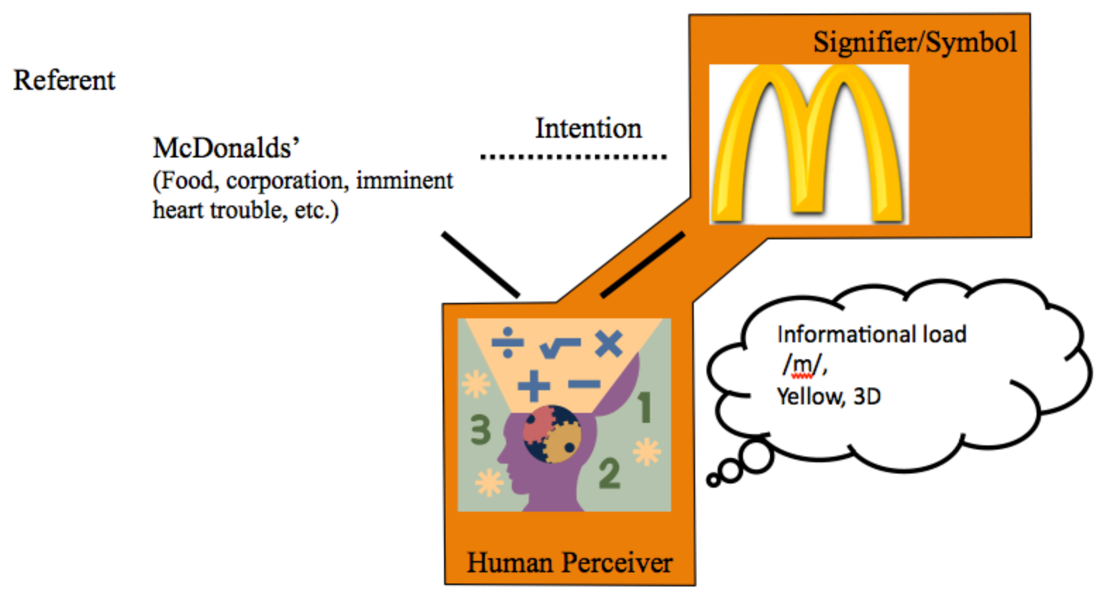

Figure 12. Concreteness as prior informational load on a symbol, represented in the cloud

symbol reading process (see Figure 12). We need a term that applies to the associated additional knowledge that inheres in – or perhaps more appropriately, adheres to – any would-be symbol for any given interpreter. I will argue that concreteness is that term.

In what follows, I first quickly outline the major components of my definition of concreteness and then explore each of these components in greater depth. *Concreteness* refers to the knowledge or content associated with a potential symbol, independent of the intended reference of the moment. Moreover, this content can be perceptual or conceptual in nature.

1. Concreteness refers to the information associated with an object and may be perceptual or conceptual in nature. It does not inhere in objects, but instead must be parameterized in terms of a) a particular symbol reader's prior knowledge associated with those objects, and b) an individual's perceptual expectancies and biases. Thus, it is not a constant term that can be quantified in absolute terms across individuals.
2. Concreteness should be manipulable. This point follows from point #1 because we know that both prior knowledge and perceptual expectancies and biases can be affected by experience.
3. The alignment of concreteness is of key importance. Because, concreteness about the prior content or knowledge that a given symbol brings to mind, the way this content aligns with or competes with the intended use of the symbol should play a major role in the way a given symbol affects thinking. This third point lays the foundation for the more practical part of my argument, which pertains to learning and transfer.

Component 1 – Concreteness refers to the information associated with an object and may be perceptual or conceptual in nature

Concreteness, at root, concerns the content associated with symbols and the potential competition or facilitation that those associations can hold for establishing the intended meanings of those symbols. This content can be perceptual or conceptual in nature. Recent experiments have marshaled evidence in support of this view.

Perceptually based concreteness. Much of Kaminski's work illustrates the ways that perceptual content contributes to concreteness for adults. Her tasks usually involve manipulating a set of objects used to instantiate a commutative group[6], a well-defined mathematical concept from abstract algebra that offers special qualities. Through a series of experiments with these tasks, she has built a convincing argument that perceptual attributes of a symbol do indeed contribute to concreteness for adults (Kaminski, 2006; Kaminski, Sloutsky, and Heckler, 2008; Sloutsky et. al., 2005). In particular, her experiments focus on how the perceptual salience of an object can import information that impedes learning and transfer in certain contexts.

For example, Sloutsky et. al. (2005) explored the effects of using sets of these artificially constructed groups with college undergraduates. The use of perceptually sparse 2-D symbols (i.e. the abstract group, ♦, ●,~) in contrast to perceptually richer screen images of novel 3-D symbols (i.e. the concrete group, ) served as a manipulation of relative concreteness. Participants were randomly assigned to one of two orders of symbol set presentation, either abstract-then-concrete or concrete-then-abstract.

[6] Mathematically speaking, a commutative group is a set on which a law of composition is defined, which is associative and has an identity element, and such that every element has an inverse (Artin, 1991).

They were trained on the rules of one symbol set and received a test phase immediately afterward. Next, they were trained and tested on the second symbol set. Experience with less concrete symbols transferred better to later performance with more concrete symbols (i.e. abstract-then-concrete) as opposed to vice versa. The authors performed a second experiment that helped confirm the operationalization of relevant concreteness, instead using the 3-D computer generated images from experiment 1 as abstract symbol set (i.e. ) and using photographs of identifiable real objects for the concrete set (i.e. ). They observed a similar effect, except this time, experience with the 3-D images facilitated performance with the identifiable real objects. Thus, both experiments demonstrated that training with relatively less concrete symbols in the first phase facilitated later performance with more concrete symbols in the second phase. Moreover, performance with the computer generated images provided a direct demonstration that a symbol considered to be concrete in one situation could be considered abstract in another, depending on what it was compared against. In a third experiment, the authors found that their participants generally fared better on the task when the perceptually sparse symbols were used than when the more perceptually salient ones were used. Altogether, they found that perceptually sparse abstract symbols led to superior learning and transfer relative to perceptually richer concrete symbols.

The authors posited a few possible mechanisms for these results: It might be that perceptual salience imposes some sort of cognitive load, leaving fewer resources for deeper conceptual processing. Alternatively, it may be that irrelevant aspects of a concrete representation can erroneously be interpreted as part of to-be-learned knowledge (see also Novick, 1988). Finally, they offered that concrete objects may have limited referential flexibility. That is, concrete objects may be ill suited for use as symbols because concrete objects are more likely to be interpreted as entities in themselves instead of as symbols.

Here, we should note that variations of DeLoache's scale-model experiment have provided evidence that somewhat parallels the findings of Sloutsky et al (2005). By manipulating relative size of the scale model, the similarity of furniture in the model or substituting photographs or maps of the room in place of the scale model, experimenters have manipulated the difficulty of the task, pushing the age of successful completion either backward or forward (see Marzolf & Deloache: Deloache, 2002). For instance, children can solve the scale model problem using photographs at a younger age (2.5 years) than they can while using physical models (3 years). A series of manipulations suggests that the more realistic the representation of the model is, the more difficult the task becomes. This can be interpreted as support for the hypothesis that perceptual salience affects concreteness for children and adults in similar ways.

Concreteness and conceptual knowledge. Kaminski (2006) suggested that conceptual information communicated by symbols might contribute to concreteness as well as their perceptual attributes, though no one has evaluated this possibility experimentally. If an object to be used as a symbol is already strongly associated with some particular knowledge, then this concreteness should compete with any newly intended referent that is not compatible with that knowledge. On the flipside, concreteness should make establishing a new intentional link easier to the extent that the intended referent easily maps onto the knowledge already associated with the object.

Recall that with Langer's peach, the argument was that information already associated with the peach (i.e. the peach as peach) might be at odds with the intended use of the peach as a symbol for something else. This argument seemed compelling enough that Uttal et. al., (1997) wrote a theoretically motivated piece warning that we might need to think a bit more deeply about the way that different attributes of manipulatives might interfere with their abilities to serve as learning aids. If the physical peach-as-object strongly activates thoughts of 'peaches', 'fruits', or 'edible things', then it should be harder to establish a situation in which the physical peach-as-object activates thoughts of 'the number 1' or 'quantity'. From this viewpoint, we can see that the peach-as-object might also be expected to pose a problem for adults relative to less familiar or salient objects, because the information associated with the peach – our prior knowledge – may need to be continually suppressed in order to establish new associations.

Of course, using it to represent the concept of 'fruit' should be much easier, as thoughts of fruit should already be activated. The *concrete* associations with physical peaches are much more aligned with the concept of fruit than with that of quantity, and this alignment should have different effects on the establishment of new referents. On a less conceptual note, a peach should more easily represent something that *looks* similar to a peach than something that does not (e.g. a nectarine versus an apple) independently of knowledge of peaches, due to pure perceptual similarities. The kernel of the argument is that knowledge associated with an object should affect the object's symbolic potential. Only recently, however, have empirical studies been conducted that provide evidence that bears on this argument, and they offer some support for Langer's thought experiment and a knowledge-based conception of concreteness.

Concreteness and the interface of conceptual knowledge and perception. From the outset, a holistic view of perception might predict that the concepts associated with a symbol can affect concreteness. If percepts are more than mere sensation (Gibson, 1929; Goldstone & Barsalou, 1998; Huttenlocher & Higgins, 1978; Kellman, Massey, Roth, Burke, Zucker, Saw, Aguero, & Wise, 2008), then we should expect that much of the prior knowledge that affects the perception of a symbol is more than mere sensation. Goldstone and Son (2005) provides a good experimental example of a situation in which the "perceptual" attributes seem to have contributed to the concreteness of selected symbols via the conceptual information they conveyed.

In this experiment, undergraduate students were trained on computer simulations using either concrete or abstract symbols to convey the same underlying principles of competitive specialization – a situation in which manipulating certain parameters of a system can lead to emergent equilibria.[7] Both simulations used identical cover stories about ants foraging for food to introduce the system to be learned. Concrete simulations used realistic depictions of ants pursuing food in the form of apples and oranges, and abstract ("idealized") simulations represented the ants as small black dots and represented food sources as solid, amorphous green patches. It is important to note that participants were given the exact same cover story with each simulation, only with different symbolic depictions.

Students were split evenly into four conditions based on training sessions: consistently idealized, consistently concrete, idealized-then-concrete (concreteness introduction), and concrete-then-idealized (concreteness fading). In the concreteness

[7] Software demonstrating this phenomenon can be downloaded at http://cognitrn.psych.indiana.edu/rgoldsto/complex/

fading condition, concrete versions of ants and food were used for the first 10 min of the simulation. Then participants received the message that, “We are now changing the appearances of the food and ants, but they still behave just as they did before,” and the abstract version was used for the final 10 minutes of training. In the concreteness introduction condition, the idealized version of ants and food was replaced by the concrete version after 10 min.

After the training session, students were trained on a separate set of simulations, which involved pattern recognition instead of foraging ants. This isomorphic transfer condition was actually governed by the same rules of competitive specialization. The experimenters found that students trained with concrete simulations were more accurate at *learning* tasks but worse at *transfer* tasks compared to those trained with abstract simulations. Moreover, they found that concreteness fading – initially beginning with concrete symbols and transitioning to abstract ones during learning – produced the best overall learning and transfer. The experimenters interpreted these results in terms of concreteness due to differing perceptual properties of the alternative symbols used for learning.

Despite the authors’ framing of the problem as one of perception, analysis of participant interviews suggested that at least part of the effect of the manipulation could be explained by the way that prior *conceptual knowledge* associated with symbol choice affected student thought. Students in the concrete training condition were more likely to give domain-specific, anthropocentric interpretations of the ants’ behavior (e.g. “one ant scares the other away” or “the ants are tempted by both food piles” for the concrete group versus “animals move quickly to food they are close to” or “It helps to make an ant move quickly at first and then more slowly" for the abstract group). These responses support the view that students were using conceptual information contained in the symbols (thinking in terms of little ant communities) when trained using concrete symbols as compared to when trained with abstract symbols. It appears that the perceptual information that counts in this case is the degree of similarity with real ants – which more strongly activates the ant concept and encourages participants to take the ants' point of view – instead of some difference in raw sensory data.

Taken together, Goldstone's and Sloutsky's experiments support the contention that the prior content associated with a symbol does in fact affect its ability to convey the intended information. Moreover, this content may be either perceptual or conceptual in nature.

Corollary to component 1 – Concreteness is in part a function of a perceiver's prior Knowledge

At this point, we should consider that what may activate prior knowledge and therefore communicate information for some, may not communicate similar information to others. In a philosophical piece, Willensky (1991) argued that, "concreteness is not a property of an object but rather a *property of a person's relationship to an object* [his italics]." At first glance, this position seems consistent with the well-documented differences in perception of identical stimuli based on expertise (see Bransford, Brown & Cocking, 1999; Chi & Ceci,1987; c.f. Piaget, 1950). Moreover, it is consistent with literature linking perception to expectation (Bruner & Postman, 1949; Carmichael, Hogan & Walter, 1932; Gibson, 1929; Pick, 1992). Together, these works provide additional leverage for theorizing about concreteness: it implies that concreteness should be characterized to some extent in terms of learners' prior knowledge. That is, a given symbol's concreteness is not absolute across persons, but must be evaluated on a case-by-case basis. Thus, we should expect differences among individuals and among groups of individuals depending on differences in prior knowledge. Hence, sweeping discussions of concrete versus abstract representations can obscure the necessarily idiosyncratic nature of concreteness.

Recent experimental evidence supports the view that concreteness depends upon learners' prior knowledge. Petersen and McNeil (2008) tested 3-year-old children's performance on a pair of counting tasks in a design that varied the type of objects being counted. Children were assigned to one of four objects types in a 2 (perceptually rich or not) x 2 (established knowledge or not) factorial design. The researchers found that 3-year old children's prior knowledge of the objects used for counting interacted with perceptual richness to determine performance. When objects were used for which children had established prior knowledge, perceptual richness hindered performance. However, when novel objects were used, perceptual richness aided performance.

Why did perceptual richness seem to help performance with unfamiliar objects and to harm performance with familiar ones? The richness-by-familiarity interaction indicates that perceptual richness *per se* is not the issue here. Perceptual richness may have highlighted surface features of the familiar objects, causing students to treat irrelevant features as though they were part of the to-be-learned concept. Hence, the authors concluded, that perceptually rich objects for which children had prior knowledge simply ceased to function as symbols of mathematical concepts. This would accord with prior work by Deloache and Marzolf (1992) showing children often fail to understand highly realistic objects to be symbols. By contrast, novel perceptually rich objects may have kept participants attention better than novel bland or perceptually sparse objects without communicating much extraneous information.

Some research on representational grounding seems to further support the view that concreteness depends on prior knowledge. For instance, Koedinger, Alibali & Nathan (2007) suggest that it is an associated network of “redundant semantic elaborations” that renders a representation grounded instead of abstract. It seems reasonable to view such grounding as a special case of concreteness based on the prior knowledge associated with a given symbol. Here we should note that the authors explicitly describe grounding as rooted in experience with particular representations as opposed to inhering in the choice of representation itself. The current framework takes this as more evidence that concreteness can only be properly understood in terms of learners’ prior knowledge.

Component 2 – Concreteness should be manipulable

If concreteness depends on the strength of prior associations, then it follows that there should be an *a priori* expectation that it can be manipulated. Any experiences that either strengthen or weaken preexisting links between a given symbol and the content associated with it should alter concreteness. This particular view on the malleability of concreteness is novel, if only because the explicit characterization of concreteness as a function of prior knowledge is new. Therefore, there are no current experimental studies that try to examine this possibility as such. These facts notwithstanding, multiple findings suggest that this view is reasonable.

Research has repeatedly shown that specific forms of practice can help children overcome difficulties with using various objects as symbols of other referents (DeLoache, 1987; DeLoache et. al., 1997; Liben & Downs, 1992; Triona & Klahr, 2005; Troseth & DeLoache, 1998; Uttal, 2000; Uttal et. al., 1997). Namy and Waxman (1998) presents a particularly interesting case from the developmental literature, which seems to illustrate the manipulability of concreteness. The experimenters introduced 18- and 26-month-olds to object categories using either novel words or novel gestures to name the objects. Although 18-month olds interpreted both novel words and gestures as symbols of the novel objects, 26-month olds did not initially recognize novel gestures as naming the novel objects. With training, however, the 26-month olds came to use the gestures as symbols for objects as well.

The researchers' interpretation of 26-month-olds' initial failure is telling: "Twenty-six-month-olds may have acquired an expectation that words but not gestures are presented within a sentence context, whereas the 18-month-olds have not yet developed this expectation" (p. 301). Because their prior knowledge indicated that sentences contained only words as elements, 26-month-olds did not use gestures as parts of sentences. Younger children, however, experienced no such competition with the establishment of the intentional link, because they had less prior competing knowledge. That additional experience – natural exposure for 18-month olds and the experimental intervention for 26-month olds – could alter children's performance with particular symbol types points toward the manipulability of symbolic concreteness.

Component 3 – The alignment of concreteness is of key importance

In prior work, Kaminski has maintained that it is useful to observe the distinction between *relevant and irrelevant concreteness* (Kaminski, 2006; Sloutsky, Kaminski & Heckler, 2005). Relevant concreteness refers to content associated with a symbol that is relevant to the to-be-learned concept. On the other hand, irrelevant concreteness refers to any content associated with a symbol that is extraneous to the concept, such as perceptual richness. I propose to add an amendment to this taxonomy, breaking relevant concreteness into *aligned* and *misaligned* concreteness. Indeed, it seems one major factor determining the effects of relevant concreteness should be the degree to which relevant associated knowledge is aligned with the to-be-learned structure.

For instance, the literature on analogy suggests that the concreteness of symbol can play a potentially large role both in the remindings that a symbol brings to mind during learning, and in aiding or hindering the structure mapping processes that are key for much of successful transfer (Gick & Holyoak, 1983; Novick, 1988; Sloutsky, Kaminski & heckler). To the degree that the information is aligned with structure, then it should facilitate learning. To the degree that such information is actually misaligned or directly contrary to the to-be-learned association, then there should be vigorous competition between the newly intended meaning and prior knowledge. In such a case, a symbol that brings to mind a misaligned schema may demand some sort of inhibition for proper use. On the other hand, irrelevant concreteness – content that is neither aligned nor misaligned – should not compete as directly with the newly intended use, but should still demand additional processing resources, retarding learning relative to more abstract symbols.

To review, c*oncreteness* refers to content – both perceptual and conceptual – that is already associated with a potential symbol. Moreover, concreteness must be understood in terms of an individual's prior knowledge and/or perception. Because it depends on prior knowledge, it should be affected by learning and experience, and the way that concreteness is aligned with a symbol's intended use should play a large part in determining the way a given symbol affects thinking.

Implications for Pedagogy and Learning

This view of concreteness has many potential implications for education in particular. One of the most fundamental questions facing educators is how to teach so that students learn content on more than just a superficial level. As Goldstone and Sakamoto (2003) point out, biology teachers want their students to understand the genetic mechanisms underlying heredity, not simply how pea plants look. Similarly, physics teachers want to teach general rules of motion, not simply how one spring uncoils. These examples illustrate a general challenge that educators face: to represent a to-be-learned concept, we must choose a symbol set to stand for that concept. This is true whether we use deictic pointing to communicate shared intention, use objects to stand in for other objects or processes, or use words to describe some altogether intangible systems or ideas. It seems then that symbol use is intrinsic to most teaching. As argued above, concreteness plays a role in determining how well a given symbol can communicate the information we intend. Thus, our understanding of symbolic concreteness stands to contribute significantly to our ability to choose symbols that will enhance teaching and best promote learning and transfer.

Historically, some have argued that the best approach to teaching is to use concrete symbols as tools to help promote initial learning that can undergo subsequent abstraction. Such views are based upon the Piagetian notion that children's thinking is inherently concrete, so can benefit most from a concrete scaffold (see Uttal et al, 1997). This has given rise to a situation in which, Ball (1992) lamented,

> Parents and teachers alike laud classrooms in which children use manipulatives, and Piaget is widely cited as having shown that young children need concrete experiences in order to learn. Some argue that all learning must proceed from the concrete to the abstract. Concrete is inherently good; abstract inherently not appropriate – at least in the beginning… (p. 16)

Counter to this current, some researchers, as described above, have argued that concrete symbols may be ill-suited to serve as teaching aids when compared to abstract symbols because they are more likely to be interpreted as objects themselves instead of as symbols that stand for other things.

Some have begun to gather experimental data on the ways that the concreteness of symbols we use to teach abstract principles affects the degree to which abstract principles are learned or transferred across contexts. The results of these investigations, however, have been mixed: Some findings suggest that abstract symbols tend to facilitate both learning and transfer better than concrete symbols do (Sloutsky et. al., 2005; see also Kaminski, Sloutsky & Heckler, 2008). Others suggest that more concrete symbols can speed initial learning, but that this concreteness can initially be an impediment to transfer (Goldstone & Son, 2005). Still others suggest that concrete symbols can speed learning for some learners while impeding it for others (Petersen & McNeil, 2008). Finally, some evidence suggests that concrete symbols help promote some aspects of learning while impeding other aspects (McNeil, Uttal, Jarvin & Sternberg, 2009). This tension among the results of various studies makes it difficult to establish a basis from which we can make practical decisions about which types of symbols are most useful for pedagogical purposes.

The information-based view of concreteness may help to settle some of these tensions and, by extension, ultimately contribute to informing practical pedagogical questions. The apparent divergence in results described above may be due in part to the lack of a well-defined construct of concreteness. Without a clearly defined conceptualization of the dimensions of concreteness, experimental manipulations cannot be properly compared. Indeed, it appears that different researchers have focused on different – perhaps independent – dimensions of concreteness. Kaminski et. al., (2008) focused on perceptual salience in terms of raw sensory information, whereas Goldstone & Son (2005) focused on perceptual similarity to some familiar object. Petersen and McNeil (2008, discussed above) began to try to disentangle the perceptual contributions to concreteness from the contributions of prior knowledge, but did not explicitly characterize perception and knowledge as components of concreteness. Moreover, none of the reviewed studies characterized concreteness in terms of alignment versus misalignment with prior knowledge. It may be that, with adequate elaboration, the theoretical implications of these various results may begin to converge.

Taking the alignment of concreteness with to-be-learned content into consideration may be especially helpful in yielding such a convergence. For instance, although the prior content associated with a given symbol may be aligned with the to-be-learned content in some aspects, it may be misaligned or irrelevant in others. Hence, it may be that concrete examples can sometimes help speed learning by facilitating a map between prior knowledge and the to-be-learned content. Nevertheless, the same prior knowledge that can speed learning should be expected to impede transfer when it is misaligned with the deep structure of the to-be-learned content. In such cases, learning may appear accelerated, but may come at the expense of decreased transfer or even the importation of some misconceptions from prior knowledge. Future studies should examine the potential tradeoffs that result from selecting among these different dimensions of concreteness.

For example, it is well known in the history of science that Rutherford used his prior knowledge of the solar system to make predictions about the structure and functioning of the atom (Gentner & Loewenstein, 2002). To the extent that the solar system schema was aligned with that of the atom (e.g. particles revolve around a nucleus, much as planets and other bodies revolve around the sun), predictions based on it should be expected to be correct, and they were. To the extent that the solar system schema was misaligned with the structure of the atom (e.g. bodies can revolve around the sun in orbits at whatever distance, but electrons can only exist in very discrete energy bands extending from the nucleus) those predictions should be expected to be in error, and indeed they were: The most salient example is the fact that Rutherford failed to predict discrete states for electrons. There was simply nothing in the solar system schema that would predict such a state of affairs, so Rutherford’s model fell short in those respects. The take-home message is this: the information-based definition of concreteness predicts that concrete examples may be aligned with to-be-learned knowledge in some respects and misaligned in others, and we accordingly should expect differential learning for different aspects of the core underlying principles.

In sum, much new experimental research confirms that the sorts of symbols we use to teach abstract concepts can indeed affect both the rate of initial learning and the degree to which learned knowledge can be transferred (Goldstone, Landy & Son 2008; Kaminski, et. al., 2008; McNeil et. al., 2009). What is not clear is how exactly these choices will exert their effects. By offering a clearly elaborated framework for analysis, I hope that the current model can provide a useful tool for explaining the implications of our choices of different symbols for teaching purposes.

Conclusion

In closing, I would like to anticipate and attempt to answer a challenge to the information-based conception of concreteness. A colleague and friend asked rather pointedly what *concrete* offers that perceptual biases, analogies, and prior knowledge do not. She wanted to know why we should keep the term *concrete* at all. The answer to this question is as simple as it is important: I am neither the first nor the last to use the term *concrete* in the field of psychology. It has a long history and is here to stay. *Concrete*, and its counterpoint, *abstract*, pervade our thought and our writing. This is due in part to the enduring influence of Piaget and his conception of concrete operational thought as "thought concerning objects that can be manipulated or known through the senses" (Piaget, 1953, p.136). Additionally, the term often goes unanalyzed because it is part of an everyday vernacular that we feel clearly communicates what we intend. Wittgenstein taught us, however, that terms that are used with such confidence often go undefined and can quite frequently lead to "bumps that the understanding has got by running its head up against the limits of language." (Wittgenstein, 2001, p. 41) This hazard is all the more dangerous in science.

Currently, *concrete* is frequently used and rarely defined. This is so in cognitive psychology, in education research, and among everyday practitioners. The information-based conception of concreteness seeks to operationalize *concreteness* and to make us self-consciously aware of its complexity. That it spans thought about perceptual biases, analogies, and prior knowledge is no more damning for concreteness than it is for other catch-all phrases like cognitive load. The value in these terms lies in the fact that they force us to take a critical gaze at how our stimuli affect perceivers and their abilities to perform or think about the tasks we have in mind.

The information based view of concreteness makes it clear that concrete is about much more than mere physicality. It helps us see that concreteness does not inhere in objects, but is largely a property of the interpreter or observer. It calls for us to reflect on the possibility that simple static marks on a page – like those that you are reading right now – may be as concrete as some physical objects in the information and meaning that they convey (see Kadosh & Walsh, in press, for a discussion of whether or not numbers are abstract). It invites us to ask why symbols that are very concrete for some are quite abstract for others and further invites us to examine the developmental pathways by which such concreteness is established, as in the Namy and Waxman (1998) case reviewed above. Moreover, it does this in a way that is in accord with both a) seminal works in psychology that have done much to inform the ways that we think about the role of *concrete* imagery in human thought (Paivio, 1965; Paivio, Clark & Khan, 1988) and b) current thinking in cognitive psychology that problematizes the distinctions that we raise between the ways that humans represent physical things-in-the-world and more mentalistic concepts (see Barsalou, 1999 on perceptual symbol systems). Finally, it has practical implications for informing pedagogical questions about what exactly constitutes a concrete teaching or learning aid (see Clements & McMillen, 1996). By turning a critical eye toward the concrete, the information-based view suggests that we think hard about a term that we use everyday. It seeks to solidify a construct that is already prevalent – and its *abstract* cousin – and in so doing, to help us do more solid science.

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Uttal, David. (2000). Seeing the big picture: map use and the development of spatial cognition. *Developmental Science*, *3,* 247-286.

Uttal, D. H., Scudder, K. V., & DeLoache, J. S. (1997). Manipulatives as symbols: A new perspective on the use of concrete objects to teach mathematics. *Journal of Applied Developmental Psychology*, *18*(1), 37-54.

Vygotsky, L. (1986). *Thought and language*. Cambridge: MIT Press.

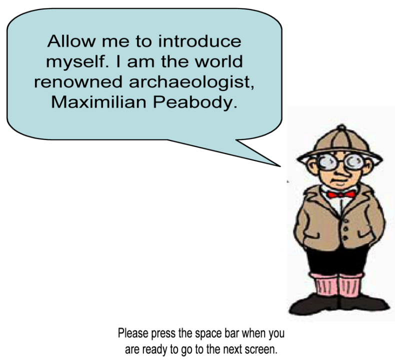
Washburn, D. A. (1994). Stroop-like effects for monkeys and humans: processing speed or strength of association? *Psychological Science*, *5*(6), 375-379.

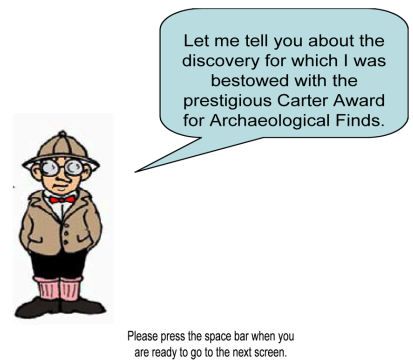
Wilensky, U. (1991) Abstract mediations on the concrete and concrete implications for mathematics education, in I. Harel, & S. Papert (Ed.) *Constructionism* (pp. 193-199). Norwood: Ablex.

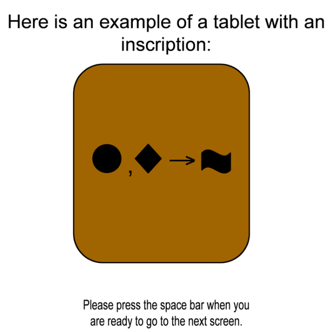
Wittgenstein, L. (2001). *Philosophical Investigations.* Malden, MA: Blackwell.

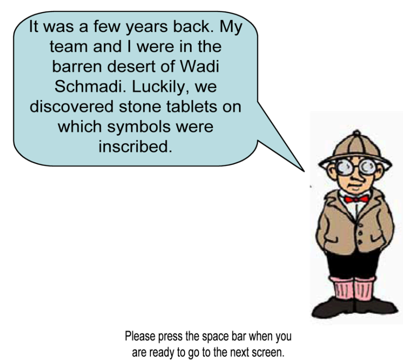
Appendix A

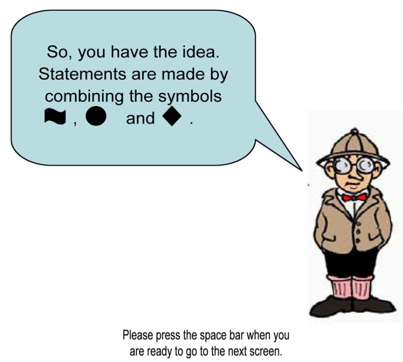
Screenshots from Abstract Symbol Training Phase

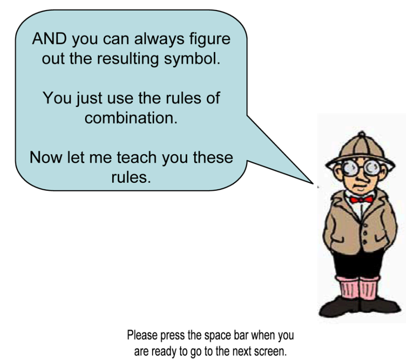


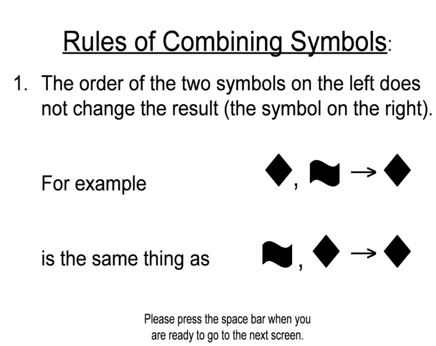


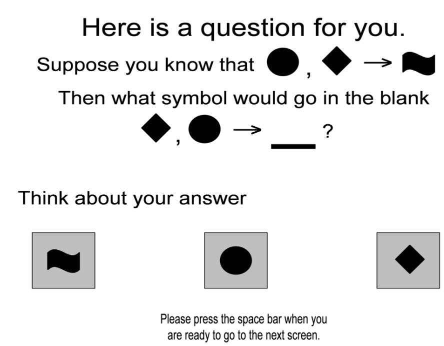


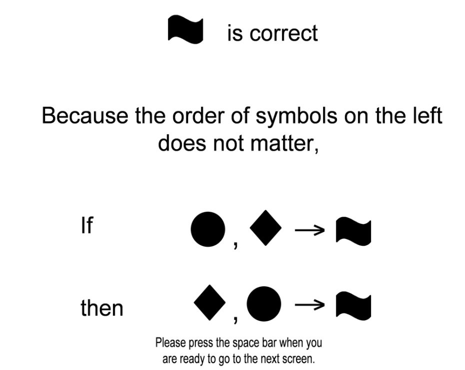


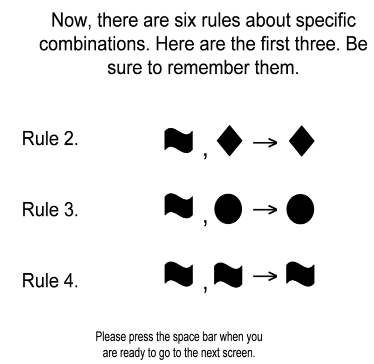

So, you have the idea. Statements are made by combining the symbols , and .
Please press the space bar when you are ready to go to the next screen.

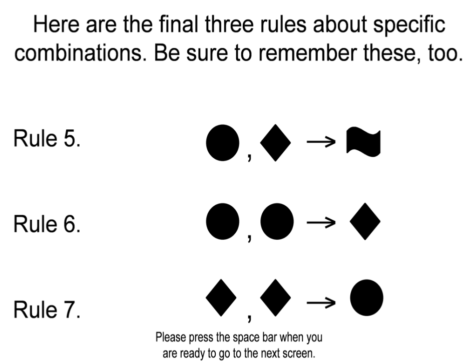

AND you can always figure out the resulting symbol.
You just use the rules of combination.
Now let me teach you these rules.
Please press the space bar when you are ready to go to the next screen.


Rules of Combining Symbols:
1. The order of the two symbols on the left does not change the result (the symbol on the right).
For example
is the same thing as
Please press the space bar when you are ready to go to the next screen.

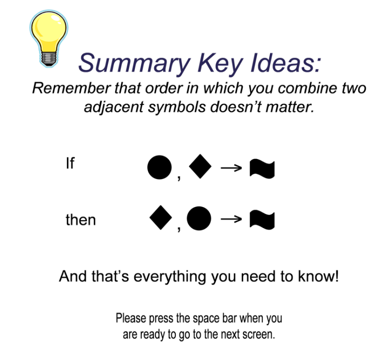

Here is a question for you.
Suppose you know that
Then what symbol would go in the blank
Think about your answer
Please press the space bar when you are ready to go to the next screen.

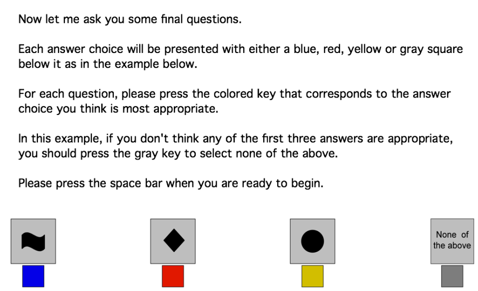

is correct
Because the order of symbols on the left does not matter,
If
then
Please press the space bar when you are ready to go to the next screen.

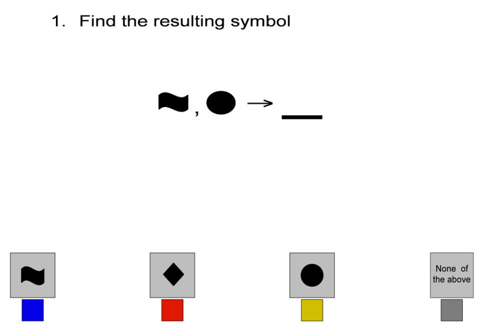

Now, there are six rules about specific combinations. Here are the first three. Be sure to remember them.
Rule 2.
Rule 3.
Rule 4.
Please press the space bar when you are ready to go to the next screen.

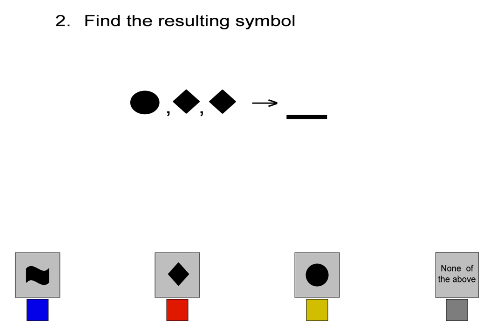

Here are the final three rules about specific combinations. Be sure to remember these, too.
Rule 5.
Rule 6.
Rule 7.
Please press the space bar when you are ready to go to the next screen.


There is one more thing you need to know about the language…
How more than two symbols combine
Please press the space bar when you are ready to go to the next screen.

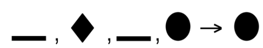

Summary Key Ideas:
Remember that order in which you combine two adjacent symbols doesn't matter.
If
then
And that's everything you need to know!
Please press the space bar when you are ready to go to the next screen.


Now let me ask you some final questions.
Each answer choice will be presented with either a blue, red, yellow or gray square below it as in the example below.
For each question, please press the colored key that corresponds to the answer choice you think is most appropriate.
In this example, if you don't think any of the first three answers are appropriate, you should press the gray key to select none of the above.
Please press the space bar when you are ready to begin.
None of the above


1. Find the resulting symbol
None of the above


2. Find the resulting symbol
None of the above

11. What symbols go in the blanks to make a correct statement?

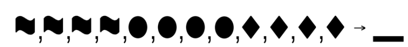


12. Which of the following symbols combine to give ◆ ?

13. How many ●'s could combine with themselves to get  ?

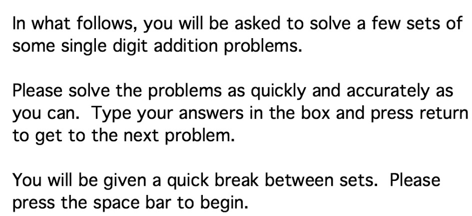
Four | Five | Six | Seven

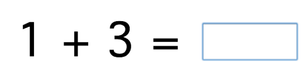
14. Find the resulting symbol

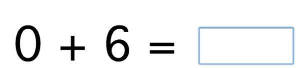


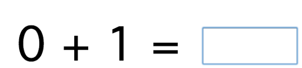
Appendix B

Screenshots from Addition and Font Comparison Warm-Ups







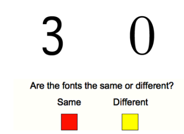


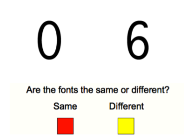
In what follows, you will be asked to judge whether or not the fonts of several pairs of numbers are identical.

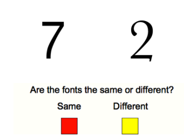
For each problem, please compare the numbers and decide if the fonts are the same or not. If they are the same, please press the (red) button indicating that they are the same. If they are different please press the (yellow) button indicating that they are different.

Please solve the problems as quickly and accurately as you can.

You will be given a quick break between sets of problems. Please press the space bar to begin.







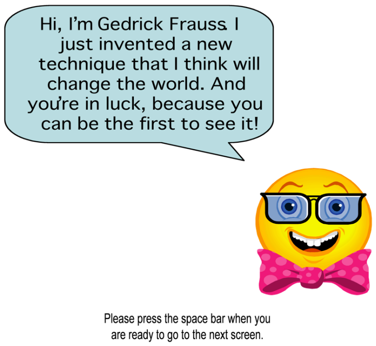
Appendix C

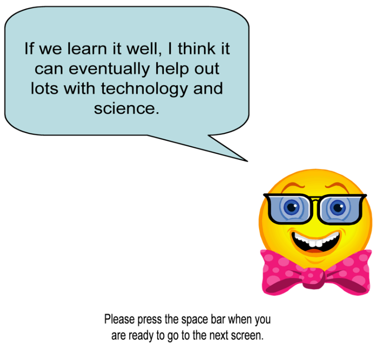
Screenshots from Aligned Concrete Symbol Training Phase

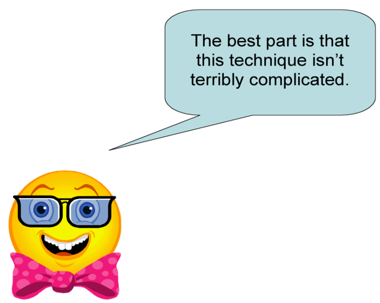
Please read the following information and answer the questions that are presented along the way.

When there is a question, please press the colored key that corresponds to your answer.

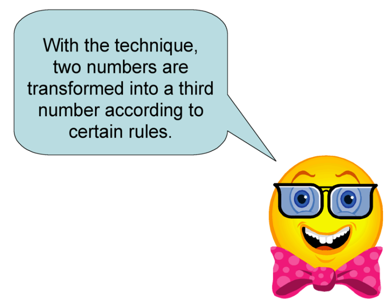
Please press the space bar to go to the next screen.







Please press the space bar when you are ready to go to the next screen.



Please press the space bar when you are ready to go to the next screen.

Only these 3 numbers are used:

0 1 2

Please press the space bar when you are ready to go to the next screen.

<u>Transforming Numbers</u>

Whenever two numbers are brought together, they are transformed into a third number. This process is written as:

First Number , *Second Number* ⟶ *Transformation Result*

Please press the space bar when you are ready to go to the next screen.

There is one more thing you need to know about the new transformation process…

How it works when more than two numbers are involved

Please press the space bar when you are ready to go to the next screen.

Rule 8. The result does not depend on which two symbols combine first.

For example: **2** , **1** , **2** → **2**

It does not matter if we do

2 , **1** first and then **2** or

1 , **2** first and then **2** .

Please press the space bar when you are ready to go to the next screen.

Let's find the result of the transformation:

1 , **2** , **1** → __

First remember that **1** , **2** → **0**

Next we have **0** , **1** → **1**

So the resulting symbol is **1**

Please press the space bar when you are ready to go to the next screen.

Let's summarize key ideas for the specific rules for transforming numbers…

Please press the space bar when you are ready to go to the next screen.

10. Do the following give the same result?

0, 0, 0, 1, 2, 2, 2, 1, 1 → __

0, 1, 2, 0, 1, 2, 0, 1, 2 → __

- Yes
- No

11. What numbers go in the blanks to make a valid transformation?

__, 2, __, 1 → 1

- 2 and 2
- 1 and 1
- 0 and 2
- 0 and 0

12. Which of the following numbers together transform to give 2 ?

- 2 and 2
- 1 and 1
- 0 and 0
- None of the above

13. How many 1's could combine with themselves to get 0?

- Four
- Five
- Six
- Seven

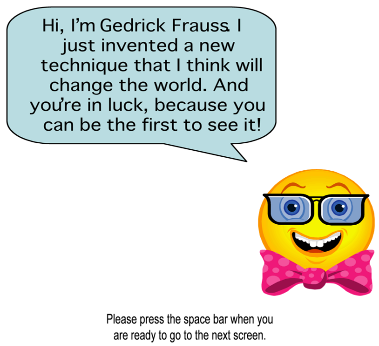
Appendix D

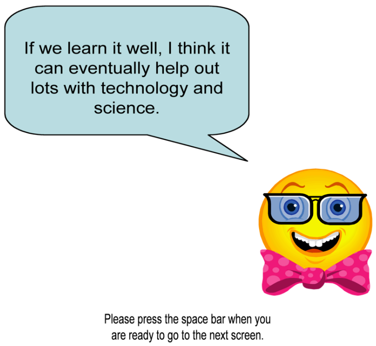
Screenshots from Misaligned Concrete Symbol Training Phase

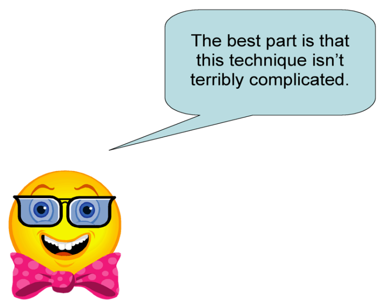
Please read the following information and answer the questions that are presented along the way.

When there is a question, please press the colored key that corresponds to your answer.

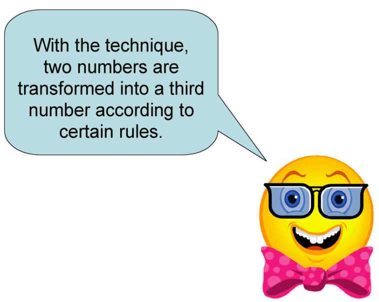
Please press the space bar to go to the next screen.







Please press the space bar when you are ready to go to the next screen.



Please press the space bar when you are ready to go to the next screen.

Only these 3 numbers are used:

2 1 0

Please press the space bar when you are ready to go to the next screen.

Transforming Numbers

Whenever two numbers are brought together, they are transformed into a third number. This process is written as:

First Number , *Second Number* ⟶ *Transformation Result*

Please press the space bar when you are ready to go to the next screen.

Summary Key Ideas:

Combinations of numbers are transformed into new numbers. Your job is to figure out what the end result of the transformations will be. The six possible transformations among pairs are:

$2, 2 \rightarrow 2$	$1, 1 \rightarrow 0$
$2, 1 \rightarrow 1$	$0, 0 \rightarrow 1$
$2, 0 \rightarrow 0$	$1, 0 \rightarrow 2$

Please press the space bar when you are ready to go to the next screen.

Summary Key Ideas:

Remember that order in which you transform two adjacent numbers doesn't matter.

If $1, 0 \rightarrow 2$

then $0, 1 \rightarrow 2$

And that's everything you need to know!

Please press the space bar when you are ready to go to the next screen.

Now let me ask you some final questions.

Each answer choice will be presented with either a blue, red, yellow or gray square below it as in the example below.

For each question, please press the colored key that corresponds to the answer choice you think is most appropriate.

In this example, if you don't think any of the first three answers are appropriate, you should press the gray key to select none of the above.

Please press the space bar when you are ready to begin.

1. Find the result of the transformation

$2, 1 \rightarrow \_\_$

2	1	0	None of the above
blue	red	yellow	gray

14. Find the result of the transformation.

0,0,0,0,1,1,1,1,2,2,2,2 → __

- 2
- 1
- 0
- We need more information to answer

15. **What symbol goes in the blank to make a correct statement?**

1,1,1,1,1,1,1,1,1, __ →2

- 2
- 1
- 0
- We need more information to answer

16. When my students were working on a chalkboard, part of a transformation was smudged so that it couldn't be read. They tried to figure out what it stated.

They did not know the result, but they did know that there were two numbers on the left; one of them was **0** .
They were trying to figure out what the result could be.
Here are some opinions of my students. Which do you agree with?

- The result could only be any number
- The result could only be **2** or **0**
- The result could only be **1**
- We need more information to answer

17. Find the result

2 , 1 , 2 → __

- 2
- 1
- 0
- None of the above

Appendix E

Screenshots from Droodle Symbol Training Phase

Which of the following images were you required to remember earlier?

Please press the key that corresponds to your answer

There were at least two images that you were not required to memorize at the beginning. If you can, please list the name or title associated with exactly one of those images.

Please write the answer in blank a) on your answer sheet.

Please press the space bar when you are ready to continue.

Which of the following images were you required to remember earlier?

Please press the key that corresponds to your answer

There were at least two images that you were not required to memorize at the beginning. If you can, please list the name or title associated with exactly one of those images.

Please write the answer in blank b) on your answer sheet.

Please press the space bar when you are ready to continue.

Which of the following images were you required to remember earlier?

Please press the key that corresponds to your answer

None of the above

There were at least two images that you were not required to memorize at the beginning. If you can, please list the name or title associated with exactly one of those images.

Please write the answer in blank a) on your answer sheet.

Please press the space bar when you are ready to continue.

Which of the following images were you required to remember earlier?

Please press the key that corresponds to your answer

None of the above

There were at least two images that you were not required to memorize at the beginning. If you can, please list the name or title associated with exactly one of those images.

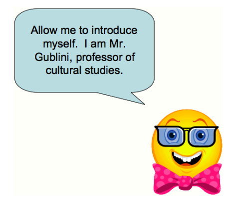
Please write the answer in blank b) on your answer sheet.

Please press the space bar when you are ready to continue.

Please read the following information and answer the questions that are presented along the way.

When there is a question, please press the colored key that corresponds to your answer.

Please press the space bar to go to the next screen.



Please press the space bar when you are ready to go to the next screen.

Please press the space bar when you are ready to go to the next screen.

Please press the space bar when you are ready to go to the next screen.

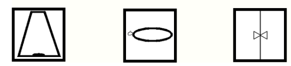
In the game, a group of children points to two or more different cards, one after another.

Then a different child who is "it" points to one final card. If this child points to the correct final card, then he or she is the winner.

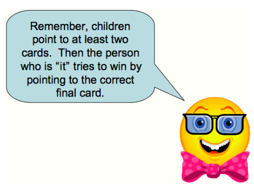
There are 3 kinds of cards, and the rules of the game state what the final card should be. So the rules always tell the winner what final card to point to.

Please press the space bar when you are ready to go to the next screen.

Here are the three kinds of cards used in the game:

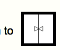


Please press the space bar when you are ready to go to the next screen.



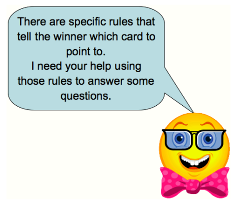
Please press the space bar when you are ready to go to the next screen.

Here is an example.

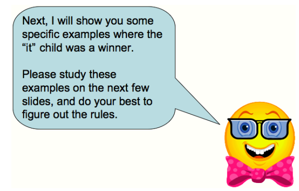
Some children pointed to  and then to 

Then the child who was "it" pointed to  and won.

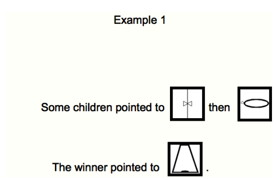
Please press the space bar when you are ready to go to the next screen.



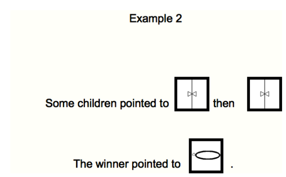
Please press the space bar when you are ready to go to the next screen.



Please press the space bar when you are ready to go to the next screen.



Please press the space bar when you are ready to go to the next screen.

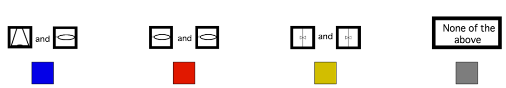


Please press the space bar when you are ready to go to the next screen.

2. What card does the winner point to when the other kids point to then then ?

3. If a group of kids wants the winner to point to , and they first point to , what other cards do they need to point to?

4. If the winner pointed to , what cards might the other kids have pointed to?



5. Which card do the children need to point to along with , so that the winner points to ?

Appendix F

Paper and Pencil Follow-Up Questions

Follow-up #1

How did you come up with your answer to the previous question?

Follow-up #2

How did you come up with your answer to the previous question?

Follow-up #3

How did you come up with your answer to the previous question?

Follow-up #4

Did either task from the experiment remind you of anything you've learned in the past? If yes, please describe.

Follow-up #5

To what extent do you agree with this statement (circle one)?

The tasks in the experiment reminded me of arithmetic.

Strongly Disagree   Somewhat Disagree   Somewhat Agree

Strongly Agree

1   2   3   4

Follow-up #6

Did any part of either task remind you of any rules/properties of arithmetic?

If so, please name them or give examples.

Follow-up #7

Does the equation below make sense to you? If so, what does it mean?

$$7 \bmod 4 + 3 \bmod 4 \equiv \_$$