Research Article

VERIFYING DIFFERENT-MODALITY PROPERTIES FOR CONCEPTS PRODUCES SWITCHING COSTS

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Abstract—*According to perceptual symbol systems, sensorimotor simulations underlie the representation of concepts. It follows that sensorimotor phenomena should arise in conceptual processing. Previous studies have shown that switching from one modality to another during perceptual processing incurs a processing cost. If perceptual simulation underlies conceptual processing, then verifying the properties of concepts should exhibit a switching cost as well. For example, verifying a property in the auditory modality (e.g., BLENDER-loud) should be slower after verifying a property in a different modality (e.g., CRANBERRIES-tart) than after verifying a property in the same modality (e.g., LEAVES-rustling). Only words were presented to subjects, and there were no instructions to use imagery. Nevertheless, switching modalities incurred a cost, analogous to the cost of switching modalities in perception. A second experiment showed that this effect was not due to associative priming between properties in the same modality. These results support the hypothesis that perceptual simulation underlies conceptual processing.*

Modern psychology relies heavily on the digital computer as a metaphor for human cognition (e.g., Fodor, 1975; Pylyshyn, 1984). According to this view, the software of the mind can be distinguished from the hardware of the body, with mental representations being amodal redescriptions of sensorimotor experience. Increasingly, however, researchers are arguing that this approach is fundamentally wrong, suggesting instead that interactions between sensorimotor systems and the physical world underlie cognition.

For example, Barsalou's (1999) theory of perceptual symbol systems proposes that conceptual knowledge is grounded in sensorimotor systems. To represent a concept, neural systems partially run as if interacting with an actual instance. For example, to represent the concept *CHAIR*, neural systems for vision, action, touch, and emotion partially reenact the experience of a chair. Increasingly, behavioral evidence supports this view (e.g., Klatzky, Pellegrino, McCloskey, & Doherty, 1989; Solomon & Barsalou, 2001, 2002; Spivey, Tyler, Richardson, & Young, 2000; Stanfield & Zwaan, 2001; Wu & Barsalou, 2002; Zwaan, Stanfield, & Yaxley, 2002), as does neural evidence (e.g., Martin, 2001; Martin & Chao, 2001; Martin, Ungerleider, & Haxby, 2000; Pulvermüller, 1999). (See Barsalou, 1999, in press, and Glenberg, 1997, for further evidence.)

Several aspects of sensorimotor simulations are important for the experiments we report here. First, simulations are componential, not holistic. Rather than being like a holistic video recording, a simulation contains many small elements of perception—perceptual symbols—organized coherently. Second, perceptual symbols arise on all modalities of experience—vision, audition, smell, taste, touch, action, emotion, introspection, and so forth. Third, perceptual symbols vary in accessibility. On a given occasion, only those perceptual symbols most active enter a simulation, such that the simulations of a concept vary considerably across occasions. Furthermore—and most important for our purposes—the modalities represented in simulations vary as well. On one occasion, the simulation of a concept might focus on how an object looks (e.g., a *LEMON* is *yellow*); on another occasion, a simulation might focus on how the object tastes (e.g., a *LEMON* is *sour*).[1] Although multiple modalities may typically be represented, one may often be more salient than others. Furthermore, over time, the focus may remain in a single modality, or it may switch from one modality to another.

If switching between modalities occurs during conceptual processing, then a phenomenon from the perception literature is relevant. Spence, Nicholls, and Driver (2000) had subjects discriminate whether a signal occurred on the left or the right in any of three modalities monitored simultaneously: vision (a light), touch (a touch on a finger), and audition (a tone). When two consecutive signals occurred on the same modality, processing stayed within a single system. When consecutive signals occurred on different modalities, processing had to switch between systems. Spence et al. found that switching modalities incurred a cost: Detecting a signal was slower when the previous signal was on a different modality than when it was on the same modality (also see Spence & Driver, 1998).

If conceptual processing utilizes sensorimotor systems, then an analogous cost should occur when conceptual processing switches from one modality to another. To investigate this prediction, we used the property-verification task. On each target trial, subjects verified a property in one of six modalities (vision, audition, taste, smell, touch, action). For example, subjects might verify the auditory property *loud* for *BLENDER*. On the previous trial, subjects verified a property from a different concept either on the *same* modality or on a *different* modality (e.g., *LEAVES-rustling* vs. *CRANBERRIES-tart*). Table 1 provides examples of the critical materials. Because the concepts on the two trials were always unassociated, no associative priming between concepts should have occurred. Also, a high ratio of filler trials to critical trials masked the purpose of the experiment (i.e., the number of paired trials on the same modality was relatively small). The key prediction was that having to switch modalities would slow verification time, relative to staying within the same modality, an effect analogous to modality-switching costs in perceptual processing.

Experiment 1 also explored whether the stimulus onset asynchrony (SOA) between presentation of the concept and presentation of the property is a factor in switching costs. Perhaps switching costs disappear when the property lags behind the concept, because the concept has longer to activate properties across modalities. Alternatively, switching

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1. Notationally, we use uppercase italics to represent concepts, lowercase italics to represent properties, and quotes to represent linguistic forms (words, sentences).

Table 1. *Examples of the target and context trials from the six modalities in Experiment 1*

Modality	Target trial	Context trial: Same modality	Context trial: Different modality
Audition	*BLENDER-loud*	*LEAVES-rustling*	*CRANBERRIES-tart*
Vision	*BABY CLOTHES-pastel*	*HAIR-fair*	*TOAST-warm*
Taste	*CUCUMBER-bland*	*BUTTERMILK-sour*	*BIRD EGG-speckled*
Smell	*SOAP-perfumed*	*OLD BOOK-musty*	*TELEVISION-noisy*
Touch	*MARBLE-cool*	*PEANUT BUTTER-sticky*	*BED SPRINGS-squeaking*
Motor	*FAUCET-turned*	*ROCK-hurled*	*HIGHWAY SIGN-green*

Note. The context trial immediately preceded the target trial. The concept and property were presented in a sentence frame stating the possibility that the "*CONCEPT* can be *property*."

costs may remain constant across SOAs if subjects do not commit to a dominant modality until receiving the property word. To assess these possibilities, some subjects received the concept and property on each trial simultaneously (SOA = 0 ms), whereas others received the concept first, followed by the property 260 ms later (SOA = 260 ms).

EXPERIMENT 1

Method

Subjects and design

Sixty-four volunteers from Emory University participated for course credit. Thirty-two were assigned randomly to each of the two between-subjects SOA conditions. Same versus different modality was manipulated within subjects, with equal numbers receiving the two counterbalanced versions of the list of concept-property items.

Materials

A set of 100 concept-property items was developed. Each property was more salient on one modality than on the others. We selected 26 properties from vision, 24 from motor actions, 18 from audition, 12 from touch, 12 from taste, and 8 from smell. Because some modalities have more words for properties than others, the number of properties differed across modalities by necessity.

From the 100 concept-property items, 50 pairs were formed. Half contained two properties from the same modality; half contained properties from different modalities. The two items forming a same-modality pair were chosen randomly from items on the relevant modality. According to the norms of Nelson, McEvoy, and Schreiber (1999), the properties in these pairs were not associated.[2] One item in each same-modality pair was randomly assigned to be presented first (the context item), and the other was presented second (the target item). Table 1 presents an example from each modality. The two items forming a different-modality pair were chosen randomly from the remaining items. In pairs of both types, if the two concepts exhibited a relation, they were re-paired with items having no relation. Two lists were created such that each target had a same-modality context in one list but a different-modality context in the other. Thus, each target item appeared with both same-modality and different-modality contexts, counterbalanced across lists. All critical properties were true of their respective concepts.

The experimental trials included 150 pairs, 50 of which were critical, for a total of 300 trials. The 100 filler pairs were designed to mask the nature of the experiment. Fifty contained two false items, 25 contained a true item then a false item, and 25 contained a false item then a true item. Thus, "true" and "false" responses were equally likely overall. Properties in the fillers sometimes referred to a specific modality, but also referred to properties that are represented on multiple modalities (e.g., *CAMERA-compact*, *TOY-plastic*, *MAP-complicated*). The concept and property in many false items were related (e.g., *OVEN-baked*, *BUFFALO-winged*, *BUTTERFLY-bird*), to ensure that subjects actually verified the properties of concepts (Solomon & Barsalou, 2002). The critical and filler pairs were randomly intermixed for each subject. All concepts and properties were used only once. The practice trials consisted of 24 true items and 24 false items, and were similar in nature to the experimental trials.

Procedure

Each trial began with a fixation stimulus (* * * * *) two lines above where the concept name would appear. After 500 ms, the fixation stimulus disappeared. In the 0-ms SOA condition, three lines of text then appeared aligned vertically, each separated from the next by an empty line. The first line of text contained the concept word in uppercase; the second line contained the words "can be" in lowercase; the third line contained the property word in uppercase. In the 260-ms SOA condition, the concept word appeared for 160 ms, then "can be" was added for 100 ms, then the property name was added. Response times (RTs) were measured from the onset of the property word. All lines of text remained on the screen until the subject made a "true" ("?/" key) or "false" ("z" key) response.

The initial instructions emphasized that a decision should be based on whether the property was "usually true" of the concept. For example, the pair *CARNATION-black* could theoretically be true, but *black*

2. Although most of the target properties were found in these norms, not all were. Those properties found did not have their paired properties as associations. Those properties not found in the Nelson et al. norms were comparable, appearing unassociated. Experiment 2 addressed associativeness directly and found that it was not a factor in these experiments.

would be a highly unusual property for *CARNATION*. Therefore, the correct response to this pair was "false." Subjects received feedback for 600 ms after pressing the wrong key ("ERROR") or after taking 2,000 ms or longer to respond ("TOO SLOW"). The next trial began 300 ms after the response, or in the case of feedback, 300 ms after the feedback disappeared. Because subjects responded to each item individually, nothing indicated that items were paired in the underlying design. Also, because only 1 of every 12 trial transitions contained properties from the same modality, it was not obvious that modality switching was of interest.

The experiment began with 48 practice trials, followed by the 100 critical and 200 filler trials in a different random order for each subject. After each block of 50 trials, subjects took a brief break and saw the percentage of errors from the previous block. If errors exceeded 15%, subjects were urged to be more accurate. If errors fell below 5%, subjects were complimented. When ready, subjects began the next block.

Results and Discussion

RTs for target trials on which errors occurred were removed from the data before analysis. RTs for target trials were also removed when subjects erred on the previous context trial, given that an assessment of modality switching assumes that subjects processed both the context and target items correctly. When subjects erred on a context trial, a variety of complicating factors could have affected processing on the target trial. Median RTs for same-modality versus different-modality target trials were computed for each subject and then averaged across subjects.

As Table 2 illustrates, RTs on the target trials were slower when the modality switched from the context trial to the target trial than when modality remained constant, $F(1, 62) = 6.87$, $p < .05$. Although the switching effect was slightly larger in the 0-ms SOA condition than in the 260-ms SOA condition (29 ms vs. 20 ms), the interaction between SOA and switching was not significant, $F(1, 62) = 0.24$. There were no effects for errors, indicating that a speed-accuracy trade-off was unlikely.

The effect of SOA was significant, $F(1, 62) = 56.12$, $p < .01$. Subjects in the 260-ms SOA condition were 270 ms faster than subjects in the 0-ms SOA condition. The near equivalence between the difference in RTs and the difference in SOAs indicates that subjects in the 260-ms SOA condition began task-relevant processing immediately on receiving the concept in isolation. By the time the property appeared, these subjects were further into the necessary processing than the 0-ms SOA subjects. Most important, however, the effect of modality switching occurred for both groups.

We began with the hypothesis that modality-specific brain areas represent properties in concepts. On the basis of this assumption, we predicted that switching modalities while verifying properties would incur a processing cost, analogous to the cost incurred while switching modalities in perceptual processing. Unlike in perceptual studies, however, the switching costs here occurred while subjects processed linguistic stimuli, not perceptual ones. This suggests that the linguistic stimuli initiated sensorimotor simulations, which behaved similarly to sensorimotor processing.

EXPERIMENT 2

An alternative explanation remains to be addressed. Perhaps properties across all modalities are stored together in a single system of amodal knowledge, and within this system, amodal symbols that represent properties from the same modality are associated to each other, such that they prime each other when processed sequentially. If so, then these associations could underlie the switching costs in Experiment 1: When a subject verifies two properties from the same modality, associations between their amodal symbols might speed processing, relative to processing of properties from different modalities, whose symbols are not associated.

As already noted, the critical property pairs in Experiment 1 were not associated in the Nelson et al. (1999) norms. Perhaps, however, these norms are not sufficiently sensitive to detect weak associations that link properties from the same modality. This hypothesis can be tested by using highly associated property pairs from the Nelson et al. norms. If associations speed same-modality pairs whose normed associative strengths are 0 (i.e., nonmeasurable), then even greater priming should occur as associative strength increases.

Thus, Experiment 2 sampled pairs of properties that are highly associated (e.g., *spotless-clean*, *polyester-cheap*) in the Nelson et al. (1999) norms. These associated properties were then combined with concepts to form pairs of verification trials (e.g., "SHEET can be SPOTLESS"—"AIR can be CLEAN"; "SHIRT can be POLYESTER"—"MEAL can be CHEAP"). If the associative hypothesis is correct, then substantial priming would be found for the second members of these pairs, relative to second members of pairs in which the context and target items had unassociated properties (e.g., "SHEET can be SPOTLESS"—"MEAL can be CHEAP").

In contrast, we did not predict an effect of association. Many previous studies have found that priming diminishes substantially—and typically disappears—when an unrelated word separates two associated words (Bentin & Feldman, 1990; Dannenbring & Briand, 1982; Joordens & Besner, 1992; Masson, 1995; McNamara, 1992). Given that three words stood between properties on adjacent trials in Experiment 1, it seems unlikely that the first property could have primed the

Table 2. *Mean reaction times and error rates for verifying properties on target trials in Experiment 1*

Context trial	0-ms SOA Reaction time (ms)	0-ms SOA Error rate (%)	260-ms SOA Reaction time (ms)	260-ms SOA Error rate (%)
Same modality	1,124 (27.8)	5.1 (0.83)	859 (23.3)	5.0 (0.71)
Different modality	1,153 (28.9)	5.6 (1.38)	879 (24.7)	4.0 (0.76)
Switching cost	29	0.5	20	−1.0

Note. Standard errors are in parentheses. SOA = stimulus onset asynchrony.

second associatively. For example, in "LEAVES can be RUSTLING" followed by "BLENDER can be LOUD," the three words "BLENDER can be" lie between "RUSTLING" and "LOUD." We expected that these intervening words would extinguish any possible priming.[3]

In addition to the strongly associated properties, we presented pairs of unassociated properties from the same versus different modalities (a replication of Experiment 1). We did this, first, because we wanted to replicate the modality-shifting effect and, second, because we wanted to directly compare this effect with any associative priming effect. Because highly associated properties are necessary for testing the effect of association, whereas unassociated properties are necessary for testing the modality-switching effect, different property pairs were used to test the two effects.

Method

Subjects and design

Eighty-eight volunteers from Emory University participated for course credit. Associated versus unassociated pairs were manipulated within subjects, as were same versus different modalities, with counterbalanced versions of the list of concept-property items being distributed equally across subjects.

Materials

Thirty pairs of associated properties that averaged 23.1% in associative frequency (i.e., how often the second property was produced as an association of the first) were selected from the Nelson et al. (1999) norms. This is a very high level of associative strength, as approximately 95% of the words in the norms have a lower first associate (D.L. Nelson, personal communication, January 23, 2002). The first property in a pair was always the cue in the norms, and the second property was always a response. The introduction to this experiment provides examples.

Two critical lists were formed from the 30 pairs of associated properties. In one list, 15 of the associated pairs remained intact, and the other 15 were scrambled to form unassociated pairs (as illustrated in the introduction). In the other list, the first 15 pairs were scrambled to form unassociated pairs, whereas the second 15 pairs remained intact to form associated pairs. Each property was combined with a concept for which the property was true.

An additional 30 pairs of trials were selected from the materials of Experiment 1. Two lists were created so that each list contained 15 pairs from the same modality and 15 pairs from different modalities. For the same-modality pairs, the associative strength between properties was 0. Across lists, each concept-property combination occurred in both conditions.

An additional set of 120 filler pairs was constructed. Sixty contained two false items, 30 contained a true item then a false item, and 30 contained a false item then a true item. Fifteen of the false filler items were associatively related to the previous trial, and 15 were in the same modality as the previous trial. Thus, the relation between two consecutive trials was not predictive of the correct response for the second trial. Subjects could not give a "true" response on the basis of the previous item being associated or in the same modality. The remaining 90 filler pairs were unrelated. The practice materials consisted of 48 additional trials that were comparable to the experimental materials. No concept or property was repeated across the materials.

Procedure

The 0-ms SOA procedure from Experiment 1 was used.

Results and Discussion

As in the previous experiment, target trials were removed from the analysis when subjects responded incorrectly either on the target trial or on the previous context trial. Median RTs in the relevant conditions were computed for each subject and then averaged across subjects.

As Table 3 illustrates, an associative priming effect did not occur in the RTs, $F(1, 87) = 0.016$, or in the errors, $F(1, 87) = 0.22$. Responses to unassociated properties were not reliably slower than responses to associated properties, indicating that associations between properties did not underlie the switching effect in Experiment 1. Indeed, RTs in these two condition differed by only 1 ms. In contrast, the RTs were 41 ms slower on the different-modality trials than on the same-modality trials, $F(1, 87) = 9.40$, $p < .01$. The two-way interaction showed that the difference between associative priming and modality switching was nearly significant, $F(1, 87) = 3.76$, $p = .056$. The error data did not show a significant effect of modality switching, nor a significant interaction.[4]

As these results show, associative strength does not explain the switching costs in Experiment 1. If associations between properties from the same modality had been responsible, an associative effect should have occurred in Experiment 2, given that the property pairs in Experiment 2 were much more associated than those in Experiment 1. Instead, no associative priming effect was obtained, whereas there was again a reliable effect of modality switching. This leaves modality-specific processing as the best account of the switching costs. After a property is verified, attention rests on its modality. If the subsequent property resides on a different modality, attention must shift, thereby incurring a cost.

GENERAL DISCUSSION

According to perceptual symbols theory (Barsalou, 1999), simulations in sensorimotor areas represent properties during conceptual processing. If the conceptual system rests on sensorimotor systems, then phenomena in perceptual processing should also occur in conceptual processing, at least to some extent. Thus, the presence of switching costs in perceptual processing suggests that analogous switching costs should occur during property verification.

The results of Experiments 1 and 2 support this prediction. When subjects verified pairs of properties, they verified the second property faster when it came from the same modality as the first property than

3. One might argue that the words "can be" actually do not count as full content words, and thus may not decrease priming. Nevertheless, there is still one completely unrelated content word between the two properties, namely, the concept for the second one. The work just cited shows that even one intervening word can dissipate priming between two related words.

4. An earlier experiment also showed no effect for a large manipulation of associative strength in a similar design. Thus, the absence of an associative effect appears robust.

Table 3. *Mean reaction times and error rates in Experiment 2*

Context trial	Reaction time (ms)	Error rate (%)
Associatively related	1,143 (15.4)	9.9 (0.82)
Associatively unrelated	1,144 (14.6)	10.2 (0.72)
Priming	1	0.3
Same modality	1,186 (14.9)	5.6 (0.63)
Different modality	1,227 (17.2)	6.5 (0.65)
Switching cost	41	0.9

Note. Standard errors are in parentheses.

when it came from a different modality. Experiment 2 ruled out the alternative hypothesis that associations between properties from the same modality were responsible. When subjects verified a pair of associated properties, no priming occurred, even though the associations between them were considerably stronger than any associations that might have existed between properties from the same modality in Experiments 1 and 2. Thus, switching from one modality-specific brain system to another—not associative strength—appears to be the critical factor in these experiments. Recent neuroimaging work on the localization of concepts corroborates this conclusion (e.g., Martin, 2001; Martin & Chao, 2001; Martin et al., 2000), as does the literature on lesion-based conceptual deficits (e.g., McRae & Cree, in press; Simmons & Barsalou, in press). Recent functional magnetic resonance imagery work in our lab shows that verifying the six types of properties assessed in Experiments 1 and 2 activates the respective modality-specific neural systems (Pecher, Hamann, Simmons, Zeelenberg, & Barsalou, 2002).

One issue to be considered is the generality of the modality-shifting effect. Within the experiments reported here—and subsequent ones like them—all six modalities generally exhibited trends in the predicted direction (because of the noisiness of the data and the small number of properties on each modality, individual trends have rarely been significant). Across four replications of the basic paradigm—an unpublished initial experiment, the 0-ms SOA condition in Experiment 1, the 260-ms condition in Experiment 1, and Experiment 2—the mean differences between the same-modality RTs and the different-modality RTs were 37, 28, −7, and 65 for vision; 42, −2, −38, and 43 for audition; 48, 48, 20, and 39 for motor action; 86, 59, 104, and −18 for smell; 10, 32, 100, and 10 for taste; and −10, 42, −20, and −34 for touch. In a given experiment, not every modality showed a trend, but across experiments, each modality has shown one at least once.[5]

A related issue is whether modality-shifting effects occur for properties that do not come from the six modalities we have addressed, such as the properties of abstract concepts. Barsalou (1999) suggested that abstract concepts draw heavily on introspective experience, such as emotional states and cognitive operations. Wiemer-Hastings, Krug, and Xu (2001) provided evidence for this hypothesis. To the extent that other sorts of properties arise on different modalities of experience, effects of shifting between these modalities should occur as well. For example, if emotion and cognitive operations constitute different domains of introspection, they might exhibit shifting effects. This issue awaits further research.

Together with other recent evidence, the findings we have reported here converge on the conclusion that the conceptual system is grounded in sensorimotor simulation. It is becoming increasingly difficult to argue that the conceptual system is completely modular and amodal. To the contrary, the conceptual system appears to share many mechanisms with perception and action, thereby making it nonmodular and modal.

5. Because the critical items were rotated through a counterbalanced design, item analyses are technically not necessary (Raaijmakers, Schrijnemakers, & Gremmen, 1999). For the record, though, in three of the four cases for which an items test was possible, an effect was present: unpublished pilot experiment, $t(44) = 2.28$; Experiment 1 (SOA = 0), $t(49) = 2.44$; Experiment 1 (SOA = 260), $t(49) = 0.87$; Experiment 2, $t(29) = 3.92$. The critical effect is generally present across items.

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