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Representing Properties Locally

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Theories of knowledge such as feature lists, semantic networks, and localist neural nets typically use a single global symbol to represent a property that occurs in multiple concepts. Thus, a global symbol represents *mane* across *HORSE, PONY,* and *LION.* Alternatively, perceptual theories of knowledge, as well as distributed representational systems, assume that properties take different local forms in different concepts. Thus, different local forms of *mane* exist for *HORSE, PONY,* and *LION,* each capturing the specific form that *mane* takes in its respective concept. Three experiments used the property verification task to assess whether properties are represented globally or locally (e.g., Does a *PONY* have *mane?*). If a single global form represents a property, then verifying it in any concept should increase its accessibility and speed its verification later in any other concept. Verifying *mane* for *PONY* should benefit as much from having verified *mane* for *LION* earlier as from verifying *mane* for *HORSE.* If properties are represented locally, however, verifying a property should only benefit from verifying a similar form earlier. Verifying *mane* for *PONY* should only benefit from verifying *mane* for *HORSE,* not from verifying *mane* for *LION.* Findings from three experiments strongly supported local property representation and ruled out the interpretation that object similarity was responsible (e.g., the greater overall similarity between *HORSE* and *PONY* than between *LION* and *PONY*). The findings further suggest that property representation

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and verification are complicated phenomena, grounded in sensory-motor simulations.

The general construct of a *property* is central to research on concepts. Across the many disciplines that address cognition, researchers almost always assume that concepts contain properties. Theoretically, concepts are defined in terms of the properties that they contain (at least in part), and models of virtually all conceptual tasks rely heavily on the processing of properties (e.g., similarity, learning, and inductive inference). In empirical investigations, researchers often identify the properties that characterize individual concepts and assess whether different kinds of concepts contain different kinds of properties. Because properties are so central to the study of concepts, it is essential to develop theories of them and to assess these theories empirically. Rather than make implicit assumptions about properties that are expedient for the study of concepts, it is essential to understand properties in their own right. In this article, we assess an assumption about properties that many researchers have adopted implicitly, what we will call the *global form assumption.* We contrast this assumption with an alternative that relatively few researchers have considered, what we call the *local form assumption.*

The Global Form Assumption

Theories of knowledge typically use a single amodal symbol to represent a property across different categories. Thus, a single symbol for *mane* refers to the manes of *HORSE, PONY,* and *LION.*[1] Because this approach assumes that the same symbol represents a property across all relevant categories, we refer to it as the *global form assumption.* This assumption has clear advantages, including simplicity, generality, and context-independence. The same amodal symbol can represent many instances of the same property across many task contexts with no modification.

A wide variety of theories implicitly adopt the global form assumption. As Fig. 1A illustrates for feature lists, the same amodal feature represents *mane* in the feature lists for *HORSE, PONY,* and *LION* (e.g., Rosch & Mervis, 1975; Smith, Shoben, & Rips, 1974). Semantic networks and localist neural nets similarly adopt the global form assumption. As Fig. 1B illustrates, the same amodal node for *mane* is linked to the amodal nodes for *HORSE, PONY,* and *LION* (e.g., Collins & Loftus, 1975). Virtually all feature list, semantic network, and localist neural nets in the cognitive literature adopt the global form assumption implicitly.

Although the global form assumption is attractive computationally, not to mention widely accepted, to our knowledge it has no direct empirical sup-

[1] Throughout this article, we use uppercase italics for object concepts (*HORSE*), lowercase italics for property concepts (*mane*), and quotes for words (‘‘mane’’).

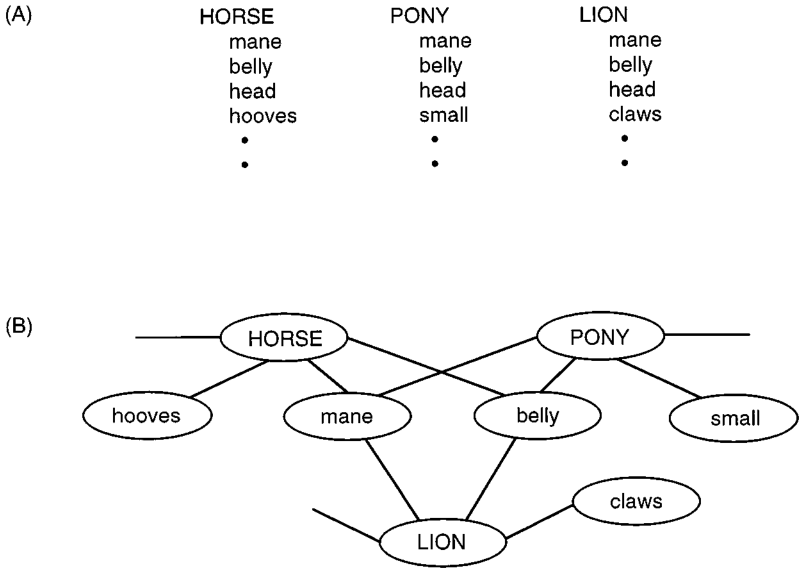

FIG. 1. Illustrations of the global form assumption in feature list (A) and semantic net (B) models of concepts. Each property is represented once in a single global manner across concepts.

port. Rather, it appears to have been adopted for purely theoretical reasons. Because of its simplicity, generality, and context-independence, the global form assumption lends itself to computational implementations that are tractable. Furthermore, it contributes to the view that cognition abstracts over the details of sensory-motor events, much like language.

The Local Form Assumption

Several sources of empirical evidence suggest caution in adopting the global form assumption. For example, Halff, Ortony, and Anderson (1976) found that people represented the property *red* differently in *HAIR, WINE, BRICK,* and *STOP SIGN.* Rather than using a single amodal symbol for *red* across concepts, people appeared to use different perceptual representations. Findings from Barsalou and Ross (1986), Medin and Shoben (1988), and Wisniewski (1998) further suggest that people represent the same property locally in different concepts rather than globally as a single symbol. Together, these findings intimate that the global form assumption may be more of an idealization than an accurate account of conceptual knowledge. Rather than a single symbol representing a property across all concepts, multiple symbols may represent its specific form in each, what we will call the *local form assumption.*

Perceptual theories of knowledge naturally adopt this assumption (e.g.,

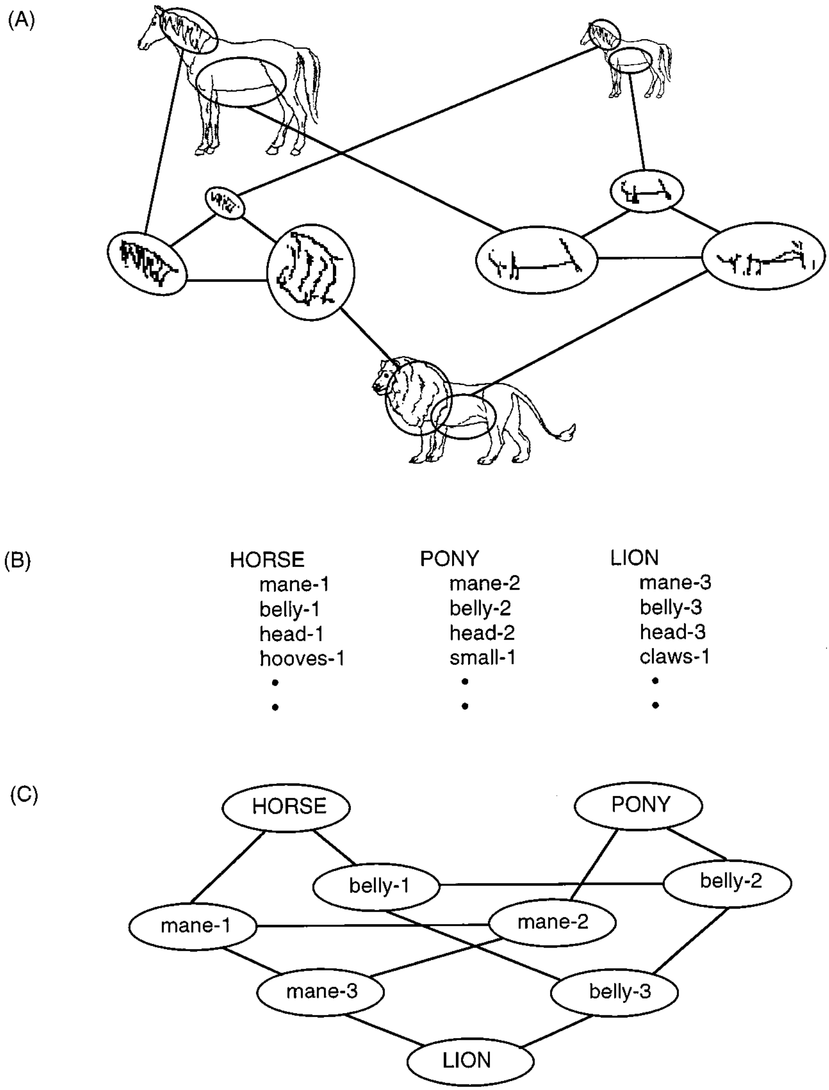

FIG. 2. Illustration of the local form assumption in perceptual symbol systems (A), feature lists (B), and semantic nets (C). Each property is represented multiple times in a local manner for individual concepts.

Barsalou, 1999). If perceptual simulations represent concepts, then the same property is likely to take different forms in different concepts. As Fig. 2A illustrates, a perceptual representation of *HORSE* represents *mane* one way, whereas a perceptual representation of *LION* represents *mane* in another. Because the topography of these two manes is so different—a horse's mane runs the length of its neck, whereas a lion's surrounds the circumference—they cannot be superimposed into a single perceptual representation that is identifiable. Instead, separate local representations are necessary. Many other properties also appear to require local representations when represented perceptually. For example, a single perceptual representation for *handle* would not be identifiable across *HAMMER, SUITCASE,* and *CAR DOOR* because each handle is so different. If *handle* is represented perceptually, it must be represented differently for each concept. The same goes for *leg, ear, seat, wing,* and most other properties. Unlike amodal symbol systems, a single perceptual symbol cannot be designated as referring to all of a property's different forms.

A critical issue for this view is explaining why different forms of a property are viewed as related during learning, such that a single name refers to them all. A related issue is how best to characterize the cognitive representation of a property that results from this learning. We do not address these issues here in detail. Generally, however, we assume that a family resemblance exists between the local forms of a property (Rosch & Mervis, 1975), such that perceiving one local form produces remindings of other local forms (Ross, Perkins, & Tenpenny, 1990). As a result of these remindings, a simulator for the property develops in memory (Barsalou, 1999) that implements a radial category (Lakoff, 1987; Malt et al., 1999).

Distributed approaches to knowledge provide another means of implementing the local form assumption. Rather than representing a property with a single static symbol, these systems use different collections of units to represent different forms of the same property (e.g., Hinton, McClelland, & Rumelhart, 1986; Metcalfe, 1982; Murdock, 1993; Smolenksy, 1988). Thus, different but correlated vectors represent *mane* in the concepts for *HORSE, PONY,* and *LION.*

Implementing the local form assumption in classic amodal theories. It is widely believed that classic amodal theories have sufficient expressive power to explain any finding (Anderson, 1978). In this spirit, Pylyshyn (1973, 1981) argued that amodal descriptions can represent the content of perceptual representations. Similarly, Fodor and Pylyshyn (1988) argued that distributed connectionism is simply an implementation of classic amodal theories. These arguments suggest that it should be possible to implement the local form assumption in amodal theories, and indeed it is. As Fig. 2B illustrates, the local form assumption can be implemented in feature lists. Rather than using a single amodal symbol to represent a property across concepts, different amodal features can represent it in different concepts. Figure 2C illustrates that this is also possible in semantic networks. To our knowledge, however, no theory of feature lists or semantic networks has ever implemented the local form assumption.

In this article, our focus is *not* on whether knowledge is represented in classic amodal theories, perceptual symbol systems, or distributed representational systems. Instead, we focus on the more general issue of whether properties are represented globally or locally, regardless of their representational format. Should our findings support the local form assumption, they would be consistent with any amodal, perceptual, or distributed view that adopts it. We explore the issue of representational format further under General Discussion.

Constrained versus Dominant Access

In the experiments to follow, subjects performed the property verification task. On each trial, subjects received the name of a concept (e.g., *PONY*) followed by the name of a property (e.g., *mane* or *wheels*) and then verified whether the property was true or false of the concept. As is shown, this task can be used to distinguish the global and local form assumptions.

If properties take local forms in concepts, it becomes essential to address how local forms are accessed during the property verification task. To derive predictions for the experiments that follow, it is necessary to explain how the correct local form is accessed during verification. When a subject verifies that a *LION* has a *mane*, for example, how is the correct form of *mane* accessed from all of the local forms available (e.g., *horse mane, pony mane*, and *lion mane*)? One possibility is *constrained access*. On this view, the concept being tested constrains access to the relevant form of the property. When verifying *mane* for *LION*, the concept *LION* constrains the access of *mane* so that only *lion mane* is accessed, not *horse mane* or *pony mane*.

An alternative possibility is *dominant access*. On this view, representations of the concept and property are retrieved independently, at least to a large extent. As a representation of the concept is retrieved, the most dominant form of the property is retrieved, even if that form is incorrect for the concept. Imagine that *horse mane* is the dominant form of *mane*. If dominant access occurs when *mane* is verified for *LION*, then *lion mane* will not be retrieved initially because it is not dominant. Instead, *horse mane*—even though incorrect—will be retrieved initially. After *horse mane* is suppressed, other senses of *mane* may be retrieved until *lion mane* is reached, eventually producing a true response. For a related view, see Edelman and Intrator (2000, 2001).

In previous work (Solomon & Barsalou, 2001), subjects' informal comments suggested that dominant access occurred during property verification. For example, when verifying *nose* for *AIRPLANE*, some subjects reported that they initially imagined a human nose that did not match any part of an airplane, often leading to an incorrect false response. If subjects did indeed simulate *human nose* for *nose*, then they represented *nose* locally, not globally. Furthermore, if they simulated *human nose* in the context of *AIRPLANE*, their retrieval of property information took the form of dominant access, not constrained access. The experiments to follow attempted to bring this phenomenon under laboratory control.

Flexible property dominance. Previous work indicates that the dominance ordering of the properties in a concept is highly flexible (e.g., Barsalou, 1982, 1987, 1989, 1993). Rather than being rigid, a given property's accessibility varies as a function of frequency, recency, and context. For example, properties processed recently become temporarily elevated in dominance, thereby speeding their subsequent processing. Imagine that a subject verifies *mane* for *HORSE* on one trial of an experiment. As a result of the processing it receives, *mane* becomes temporarily elevated in accessibility, speeding its subsequent processing on a later trial.

The distinction between global and local properties adds an important wrinkle to flexible property dominance. If properties are represented globally, then processing *mane* in any concept should increase the accessibility of the global symbol for *mane.* Because the same global symbol represents *mane* in all concepts, it should become more available after verifying it for any concept—*HORSE, PONY,* or *LION.* In contrast, if properties are represented locally, then verifying *mane* for a concept should only make the local form for *mane* in that concept more accessible. The accessibility of different local forms in other concepts should not increase. If *mane* is verified for *HORSE, horse mane* should increase in accessibility but not *lion mane.*

Scope of Facilitation

Assessing the scope of facilitation provides a means of distinguishing the global and local form assumptions empirically. Whereas the global form assumption predicts a wide scope of facilitation, the local form assumption predicts a narrow scope. To see this, imagine that a subject verifies *mane* for *LION* on one trial in an experiment and then 20 trials later verifies *mane* for *PONY.* The global form assumption predicts that verifying *mane* for *LION* should speed the verification of *mane* for *PONY.* Because a single global form represents *mane,* the same form should be verified for both concepts. If verifying *mane* for *LION* primes this global form, its increased accessibility should speed the later verification of *mane* for *PONY.* In contrast, the local form assumption predicts no such facilitation. Because *mane* is represented locally in *LION* and *PONY,* verifying *mane* for *LION* only facilitates the local form for *LION*—not the one for *PONY.* As a result, later verifying *mane* for *PONY* does not benefit from having verified *mane* for *LION* earlier. If properties are represented locally, facilitation should not occur. Thus, the global form assumption predicts a wide scope of facilitation, whereas the local form assumption predicts a narrow scope.

Analogical verification. Solomon and Barsalou (2001) reported a finding that has implications for the scope of facilitation. When different forms of a property occurred in alignable regions across concepts, the property was verified faster than when its forms occurred in nonalignable regions. For example, *steering wheel* tends to occur in alignable structural regions across concepts (e.g., *CAR, BOAT,* and *AIRPLANE*), whereas *propeller* tends to occur in nonalignable regions (e.g., *AIRPLANE, HELICOPTER,* and *BOAT*). Solomon and Barsalou (2001) found that the time to verify a property decreased as the alignability of its local forms increased. Even when many other variables were removed from the prediction of verification times, the alignability of a property's locations accounted significantly for unique regression variance. This finding suggests that people verify properties in one concept based on structural analogies with properties in other concepts. Much additional work has shown that people do not process properties independently of their locations but process them instead in structural configurations (e.g., Gentner & Markman, 1994; Goldstone, 1994; Wisniewski, 1997).

To see how analogical verification works, imagine that *horse mane* is the dominant form of *mane* and that subjects therefore access *horse mane* initially when verifying *mane* for *PONY*. Without retrieving *pony mane,* a true response can nevertheless follow because *horse mane* matches the analogous region of *PONY*. Because a known sense of *mane* (i.e., *horse mane*) is sufficiently similar to the analogous region of *PONY,* it is reasonable to infer that *mane* is true of *PONY.*

The forms of alignable properties are probably also important—not just their locations. Even when two forms of a property are in alignment, analogical verification may not occur if these forms differ significantly. Thus, verifying *wing* for *BUTTERFLY* may not facilitate verifying *wing* for *BEE*. Even though *butterfly wing* and *bee wing* reside in analogous locations, their forms may differ too much for one to serve as the basis for verifying the other.

Most importantly for our purposes here, analogical verification broadens the scope of facilitation for the local form view. Verifying a local form on an earlier trial facilitates verifying similar forms in different concepts later—not just the same form in the same concept. Thus, verifying *mane* for *HORSE* facilitates later verifying *mane* for *PONY*. The global form view similarly predicts facilitation but for a different reason, namely, a common global symbol becomes active in the first verification that later facilitates the second. These two views only make different predictions when local forms differ too much in location and/or form for analogical verification to occur. For example, verifying *LION-mane* initially may not support analogical verification in *PONY-mane* later because the two manes differ too much. In contrast, the global form view still predicts facilitation because the same global symbol underlies both verifications. The three experiments to follow assess these different scopes of facilitation for the local and global views.

Facilitation versus interference. Thus far we have assumed that changes in accessibility reflect facilitation. For example, we have assumed that verifying *mane* for *HORSE* facilitates a form of *mane* that speeds the later verification of this form for *PONY.* Alternatively, such effects could reflect interference. For example, verifying *mane* for *LION* could interfere with *pony mane,* thereby slowing its later verification. Experiment 3 assesses whether changes in accessibility reflect facilitation, interference, or both. Until then, we adopt the default assumption that such changes simply reflect facilitation.

By using the terms ‘‘facilitation’’ and ‘‘interference,’’ we remain agnostic as to the specific mechanisms that underlie changes in the accessibility of property forms. We do assume, however, that facilitation and interference result from *long-term* changes in storage—not short-term fluctuations in activation. Because the context effects in our experiments last at least 15 to 25 trials, we assume that they represent structural changes to memory, such as the storage of exemplars (Jacoby & Brooks, 1984), changes in connection strengths between concepts and properties (McClelland & Rumelhart, 1985), and so forth.

Overall concept similarity. The logic presented thus far contains a potential confound. Overall, *HORSE* is more similar to *PONY* than is *LION*, not just in their manes, but in many other properties as well. Thus, concept similarity—not property similarity—could be responsible for greater facilitation from *HORSE* to *PONY* than from *LION* to *PONY* when verifying *mane.*

To assess this possibility, a second property was verified for each set of three concepts used in these experiments. For the *LION-HORSE-PONY* set, the second property was *belly*. Unlike *mane*, *belly* takes roughly the same form in all three concepts (Fig. 2A), as we verified through independent scaling. The *belly* of *LION* is about as similar to the *belly* of *PONY* as is the *belly* of *HORSE*. Including such properties allowed us to assess whether concept similarity or property similarity underlies the differential facilitation of a property. If concept similarity is the critical factor, then verifying *belly* for *PONY* should benefit more from verifying *belly* for *HORSE* earlier than from verifying *belly* for *LION*. It should not matter what forms the property takes because *PONY* remains more similar to *HORSE* than to *LION*, regardless of the forms verified. In contrast, if the similarity of property forms is critical, verifying *belly* for *HORSE* or *LION* initially should not matter. In each case, a specific belly is facilitated that is highly similar to the *belly* for *PONY* and that therefore speeds its later verification.

EXPERIMENT 1

This first experiment contained four critical conditions: *same form*, *different form*, *similar concept*, and *dissimilar concept*. As Table 1 illustrates, each condition contained related pairs of property verification trials—a *context trial* followed by a *target trial*—separated by 10 to 25 filler trials. In the *same form condition*, subjects verified properties that took the same form on the context and target trials (e.g., *HORSE-mane* then *PONY-mane*). In the *different form condition*, subjects verified properties that took different forms on the context and target trials (e.g., *LION-mane* then *PONY-mane*). Each pair of trials in the different form condition always had the same target

TABLE 1
Examples of the Materials in Experiments 1 and 2

	Critical conditions		Control conditions	
Trial	Same form	Different form	Similar concept	Dissimilar concept
Context	HORSE mane	LION mane	HORSE belly	LION belly
Target	PONY mane	PONY mane	PONY belly	PONY belly
Context	KNIFE handle	SCISSORS handle	KNIFE blade	SCISSORS blade
Target	SWORD handle	SWORD handle	SWORD blade	SWORD blade
Context	EAGLE claw	CAT claw	EAGLE eye	CAT eye
Target	HAWK claw	HAWK claw	HAWK eye	HAWK eye
Context	SCHOOL roof	CAR roof	SCHOOL carpet	CAR carpet
Target	HOUSE roof	HOUSE roof	HOUSE carpet	HOUSE carpet
Context	SHEEP neck	GIRAFFE neck	SHEEP ear	GIRAFFE ear
Target	GOAT neck	GOAT neck	GOAT ear	GOAT ear
Context	WASP wing	BUTTERFLY wing	WASP body	BUTTERFLY body
Target	BEE wing	BEE wing	BEE body	BEE body
Context	BUS seat	BICYCLE seat	BUS tire	BICYCLE tire
Target	TRUCK seat	TRUCK seat	TRUCK tire	TRUCK tire

concept (*PONY*) and the same property (*mane*) as its matched pair in the same form condition—only the context concepts differed. Thus, the critical target trials were identical for the two conditions.[2]

[2] To make the same form and different form conditions as comparable as possible, the same form condition did not use the same concept on both the context and target trials (e.g., *PONY-mane*, *PONY-mane*). In the different form condition, it was impossible for the two concepts to ever be the same (i.e., two different forms could not occur for the same concept). Thus, both the same form and different form conditions used different concepts on the context and target trials. By always using two different concepts, and by capitalizing on analogical verification to produce facilitation in the same form condition, it was possible to isolate the effect of property form (using the similar and dissimilar concept conditions to control concept simi-

As noted above, concept similarity and property similarity are confounded in the same form and different form conditions. Not only is property similarity higher in the same form condition than in the different form condition, so is concept similarity. To assess whether concept similarity is a factor in property verification, the following experiments included two control conditions. In the *similar concept condition,* subjects verified properties that took the same form in two similar concepts (e.g., *HORSE-belly* then *PONY-belly*). In the *dissimilar concept condition,* subjects verified properties taking the same form in two dissimilar concepts (e.g., *LION-belly* then *PONY-belly*). Each pair in the dissimilar concept condition always had the same target concept (*PONY*) and the same property (*belly*) as its matched pair in the similar concept condition. Thus, the critical target trials were identical for these two conditions. Furthermore, each set of three concepts (e.g., *LION, HORSE,* and *PONY*) was used in the similar concept and dissimilar concept conditions and also in the same form and different form conditions, thereby controlling concept similarity across the four conditions. For example, the concepts *LION, HORSE,* and *PONY* were used for *mane* in the same form and different form conditions and also for *belly* in the similar concept and dissimilar concept conditions.

In a counterbalanced within-subject design, subjects received pairs of trials from all four conditions; however, they only received one pair from a given set of concepts and properties. Three times as many filler pairs as critical pairs obscured the critical materials.

Highly related false properties were used to preclude a superficial linguistic strategy and to ensure conceptual processing (Solomon & Barsalou, 2001). When unrelated false trials have been used in previous research (cf. Kosslyn, 1976), subjects appeared to use the associative strength between concept and property words to make decisions, bypassing conceptual knowledge. When concept and property words are always associated for true trials, subjects can respond ‘‘true’’ if a lexical association is present (e.g., *PONY-mane*). Conversely, when concept and property words are always unassociated for false trials, subjects can respond ‘‘false’’ if a lexical association is absent (e.g., *PONY-glass*). To measure conceptual knowledge, an experiment must ensure that subjects access conceptual knowledge and not rely on word associations. Solomon and Barsalou (2001) demonstrated that the use of highly associated false trials (e.g., *PONY-barn*) blocks the word association strategy and forces conceptual processing. When properties on both true and false trials are highly associated with their target concepts, subjects cannot use word associations as the basis of their decisions. For this reason, related false trials were used in all of the experiments to follow.

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larity was also essential to isolating this effect). Experiment 5 in Solomon (1997) included a condition in which identical concept-property pairs were verified on both the context and target trials. This condition is described under General Discussion for the identity benefit.

Predictions. Of primary interest was whether facilitation would occur only in the same form condition or also in the different form condition. Does verifying *mane* for *PONY* benefit only from verifying *mane* for *HORSE* earlier or does it also benefit from verifying *mane* for *LION?* If verifying a property on a target trial only benefits from verifying the same form on an earlier context trial, then both the local form and dominant access assumptions receive support. This pattern would not occur if properties were represented globally or if they exhibited constrained access. If a single global form represents a property, then different forms as well as the same form should facilitate it. If the target concept being processed constrains access to the relevant form of the property, it should not matter which form was verified previously because the target concept always activates its local form. In contrast, if only the same form produces facilitation, then properties must take different forms in different concepts and the most dominant form must be retrieved initially.

As noted above, however, observing facilitation only in the same form condition also supports the interpretation that concept similarity—not property similarity—is responsible. Comparing the similar and dissimilar concept conditions allows us to distinguish these two possibilities. If concept similarity is responsible, then facilitation should only occur in the similar concept condition because concept similarity is low in the dissimilar concept condition. However, if property similarity is responsible, then facilitation should occur in both the similar and dissimilar concept conditions because property similarity is high in each.

Method

Design and subjects. Sixteen sets of materials were developed, each containing three concepts and two properties. As Table 1 illustrates, four pairs of trials were drawn from each set: same form, different form, similar concept, and dissimilar concept. Each pair contained a context trial and a target trial. In a within-subject design, each subject received only one pair from each materials set but received four pairs from each condition across the 16 sets (i.e., same form, different form, similar concept, and dissimilar concept). Thus, four counterbalanced versions of the materials were developed that each contained four pairs in each condition, with each pair drawn from a different materials set. Orthogonally, the critical pairs in each version were presented in one of two random orders. Within each order, the two pairs from the same materials set occurred in the same absolute positions. The dependent measures were verification times and errors for the target trials of the critical target pairs.

Forty-eight students at the University of Chicago participated for pay, with six assigned randomly to each of the version (4) $\times$ order (2) sets of materials. All subjects were native speakers of English. Two additional subjects were excluded because of error rates exceeding 12% on the 16 critical trials (i.e., three or more errors).[3]

Materials. For each of the 16 critical materials sets, one critical property was selected that took roughly the same form in two of the concepts and a different form in the third (i.e., the property used in the same form and different form conditions). The other control property for

[3] A criterion of 12% was adopted because this was the maximum number of trials that we believed could be removed from a subject's data and not distort mean reaction times for correct trials.

the set took roughly the same form in all three concepts (i.e., the property used in the similar and dissimilar concept conditions). Both properties were always physical parts of the corresponding objects, not any other type of property. The critical properties and contrrol properties were comparable in average number of letters [4.5 vs 4.6; $F(1, 30) = .02$, $MSE = 1.80$, $p > .25$], horizontal visual angle [4.28° vs 4.34°; $F(1, 30) = .017$, $MSE = 1.61$, $p > .25$], and word frequency from Kučera and Francis (1967) [117 vs 134; $F(1, 30) = .04$, $MSE = 58{,}410$, $p > .25$].

Ratings from an independent group of 48 subjects confirmed our intuitions about property form. For each critical pair of concepts that shared a property, subjects rated the similarity of the property's forms in the two concepts on a scale from 1 (*not at all similar*) to 7 (*extremely similar*). On average, the similarity of the property forms was 6.0 in the same form condition versus 2.6 in the different form condition. In contrast, the mean similarity of the property forms was 6.4 in the similar concept condition versus 5.0 in the dissimilar concept condition. The properties for the same and different form pairs were significantly less similar than the properties for the similar and dissimilar concept pairs, as indicated by an interaction between property sets (same vs different form) and concept pairs (similar vs dissimilar) [$F(1, 47) = 29.98$, $MSE = 6.38$, $p < .001$]. Solomon (1997) presents the full set of critical materials.

The three concepts and two properties in each materials set were used to construct pairs of verification trials like those in Table 1, following the design described above. Across the 16 critical pairs of trials for a subject, the number of intervening trials between a context trial (e.g., *HORSE-mane*) and the corresponding target trial (e.g., *PONY-mane*) ranged from 10 to 25 with an average of 17.5. Across the four versions of the critical materials crossed with the two presentation orders, the average lag between context and target trials was the same across the four conditions, as were their average absolute positions in the presentation sequence. In the four versions sharing the same order, the two target trials for a materials set always occurred at the same position in the presentation sequence, with each of the four possible context trials occurring at the same earlier position.

Another 48 pairs of filler trials were constructed to implement false trials and to mask the structure of the 16 critical pairs. For each filler pair, the same property was verified on two trials for two different concepts, with none of these properties or concepts being the same as a concept or property in the critical pairs. For 16 of the filler pairs, the first trial was true and the second false (e.g., *KANGAROO-pouch* and *ELEPHANT-pouch*). For 16 other filler pairs, the first trial was false and the second true (e.g., *SINK-drawer* and *DRESSER-drawer*). For the final 16 filler pairs, both trials were false (e.g., *BEAVER-quills* and *RACCOON-quills*). Thus, the probability of a true response was .5 for both the first and the second presentations of a pair. The properties on false trials were always parts of taxonomically related categories (e.g., *CAR-propeller* and *OWL-bill*), thereby ensuring that subjects processed the true materials conceptually (Solomon & Barsalou, 2001).

A single order of the filler trials was used across the four versions and the two orderings of the critical materials. To preclude guessing strategies, the four different pairings of responses (true–true, true–false, false–true, and false–false) were equally distributed across absolute positions of the list, with an average lag comparable to the critical materials. Solomon (1997) presents the full set of filler materials.

Forty-eight practice trials were constructed that had a similar structure to the critical and filler materials but that shared no concepts or properties with them. This number of practice trials ensured that subjects were familiar with the task and had reached a stable speed by the critical trials.

Procedure. Macintosh IIci computers running PsyScope controlled all aspects of the experiment, and CMU button boxes recorded subjects' responses with millisecond accuracy. Prior to each trial, subjects rested their forefingers on the two response buttons, placed a foot on a pedal, and focused their attention on two vertically aligned asterisks 2.5 cm apart in the center of the screen. When ready to initiate the trial, subjects pressed the foot pedal and the asterisks disappeared. A concept word in capital letters appeared immediately where the upper asterisk had been and stayed on the screen for 500 ms, followed by a blank 1200 ms interstimulus interval. A property word in lowercase letters then appeared where the lower asterisk had been and remained on the screen until a response occurred. Thus, the SOA between the concept and property words was 1700 ms. Verification time was measured from the onset of the property word to the detection of a response. Following a subject's response, a 1-s delay intervened before the asterisks reappeared, signaling subjects to initiate the next trial when ready.

Subjects were instructed to respond true only if a property was a ''physical part'' of the concept object; otherwise, they were to respond false. Subjects used their dominant hand to make true responses and were instructed to respond as quickly as possible but to avoid making errors. When subjects erred, the words ''Incorrect Response'' appeared on the screen. Nothing was said to subjects about using imagery or any other perceptual strategy. Solomon (1997) presents the complete instructions.

To ensure optimal performance on the critical trials, subjects were led to believe that only the first 28 practice trials were practice and that all remaining trials were critical, including the last 20 practice trials. The 48 practice trials continued seamlessly into the 128 critical trials. Subjects received eight short breaks distributed evenly over the 176 total trials, one every 22 trials. The experiment lasted about 30 min.

Results

To trim spurious outliers on the critical trials, we examined the distribution of verification times for all critical trials combined across subjects, found an upper tail, and set the criterion at the tail's lower bound (i.e., 2600 ms). Our rationale for eliminating verification times further out in the tail was that they were likely to represent spurious processes other than the processes of interest. Eliminating verification times greater than 2600 ms led to a .5% reduction in the data, distributed evenly across conditions. Verification times for errors on the critical target trials were also removed.

Error rates were analyzed in two ways. First, they were analyzed as percentages and then as transformed arcsins (Winer, 1971). Because all three experiments found the same results for both analyses, we only report the analyses on percentages, so that the means and variances are transparent. Data for the context and filler trials are not reported, given that they do not bear on the hypotheses of interest. Separate ANOVAs on subjects and items were performed on the critical trials to ensure generalizability across both populations. In the analyses to follow, F_S refers to a test from a subjects ANOVA and F_I refers to a test from an items ANOVA. In nearly all inferential tests, we use conservative two-tailed tests, even though the hypotheses tested always made directional predictions.

Planned comparisons. Because a factorial structure did not underlie the hypotheses nor the designs of the experiments, tests of main effects and residuals (i.e., interactions) were not appropriate. Instead, the a priori predictions concerned differences between means such that planned comparisons between means were the appropriate tests. Table 2 presents the planned comparisons used to test the critical hypotheses. The first tested the hypothesis that properties are represented locally and exhibit dominant access. If this hypothesis is correct, then verification should be faster when the two forms of a property remain constant across the context and target trials than when

TABLE 2
The Hypotheses in Experiments 1 and 2 and the Planned Comparisons Used to Assess Them

Hypothesis	Critical conditions		Control conditions	
	Same form	Different form	Similar concept	Dissimilar concept
Local Form + Dominant Access	−1	3	−1	−1
Concept Similarity	−1	1	−1	1
Global Form	Null Hypothesis (no differences between means)			
Local Form + Constrained Access	Null Hypothesis (no differences between means)			

they differ. Specifically, verification should be faster in the same form, similar concept, and dissimilar concept conditions when property forms remain constant than in the different form condition when they do not. Weights of −1 for the first three conditions and 3 for the fourth operationalize this predicted pattern of results.

The second planned comparison in Table 2 tested the hypothesis that overall concept similarity determines verification time. If this hypothesis is correct, then verification should be faster when the overall similarity between concepts is high than when it is low. Property similarity should not matter. Thus, verification should be faster in the same form and similar concept conditions than in the different form and dissimilar concept conditions, as reflected in weights of −1 for the first two conditions and 1 for the other two.

The two planned comparisons in Table 2 are not orthogonal. Because they test a priori hypotheses, however, and are few in number, testing both is allowable (Keppel, 1973; Winer, 1971). To compensate for their dependence, we apply the Bonferroni correction, which increases their conservativeness by requiring stricter p values for significance. We also report the percentage of the explainable variance that each comparison captures. To the extent that one comparison captures most of the explainable variance and the other does not, their dependency is not a problem. The potency of the first comparison cannot be explained by its dependency on the second.

The null hypothesis in Table 2 tested the global form assumption. If properties are represented globally, the means for the four conditions should not differ. Because the earlier presentation of a property should speed its later processing regardless of its perceptual form, processing should be just as fast in the different form condition as in the other three conditions, where property form is the same. The null hypothesis also tested whether properties are represented locally and exhibit constrained access. If this hypothesis is correct, the four means should again not differ. Because each concept activates its own form of a property, the time to verify a property should be unaffected by the verification of the property's forms in earlier concepts.

Underlying all of these planned comparisons is the assumption that properties in the same form and different form conditions are comparable to properties in the similar concept and dissimilar concept conditions. As described under ''Methods'' for Experiments 1 and 2, these two sets of properties were comparable in length, visual angle, and word frequency. As described later for Experiment 2, those two sets did not differ in average verification time when verified in an isolated control condition. Thus, it is appropriate to treat these two property sets as equivalent in the planned comparisons. Because Experiment 3 used a factorial design, the planned comparisons in Table 2 were not used, and this issue was irrelevant.

Verification times. As Table 3 illustrates, the verification times confirmed the hypothesis that properties are represented locally and exhibit dominant access. The planned comparison in Table 2 that tests this hypothesis was significant for subjects and marginally significant for items [$F_S(1, 132) = 6.02$, $MSE = 18{,}970$, $p < .05$; $F_I(1, 48) = 3.03$, $MSE = 14{,}097$, $p < .10$; both Bonferroni corrected]. In the critical conditions, properties taking the same form were verified 44 ms faster than properties taking different forms. This difference did not reflect concept similarity, given that the planned comparison for this hypothesis was not significant [$F_S(1, 132) = .18$, $MSE = 18{,}970$, $p > .25$; $F_I(1, 48) = 0.02$, $MSE = 14{,}097$, $p > .25$; both Bonferroni corrected]. In the control conditions, the similar concept condition was actually 28 ms slower than the dissimilar concept condition.

The percentage of the explainable variance captured by the two comparisons further supports the hypothesis that properties are represented locally and exhibit dominant access. The planned comparison for this hypothesis captured 89% of the explainable variance, whereas the planned comparison for concept similarity only captured 15%. This pattern indicates that the large amount of variance explained by the first comparison does not reflect its dependency on the second.

TABLE 3
Average Verification Times and Error Proportions for the Critical Target Trials in Experiments 1 and 2

	Critical conditions		Control conditions	
Experiment/measure	Same form	Different form	Similar concept	Dissimilar concept
Experiment 1				
Verification times	753	797	749	721
Errors	.02	.05	.04	.02
Experiment 2				
Verification times	867	1004	827	799
Errors	.04	.05	.02	.02

Finally, the significance of the first planned comparison rejects the null hypothesis, thereby disconfirming the third and fourth hypotheses in Table 2. Properties do not appear to be represented globally, nor do they appear to be represented locally with constrained access.

Errors. Because the errors tended to covary positively with the verification times, subjects did not trade off speed for accuracy. Instead, subjects tended to make more errors as their verification times increased, indicating that both measures assessed task difficulty.

As Table 3 illustrates, the errors further suggest that properties are represented locally and exhibit dominant access. Although the planned comparison for this hypothesis approached significance for subjects, it did not reach significance for items [$F_S(1, 132) = 4.48$, $MSE = 0.006$, $p < .10$; $F_I(1, 48) = 1.31$, $MSE = 0.005$, $p > .10$; both Bonferroni corrected]. In the critical conditions, properties taking the same form produced .03 fewer errors than properties taking different forms, indicating a slight benefit of having verified the same property form earlier. As is shown below, this benefit appears consistently across experiments. The slight benefit here did not result from concept similarity, given that the planned comparison for this hypothesis was not significant [$F_S(1, 132) = 0.20$, $MSE = 0.006$, $p > .25$; $F_I(1, 48) = 0.08$, $MSE = 0.005$, $p > .25$; both Bonferroni corrected]. Finally, the marginal significance of the first planned comparison is inconsistent with the null hypothesis, further disconfirming global form representation and also local form representation plus constrained access.

Discussion

Verifying a property on a target trial was faster after verifying a similar form of the property earlier than after verifying a different form. This benefit occurred in all three conditions that held the forms of properties constant across the context and target trials (i.e., in the same form, the similar concept, and dissimilar concept conditions). Concept similarity was not responsible, given that it had no effect when property similarity was held constant in the two control conditions.

This pattern of results supports the hypothesis that properties are represented locally and exhibit dominant access. Because dissimilar forms of properties did not produce the same benefit as similar forms, properties are not represented globally, nor are local forms accessed in a constrained manner. Instead, verifying a local form on a context trial increases the form's dominance such that verifying a similar form later on the target trial benefits.

EXPERIMENT 2

To ensure that subjects had sufficient time to construct detailed representations of the target concepts prior to presentation of the critical properties, Experiment 1 used an SOA of 1700 ms. During this relatively long duration, however, subjects could have performed strategic processing that compromises our interpretation of the results. On seeing a concept, subjects could have tried to guess what property would be tested based on memories of earlier trials. Although we controlled the materials carefully and attempted to mask their critical structure, subjects may have nevertheless developed hypotheses that they implemented in guessing strategies. If so, then this complicates the interpretation of Experiment 1.

To eliminate this possibility, Experiment 2 used an SOA of 250 ms. Much research has shown that subjects can initiate little if any strategic processing within the first 300 ms of perceiving a stimulus (e.g., Neely, 1977; Posner & Snyder, 1975). Thus, when subjects in Experiment 2 received a property 250 ms after receiving the preceding concept, they should not have had time to implement a guessing strategy.

Besides reducing the SOA to 250 ms, Experiment 2 also differed from Experiment 1 in the critical materials. To ensure that specific concept-property sets were not responsible for the effects in Experiment 1, we replaced the majority of these sets with new ones. Thus, Experiment 2 provides an opportunity to replicate Experiment 1 using new materials and a more conservative SOA.

Method

Design and subjects. The design was identical to that of Experiment 1. Twenty-four students at the University of Chicago participated for pay, with three assigned to each of the version (4) × order (2) sets of materials. All subjects were native English speakers, none had participated in Experiment 1, and none were excluded for having high error rates on the 16 critical target trials (3 or more errors).

Materials. The materials were identical to those in Experiment 1 except that 10 of the original 16 critical sets were replaced to ensure robustness of the critical effects across materials. The critical properties and control properties were again comparable in number of letters [4.4 vs 4.6; $F(1, 30) = .16$, $MSE = 1.79$, $p > .25$], horizontal visual angle [4.22° vs 4.40°; $F(1, 30) = .156$, $MSE = 1.61$, $p > .25$], and word frequency [137 vs 73; $F(1, 30) = 1.00$, $MSE = 33{,}004$, $p > .25$]. Most importantly, Solomon (1997, Experiment 5) found that these two property sets did not differ in verification time when no related context trials preceded them [835 vs 850 ms; $F_S(1, 68) = .18$, $MSE = 15{,}324$, $p > .25$]. Thus, these two sets are equivalent on the key dependent measure that will be examined later. The final materials included all of the sets in Table 1. Some of the filler and practice items were also replaced to ensure no overlap with the newly introduced items. Solomon (1997) presents the full set of critical and filler materials.

As in Experiment 1, ratings from an independent group of 48 new subjects confirmed our intuitions about property form. On a 7-point rating scale, the mean similarity of the property forms in the same form condition was 6.2 versus 2.9 in the different form condition. In contrast, the mean similarity of the property forms in the similar concept condition was 6.4 versus 3.9 in the dissimilar concept condition. As in Experiment 1, the properties for the same and different form pairs were significantly less similar than the properties for the similar and dissimilar concept pairs, as indicated by an interaction between property sets (critical vs control form) and concept pairs (similar vs dissimilar) [$F(1, 47) = 5.30$, $MSE = 4.81$, $p < .025$].

Procedure. The procedure was identical to Experiment 1 except for the reduction in SOA from 1700 ms to 250 ms. On each trial, the concept was presented for 150 ms, followed by the property 100 ms later.

Results

Outliers were identified using the method described under Experiment 1, resulting in a cutoff point of 3000 ms. As in Experiment 1, this cutoff led to a .5% reduction in the data, distributed evenly across conditions. Verification times for errors were also removed. Again, the planned comparisons in Table 2 were tested, and analyses were performed on both subjects and items.

Verification times. As Table 3 illustrates, the verification times again confirmed the hypothesis that properties are represented locally and exhibit dominant access. The planned comparison in Table 2 that tests this hypothesis was significant for both subjects and items [$F_S(1, 60) = 22.52$, $MSE = 23{,}825$, $p < .001$; $F_I(1, 48) = 28.73$, $MSE = 12{,}261$, $p < .001$; both Bonferroni corrected]. In the critical conditions, properties taking the same form were verified 137 ms faster than properties taking different forms (a 211% increase over the 44 ms effect in Experiment 1).

This advantage for the same form condition did not reflect concept similarity, given that the similar concept condition was 28 ms slower than the dissimilar concept condition. Furthermore, the planned comparison for concept similarity hypothesis was not significant [$F_S(1, 60) = 2.94$, $MSE = 23{,}825$, $p < .10$; $F_I(1, 48) = 3.63$, $MSE = 12{,}261$, $p > .10$; both Bonferroni corrected]. The planned comparison for property similarity captured 95% of the explainable variance, whereas the planned comparison for concept similarity only captured 35%. Again, this pattern indicates that the importance of the first comparison does not reflect its dependency on the second.

Finally, the null hypothesis was also rejected, thereby disconfirming the third and fourth hypotheses in Table 2. Properties do not appear to be represented globally, nor do they appear to be represented locally with constrained access.

Errors. Because the errors again tended to covary positively with the verification times, subjects did not trade off speed for accuracy. Instead, subjects tended to make more errors as their verification times increased, indicating that both measures assessed task difficulty.

As Table 3 illustrates, the errors again suggest that properties are represented locally and exhibit dominant access. Although the planned comparison for this hypothesis was not significant [$F_S(1, 60) = 1.40$, $MSE = 0.007$, $p > .25$; $F_I(1, 48) = 1.09$, $MSE = 0.006$, $p > .25$; both Bonferroni corrected], errors occurred slightly more often in the different form condition than in the other conditions, a pattern that recurs consistently across experiments. The planned comparison for the concept similarity hypothesis was not significant, again indicating that concept similarity was not an important factor [$F_S(1, 60) = 0.09$, $MSE = 0.007$, $p > .25$; $F_I(1, 48) = 0.07$, $MSE = 0.006$, $p > .25$, both Bonferroni corrected].

Discussion

The results of Experiment 2 replicate those of Experiment 1 under a faster SOA of 250 ms and with different materials. SOA does not appear to be critical, given that the same results hold for SOAs of 250 and 1700 ms. Furthermore, guessing strategies do not appear important, given that these results occur at an SOA of 250 ms that precludes guessing. Instead, the accessibility of local property forms appears responsible for these findings. Verifying a local property form increases its accessibility such that verifying a similar form later proceeds more rapidly. This benefit occurred in every condition that held the forms of properties constant across context and target trials (i.e., in the same form, similar concept, and dissimilar concept conditions). No difference between the similar and dissimilar concept conditions again indicates that concept similarity was not important.

EXPERIMENT 3

Experiment 3 served two purposes. First, it assessed whether the advantage of the same form condition over the different form condition in the previous experiments reflected facilitation and/or interference. Second, Experiment 3 assessed an uncontrolled factor in the previous experiments: the chronic dominance of property forms. We address each in turn.

Facilitation versus Inhibition

The advantage of the same form condition over the different form condition could reflect facilitation, interference, or both. Verifying a property form on a context trial could facilitate the same form on later target trials, and/or it could interfere with different forms. To establish facilitation and/or interference, verification times in the same form and different form conditions must be compared to a baseline condition.

Selecting a baseline is no easy matter. Many different baselines are possible, and many factors are important in selecting a satisfactory one (Jonides & Mack, 1984). After assessing several possible baselines for the property verification task, Solomon (1997, Experiment 5) concluded that unrelated context trials enable an uncontaminated assessment of baseline performance (e.g., using *STOVE-burner* as a context trial for *PONY-mane*). By comparing target trials in the same form and different form conditions to target trials in this baseline condition, it was possible to measure facilitation and interference. If target trials are faster after same form context trials than after baseline context trials, then same form context trials produce facilitation (e.g., *PONY-mane* is faster after *HORSE-mane* than after *STOVE-burner*). Conversely, if target trials are slower after different form context trials than after baseline context trials, then different form context trials produce interference (e.g., *PONY-mane* is slower after *LION-mane* than after *STOVE-burner*).

A potential problem is that properties do not repeat across context and target trials in the baseline condition (e.g., STOVE-*burner*, then PONY-*mane*), whereas they do in the same form condition (e.g., HORSE-*mane*, then PONY-*mane*) and in the different form condition (e.g., *LION-mane*, then PONY-*mane*). If repeating a property speeds its processing, then a bias to detect facilitation but not interference results. To assess whether such bias exists, Solomon (1997) included three other baseline conditions to assess the effect of property repetition and found that it did not speed verification. For this and other reasons discussed in Solomon (1997), the baseline used here is appropriate for assessing facilitation and inhibition.

Chronic Dominance

The second purpose of Experiment 3 was to address the chronic dominance of local property forms. In Experiments 1 and 2, context trials increased the temporary dominance of local forms, while allowing their chronic dominance to vary uncontrolled. To appreciate this issue, consider the properties of *roof* and *wing*. On hearing ''roof,'' the local form that often comes to mind first is *house roof,* not *car roof;* on hearing ''wing,'' the local form that often comes to mind first is *bird wing,* not *bee wing.* In terms of chronic dominance, *house roof* and *bird wing* are dominant forms, whereas *car roof* and *bee wing* are weak forms. As Experiments 1 and 2 demonstrated, verifying a local form on a context trial increases its temporary dominance. What remains unresolved, though, is whether these changes in temporary dominance interact with chronic dominance. Does temporarily increasing a local form's accessibility on a context trial have the same effect later on verifying chronically dominant and chronically weak forms? Because chronic dominance was not controlled in Experiments 1 and 2, this issue remains unresolved.

Perhaps facilitation only occurred for property forms that were low in chronic dominance but not for ones that were high. For example, verifying *wing* for *BEE* may have benefited from verifying *wing* for *WASP* earlier, whereas verifying *roof* for *HOUSE* may not have benefited from verifying *roof* for *SCHOOL.* Because weak property forms reside at a low level of accessibility, they may possess much potential for facilitation. Conversely, because dominant forms reside at a high level of accessibility, they may possess little potential. If dominant property forms reside at a moderate level of accessibility, however, or if they are subject to competition from other property forms, they too may exhibit facilitation.

In a within-subjects design, Experiment 3 factorially crossed the chronic dominance of target properties (dominant vs weak) with the type of context trial (same form vs different form vs baseline). Thus, Experiment 3 assessed the interaction between temporary and chronic dominance, as well as facilitation and interference. Because Experiments 1 and 2 observed no effect of concept similarity, this factor was not manipulated here.

Method

Design. Thirty-six sets of critical materials were developed, each containing three concepts and one property. Eighteen sets assessed a chronically dominant property form on the target trials, and the other 18 sets assessed a chronically weak property form. Table 4 provides

TABLE 4
Examples of the Materials in Experiment 3

Target form/trial	Same form condition	Different form condition	Baseline condition
Dominant property forms			
Context	BARN roof	CAR roof	BUS transmission
Target	HOUSE roof	HOUSE roof	HOUSE roof
Context	TIGER whiskers	MAN whiskers	ZOO cage
Target	CAT whiskers	CAT whiskers	CAT whiskers
Context	WOLF tail	PEACOCK tail	MUSEUM display
Target	DOG tail	DOG tail	DOG tail
Weak property forms			
Context	BUTTERFLY wing	BIRD wing	THEATER stage
Target	MOTH wing	MOTH wing	MOTH wing
Context	APPLE skin	HUMAN skin	IGLOO ice
Target	PEAR skin	PEAR skin	PEAR skin
Context	ROBIN breast	WOMAN breast	SHIP deck
Target	SPARROW breast	SPARROW breast	SPARROW breast

examples of both types of critical sets. As Table 4 further illustrates, three pairs of trials were drawn from each set: same form, different form, and baseline.

In a counterbalanced within-subject design, each subject received only one pair from each critical set, 18 from the dominant form materials (6 same form, 6 different form, and 6 baseline) and 18 from the weak form materials (6 same form, 6 different form, and 6 baseline). Three counterbalanced versions of the materials were developed that each contained 18 pairs from the dominant form sets (6 of each type) and 18 pairs from the weak form sets (6 of each type). Across versions, a same form pair, a different form pair, and a baseline pair from a given set each occurred in one version. Orthogonally, the critical pairs in each version were presented in one of two random orders. For each order, the context and target pairs from the same materials set always occurred in the same absolute positions, with the context pair varying across versions. The dependent measures were verification times and errors for the target trials of the critical pairs.

Subjects. Fifty-four students at the University of Chicago participated for pay, with nine assigned to each of the version (3) $\times$ order (2) sets of materials. All subjects were native English speakers, and none had participated in Experiment 1 or 2.

As will be shown, subjects made many more errors than in the previous experiments. As will also be shown, most of these errors occurred on the weak property forms (a 20% error rate), suggesting that Experiments 1 and 2 primarily used dominant forms that were relatively easy to verify. Indeed, the error rates for the dominant forms here (.04) were quite similar to the error rates in the previous experiments (.03). Because the error rates for dominant forms seemed like a good indicator of whether subjects made excessive errors, a new criterion for excluding subjects was established: If a subject's error rate exceeded 11% on the 18 dominant form trials (three or more errors), he or she was excluded from the experiment. Under this criterion, 10 additional subjects were excluded.

Materials. For each of the 36 critical materials sets, a property was selected that took roughly the same form in two of the concepts and a different form in the third. All properties were physical parts of the corresponding objects, not any other type of property. As described above, 18 properties took dominant forms and 18 took weak forms. The words for the dominant and weak forms were comparable in number of letters (5.7 vs 5.2; $F(1, 34) = 1.05$, $MSE = 2.14$, $p > .25$], horizontal visual angle [5.44° vs 4.97°; $F(1, 34) = 1.05$, $MSE = 1.91$, $p > .25$], and word frequency [23 vs 29; $F(1, 34) = .40$, $MSE = 1{,}019$, $p > .25$].

To confirm our intuitions about the relative dominance of property forms, all critical properties were normed for dominance. Sixteen new subjects received the 36 critical properties sequentially and, for each, generated three objects that possessed it. The dominance of a particular property form was the percentage of subjects who generated an object possessing the form in at least one of their three generated objects. Because dominance was defined as the percentage of subjects who generated a particular form at least once in three attempts, dominance for individual property forms does not sum to 1. If every subject generated *HOUSE*, *TEPEE*, and *CAR* for *roof*, the different form of *roof* in each concept would receive a dominance scaling of 100%, with the three forms summing to 300%.[4]

To see how the measure worked, consider the dominant and weak forms used for *roof*. The dominant form was common to *house*, *building*, *dog house*, *garage*, *hut*, *barn*, *shed*, and *outhouse*. The scaled dominance of this form was the percentage of subjects who generated one or more of these objects in their protocol (62%). Analogously, *car*, *truck*, and *vehicle* were grouped to establish the weak form of *roof*, with 18% of the subjects producing it. The 18 dominant forms used for the critical materials had an average dominance of 57%, whereas the 18 weak forms had an average dominance of 20% [$F_I(1, 34) = 69.53$, $MSE = 0.018$, $p < .001$]. Thus, the dominance manipulation was potent, with dominant forms being generated nearly three times as often as weak forms.

The three concepts and one property in each materials set were used to construct pairs of verification trials like those in Table 4, following the design described earlier. Across the 36 critical pairs of trials for a subject, the number of intervening trials between a context trial (e.g., *BARN-roof*) and the corresponding target trial (e.g., *HOUSE-roof*) ranged from 10 to 25 with an average of 17.5. Across the three versions of the critical materials crossed with the two presentation orders, the average lag between context and target trials was the same for the same form, different form, and baseline conditions, as was their average absolute position in the presentation sequence. For each set of three versions sharing the same order, the target trial for a materials set always occurred at the same position in the presentation sequence, with each of the three possible context trials occurring at the same earlier position. Solomon (1997) presents the full set of critical materials.

Another 108 pairs of filler trials were constructed to implement false trials and to mask the structure of the 36 critical pairs. These filler pairs were constructed in the same manner as

[4] In another analysis, we attempted to develop a dominance measure that summed to 1. In that analysis, the measure was the percentage of subjects who produced a given property form in the *first* object generated. Problematically, however, an insufficient number of weak forms was produced for the first object. Because a sufficient number of weak forms was produced across three generated objects, this measure was used instead.

the filler pairs for Experiments 1 and 2. For 36 filler materials, the first trial was true and the second false (e.g., *KANGAROO-pouch* and *ELEPHANT-pouch*); for another 36 filler materials, the first trial was false and the second true (e.g., *SKIRT-collar* and *BLOUSE-collar*); for the final 36 filler materials, both trials were false (e.g., *STARFISH-eyelash* and *QUAIL-eyelash*).

As in the previous experiments, none of the concepts or properties in the filler pairs were the same as a concept or property in the critical pairs; the probability of a true response was .5 for the first and the second presentation of a pair; and the properties on false trials were always parts from taxonomically related categories, thereby ensuring that subjects processed the true items conceptually. Also as in the previous experiments, a single order of the filler trials was used across the three versions and the two orderings of the critical materials; the four different pairings of responses (true-true, true-false, false-true, and false-false) were equally distributed across absolute positions of the list; and the lag between context and target trials for the fillers ranged from 10 to 25 with an average of 17.5. Solomon (1997) presents the full set of filler materials.

Twenty-six practice trials were constructed that had a similar structure to the critical and filler materials but that shared no concepts or properties with them.

Procedure. The procedure from Experiment 2 (with a 250-ms SOA) was used except that subjects performed 26 practice trials and 288 critical trials. To ensure optimal performance on the critical trials, subjects were led to believe that only the first 16 practice trials were practice and that all remaining trials were critical. Subjects received 10 breaks distributed evenly over the 314 total trials, about 1 every 31 trials. The experiment lasted around 45 min.

Results

Outliers were identified using the method described under Experiment 1, resulting in a cutoff point of 3000 ms. As in Experiments 1 and 2, this cutoff led to a .5% reduction in the data, distributed evenly across conditions. Verification times for errors were also removed. Again, analyses were performed on subjects and items.

Verification times. As Table 5 illustrates, Experiment 3 replicated the basic finding of Experiments 1 and 2: Properties in the same form condition were verified 69 ms faster than properties in the different form condition [$F_S(1, 102) = 13.48$, $MSE = 19{,}079$, $p < .01$; $F_I(1, 90) = 6.73$, $MSE = 16{,}693$,

TABLE 5
Average Verification Times and Error Proportions for the Critical Target Trials and the Corresponding Baseline Trials in Experiment 3

Measure/form	Same form condition	Different form condition	Baseline condition
Verification times			
Dominant forms	722	780	775
ΔBaseline	+53	−5	
Weak forms	866	946	948
ΔBaseline	+82	+2	
Errors			
Dominant forms	.03	.06	.03
ΔBaseline	0	−.03	
Weak forms	.11	.27	.22
ΔBaseline	+.11	−.05	

$p < .05$]. As in Experiments 1 and 2 this finding supports the hypothesis that properties are represented locally and exhibit dominant access.

Table 5 further illustrates that subjects verified dominant property forms 155 ms faster than weak property forms in the same and different form conditions [$F_S(1, 102) = 62.41$, $MSE = 20{,}787$, $p < .001$; $F_I(1, 90) = 33.02$, $MSE = 16{,}693$, $p < .001$].[5] Although dominant and weak forms differed considerably in accessibility, they nevertheless exhibited roughly the same benefit from processing the same property form earlier. For dominant forms, receiving the same property form earlier speeded processing by 58 ms relative to receiving a different form [$F_S(1, 102) = 4.37$, $MSE = 20{,}787$, $p < .05$; $F_I(1, 90) = 1.75$, $MSE = 16{,}693$, $p > .10$]. For weak forms, receiving the same property form earlier speeded processing by 80 ms [$F_S(1, 102) = 8.31$, $MSE = 20{,}787$, $p < .01$; $F_I(1, 90) = 5.50$, $MSE = 16{,}693$, $p < .05$]. The trend for weak forms to show a larger difference between same and different forms (80 ms) than dominant forms (58 ms) failed to reach significance [$F_S(1, 102) = 0.31$, $MSE = 20{,}787$, $p > .25$; $F_I(1, 90) = 0.52$, $MSE = 16{,}693$, $p > .25$].

Finally, Table 5 demonstrates evidence of facilitation for verification times. For dominant forms, the same form condition was 53 ms faster than the baseline. This difference did not reach significance by the conservative two-tailed *F* tests that we have been using throughout this article [$F_S(1, 102) = 3.65$, $MSE = 20{,}787$, $p < .10$; $F_I(1, 90) = 1.35$, $MSE = 16{,}693$, $p > .10$]; however, this difference was significant on an a priori one-tailed test predicting that the same form condition is faster than the baseline [$t_s(102) = 1.91$, $p < .05$]. The different form condition did not differ from the baseline, indicating no interference [$F_S(1, 102) = 0.03$, $MSE = 20{,}787$, $p > .25$; $F_I(1, 90) = 0.03$, $MSE = 16{,}693$, $p > .25$]. Similarly for weak forms, the same form condition was 82 ms faster than the baseline, indicating facilitation [$F_S(1, 102) = 8.73$, $MSE = 20{,}787$, $p < .01$; $F_I(1, 90) = 6.29$, $MSE = 16{,}693$, $p < .05$]. The different form condition did not differ from the baseline, indicating no interference [$F_S(1, 102) = 0.01$, $MSE = 20{,}787$, $p > .25$; $F_I(1, 90) = 0.02$, $MSE = 16{,}693$, $p > .25$]. The trend for weak forms to produce more facilitation (82 ms) than dominant forms (53 ms) failed to reach significance [$F_S(1, 102) = 0.47$, $MSE = 20{,}787$, $p > .25$; $F_I(1, 90) = 0.52$, $MSE = 16{,}693$, $p > .25$]. This lack of an interaction further supports the conclusion that dominant forms in the same form condition exhibited facilitation.

Errors. As Table 5 illustrates, Experiment 3 replicated the trend in Experiments 1 and 2 for the different form condition to exhibit higher error rates than the same form condition (.17 vs .07), this time significantly [$F_S(1, 102) = 31.24$, $MSE = 0.014$, $p < .001$; $F_I(1, 90) = 6.34$, $MSE = 0.023$,

[5] As described above, the dominant and weak properties did not differ in number of letters, visual angle, or word frequency. Thus, these factors do not explain this effect of chronic dominance.

$p < .05$]. As in the previous experiments, this finding supports the hypothesis that properties are represented locally and exhibit dominant access.

As Table 5 illustrates further, subjects made many more errors for weak property forms (.19) than for dominant property forms (.04) across the same and different form conditions [$F_S(1, 102) = 148.11$, $MSE = 0.014$, $p < .001$; $F_I(1, 90) = 30.05$, $MSE = 0.023$, $p < .001$]. This finding strongly suggests that dominant forms typically interfere with the access of weak forms. The .22 error rate for weak forms in the baseline condition provides an uncontaminated assessment of how frequent such errors are, given that the context trials were unrelated and therefore not responsible for errors on these target trials.

Verifying the same property form earlier reduced errors for both dominant and weak forms relative to verifying different forms earlier. This reduction, however, was much larger for weak forms. For dominant forms, as in the previous experiments, verifying the same property form earlier reduced errors slightly by .03 but did not reach significance [$F_S(1, 102) = 2.03$, $MSE = 0.023$, $p > .10$; $F_I(1, 90) = 0.35$, $MSE = 0.023$, $p > .25$]. For weak forms, verifying the same property form earlier reduced errors significantly by .16 [$F_S(1, 102) = 57.60$, $MSE = 0.012$, $p < .001$; $F_I(1, 90) = 10.02$, $MSE = 0.023$, $p < .01$].

Finally, Table 5 demonstrates a complex pattern of facilitation and interference for errors. Whereas dominant property forms exhibited neither facilitation or interference, weak property forms exhibited both. For dominant forms, the same form condition did not differ from the baseline, indicating no facilitation [$F_S(1, 102) = 0.00$, $MSE = 0.012$, $p > .25$; $F_I(1, 90) = 0.00$, $MSE = 0.023$, $p > .25$]. Although the different form condition exhibited a trend toward interference, it was not significant [$F_S(1, 102) = 2.03$, $MSE = 0.012$, $p > .10$; $F_I(1, 90) = 0.35$, $MSE = 0.023$, $p > .25$]. For weak forms, the same form condition produced .11 fewer errors than the baseline, indicating facilitation [$F_S(1, 102) = 27.23$, $MSE = 0.012$, $p < .01$; $F_I(1, 90) = 4.73$, $MSE = 0.023$, $p < .05$]. Conversely, the different form condition produced .05 more errors than the baseline, with this interference being significant for subjects [$F_S(1, 102) = 5.63$, $MSE = 0.012$, $p < .05$; $F_I(1, 90) = 0.98$, $MSE = 0.023$, $p > .25$].

Concept dominance. As we just saw, the chronic dominance of property forms has considerable impact on verification performance. Properties high in chronic dominance are verified faster and more accurately than properties low in chronic dominance. In designing this experiment, we did not consider another possible form of dominance: *concept dominance.* However, the materials of Experiment 3 allowed us to test it post hoc. Because concept dominance was not counterbalanced for items or subjects, caution is necessary. As is shown, however, the effect of concept dominance is substantial, suggesting that it is an important factor in property verification.

In the dominance scaling reported above, subjects often produced concepts that shared the same local form of a property. For *mane,* subjects produced *HORSE* and *PONY,* which share the same dominant form. Concept dominance reflects differences in how strongly concepts are associated to a local form that they share. For example, subjects produced *HORSE* more often than *PONY* for *mane* (88% vs 13%).[6] Thus, *HORSE* is a more dominant concept for this form of *mane* than is *PONY*. Concept dominance also occurs for weak property forms. For skin, subjects produced *APPLE* and *PEAR,* which share the same weak form. However, subjects produced *APPLE* more often than *PEAR* (25% vs 0%), indicating that *APPLE* is a more dominant concept for this form than is *PEAR*. These large differences in concept dominance suggest that it may be an important factor in verification.

Materials from the same form condition enabled an assessment of concept dominance. In each set of these materials, two of the three concepts had the same property form (e.g., *HORSE* and *PONY* shared a common form of *mane*). In most of these sets, one of these two concepts received a higher dominance scaling than the other (e.g., 88% of the subjects generated *HORSE* for *mane,* whereas only 13% generated *PONY*). Every set for which the two similar concepts differed by more than 5% on the scaling measure was included in this analysis (17 pairs in the dominant form condition and 15 pairs in the weak form condition).[7] For each pair, one concept had been verified on *context* trials in the same form condition (e.g., *HORSE*) and the other concept had been verified on *target* trials across all conditions (e.g., *PONY*). To maximize comparability, latencies for concepts verified on target trials were only taken from the baseline condition (i.e., when both concepts were verified for the first time, as were their properties). Because context and target trials were distributed across all sections of the presentation sequence, their positions were comparable.

As Table 6 illustrates, the same property form was verified 94 ms faster for dominant concepts than for weak concepts (841 vs 935 ms) [$F_S(1, 51) = 11.76$, $MSE = 28{,}615$, $p < .01$; $F_I(1, 60) = 9.71$, $MSE = 18{,}816$, $p < .01$]. Similarly, error rates were lower for dominant concepts than for weak concepts (.06 vs .13) [$F_S(1, 51) = 15.94$, $MSE = .018$, $p < .005$; $F_I(1, 60) = 6.472$, $MSE = .021$, $p < .05$]. Concept dominance and property dominance did not interact for either verification times [$F_S(1, 51) = 2.13$, $MSE = 32{,}333$, $p > .10$; $F_I(1, 60) = .234$, $MSE = 18{,}816$, $p > .25$] or errors [$F_S(1, 51) = .157$, $MSE = .016$, $p > .25$; $F_I(1, 60) = .148$, $MSE = .021$, $p > .25$].

A possible account of the concept dominance effect is that it reflects word frequency, with the words for dominant concepts being more frequent than

[6] As described under ‘‘Method,’’ the dominance scalings for the objects sharing a property do not sum to 1, because subjects generated three objects for each property.

[7] More pairs were omitted for weak forms than for dominant forms because neither concept was ever generated for some weak target properties, thereby producing ties of zero for the two concepts.

TABLE 6
Results for Concept Dominance in Experiment 3

Measure	Dominant property forms		Weak property forms	
	Dominant concepts	Weak concepts	Dominant concepts	Weak concepts
Examples	HORSE-mane HOUSE-roof PICTURE-frame	PONY-mane BARN-roof MIRROR-frame	APPLE-skin ROBIN-breast PILLOW-feather	PEAR-skin SPARROW-breast COMFORTER-feather
Average dominance (%)	79	24	32	5
Verification times (ms)	769	883	914	956
Error proportions	.02	.10	.10	.17
Word frequency	91	15	28	14

Note. Because concept dominance was not intentionally manipulated in the a priori design, these comparisons are post hoc and reflect neither counterbalanced materials nor subjects. Average dominance is from the scaling of dominance in the method section. Average verification times and errors are for first presentations of the concepts and properties.

those for weak concepts. As Table 6 illustrates, dominant concept words were indeed more frequent than weak concept words (59 vs 14) [$F_I(1, 62) = 5.35$, $MSE = 6{,}755$, $p < .05$].

To assess whether word frequency was responsible for the concept dominance effect, the items analysis was redone using word frequency as a covariate. Even with word frequency removed, however, concept dominance still affected verification times [$F_I(1, 59) = 7.36$, $MSE = 18{,}825$, $p < .01$] and errors [$F_I(1, 59) = 5.15$, $MSE = .021$, $p < .05$]. Again, concept dominance and property dominance did not interact for either verification times [$F_I(1, 59) = 0.80$, $MSE = 18{,}825$, $p > .25$] or errors [$F_I(1, 59) = 0.004$, $MSE = .021$, $p > .25$]. Furthermore, the word frequency covariate failed to explain any significant variance in verification times [$F_I(1, 59) = 0.97$, $MSE = 18{,}825$, $p > .25$] or errors [$F_I(1, 59) = 0.18$, $MSE = .021$, $p > .25$]. Thus, the concept dominance effect is both real and substantial. For a given local form, verification is easier for dominant than for weak concepts.[8]

Discussion

Experiment 3 replicated the basic finding in Experiments 1 and 2. Verifying the same property form on a context trial facilitated processing the same form later on a target trial, relative to verifying a different form on a context trial. In Experiment 3, such benefits included both faster verification times and lower error rates. Obtaining these findings in a completely different design with new materials strengthens the case for local property representation and dominant access.

The benefit of verifying a particular property form on an earlier trial occurred not only for weak forms but also for dominant forms. However, weak forms were verified much more slowly than dominant forms, and they exhibited dramatically higher error rates. These latter findings indicate that weak forms are difficult to access in memory and that they are likely to be missed in the search for relevant property information.

Context trials produced facilitation on target trials for verification times and both facilitation and interference for errors. First consider verification times. Verifying a property form on a target trial was faster after verifying the same form earlier on a context trial, relative to a baseline, for both domi-

[8] Analysis of covariance assumes that the relationship between a covariate and a dependent variable is linear. Because response times are typically related linearly to log word frequency—not to raw word frequency—the covariate analysis was performed again using log frequency as the covariate. On removing log frequency, a similar pattern of results occurred. Concept dominance again affected both verification times [$F_I(1, 59) = 6.61$, $MSE = 18{,}564$, $p < .05$] and errors [$F_I(1, 59) = 5.50$, $MSE = .021$, $p < .05$]. Again, concept and property dominance did not interact for either verification times [$F_I(1, 59) = 0.09$, $MSE = 18{,}564$, $p > .25$] or errors [$F_I(1, 59) = 0.16$, $MSE = .021$, $p < .05$]. Finally, word frequency again explained no significant variance in either verification times [$F_I(1, 59) = 1.82$, $MSE = 18{,}564$, $p > .10$] or errors [$F_I(1, 59) = 0.03$, $MSE = .021$, $p > .25$].

nant and weak forms. No interference was found for verification times, given that verifying different forms on context trials did not slow later verifications. Errors exhibited a more complex pattern of both facilitation and interference. Relative to the baseline, errors for weak forms on target trials decreased after verifying the same forms earlier on context trials (i.e., facilitation). Conversely, errors for weak forms on target trials increased after verifying different forms earlier (i.e., interference). Errors for dominant forms on target trials exhibited neither facilitation nor interference.

The results of Experiment 3 suggest that the property forms used in Experiments 1 and 2 tended to be dominant. As a comparison of Tables 3 and 5 reveals, the results for the dominant property materials in Experiment 3 are comparable to the results in Experiments 1 and 2. Conversely, the results for the weak materials in Experiment 3 are not comparable. Thus, any conclusions drawn from Experiments 1 and 2 should probably be limited to dominant property forms. When weak property forms are examined, as in Experiment 3, somewhat different patterns emerge, especially for errors.

GENERAL DISCUSSION

We first consider the implications of our results for the process of property verification. We then consider their implications for theories of representation.

Implications for Property Verification

Property verification has traditionally been viewed as a primitive operation. In semantic network models, verifying a property is viewed as simply establishing a path between two adjacent nodes, one for the concept and one for the property (e.g., Collins & Loftus, 1975). In feature list models, verifying a property is viewed as simply looking up a feature on a concept's feature list. To the contrary, the experiments here suggest that property verification is a much more complicated activity. A given property takes different forms across concepts; these forms may be organized in dominance orders; concepts and properties are accessed somewhat independently; and sophisticated processing strategies, such as analogical verification, operate effortlessly and ubiquitously. As we review our findings, we explore their implications for the process of property verification and raise issues for future research.

Findings from the three experiments fall into two general groups: *dominance effects* and *context effects*. Dominance effects reflect the ease of verifying a property for a concept independent of preceding trials—all that matters are relations between the concept and the property. Conversely, context effects concern the impact of earlier trials on later trials. Table 7 organizes these findings.

TABLE 7
Summary of Effects on Property Verification

Effect	Context trial	Target trial	Description
Dominance effects			
Dominant form benefit	—	*HORSE-mane* vs *LION-mane*	Dominant forms of a property are processed more efficiently than weak forms
Dominant concept benefit	—	*HORSE-mane* vs *PONY-mane*	Dominant concepts associated with a property form are processed more efficiently than weak concepts
Context effects			
Identity benefit	*PONY-mane* *STOVE-burner*	*PONY-mane* vs *P0NY-mane*	Verifying the identical concept-property pair earlier facilitates later verification, relative to baseline
Same form benefit	*HORSE-mane* *STOVE-burner*	*PONY-mane* vs *PONY-mane*	Verifying the same property form earlier facilitates later verification, relative to baseline
Different form cost	*LION-mane* *STOVE-burner*	*PONY-mane* vs *PONY-mane*	Verifying a different property form earlier interferes with later verification, relative to baseline
Weak form amplification	*BUTTERFLY-wing* *HORSE-mane*	*MOTH-wing* vs PONY-mane	Same form benefits are larger for weak property forms than for dominant forms
Weak form amplification	*BIRD-wing* *LION-mane*	*MOTH-wing* vs *PONY-mane*	Different form costs are larger for weak property forms than for dominant forms

Dominance effects. Property verification exhibits two dominance effects: the *dominant form benefit* and the *dominant concept benefit.* In the dominant form benefit, a dominant property form is verified faster and more accurately than a weak form (e.g., *horse mane* is verified faster for *HORSE* than *lion mane* is for *LION*). Indeed, the advantage for dominant forms was substantial, being verified 173 ms faster than weak forms on the baseline trials of Experiment 3 and exhibiting .18 fewer errors.

In the dominant concept benefit, the same property form is verified faster and more accurately when it occurs in a concept that is highly associated to the property than in a concept that is less associated (e.g., the dominant form of *mane* is verified faster for *HORSE* than for *PONY*). The advantage for dominant concepts was also substantial, being verified 94 ms faster than weak concepts in Experiment 3, and exhibiting .07 fewer errors.

These dominance effects suggest several conclusions about the organization of concepts and properties. First, the representation of a property is not centralized but is distributed across multiple concepts. Each local form of the property resides in the representation of its respective concept, with different local forms linked by a common property name. Thus, different local forms for *mane* reside in the concepts for *HORSE, PONY,* and *LION,* with these different forms associated to the common word ''mane.'' Rather than being simple, the representation of a property is complex.

Second, the connections from a property name to its local forms vary in associative strength. For example, ''mane'' is decreasingly associated to *horse mane, pony mane,* and *lion mane* such that these forms take increasingly long to activate. As a result, encoding the property name activates some local forms more rapidly than others, creating an implicit dominance order.

Third, the activation of concepts and properties during verification proceeds at least somewhat independently. Even when the concept name is processed first, it may not constrain the relevant form of the property accessed initially. Instead, the most dominant form of the property tends to be activated first, slowing verification when a less dominant form is relevant.

Fourth, the dominant form benefit may reflect two aspects of the distributed representation for a property. First, a given property form may become increasingly dominant as more concepts contain it. For example, the form of *mane* exemplified by *horse mane* occurs in more concepts than any other form of *mane,* thereby contributing to its dominance. Second, a property form may become increasingly dominant as its local form co-occurs more frequently with the property name, leading to a strong association between them. For example, *horse mane* co-occurs frequently with ''mane,'' thereby enhancing the dominance of this form.

Fifth, the dominant concept benefit may similarly reflect various aspects of the distributed representation of a property. For example, the association from a property name to a local form may be stronger for dominant than for weak concepts, causing local forms in dominant concepts to be accessed more quickly (e.g., ''mane'' may be more strongly associated to *horse mane* than to *pony mane*). Similarly, local forms may be more strongly associated to dominant concepts than to weak concepts, thereby creating more internal priming for dominant concepts (e.g., *horse mane* may receive more activation from *HORSE* than *pony mane* receives from *PONY*). Should such priming occur, it would mean that the activation of concepts and properties is not completely independent. Further experiments are necessary to assess these factors.

Context effects. As Table 7 illustrates, property verification exhibits four context effects: the *identity benefit*, the *same form benefit*, the *different form cost*, and *weak form amplification*. In all four cases, verifying a property on an earlier trial affects verifying a property on a later trial. Such effects can all be viewed as a form of learning—perhaps mostly implicit—where each verification alters the dominance order of a property's local forms.

First, consider the identify benefit. This effect was not demonstrated in any of the three experiments reported here. However, it was reported in another experiment from this series (Solomon, 1997, Experiment 5). In that experiment, a large identity benefit occurred when the concept and property on a context trial were identical to the concept and property on a target trial (e.g., *PONY-mane*, then *PONY-mane*). Under these conditions, substantial facilitation occurred. Identical context trials speeded later target trials by 154 ms relative to the same baseline condition used here in Experiment 3 (i.e., target trials were preceded by unrelated context trials; e.g., *STOVE-burner*, then *PONY-mane*). Because the materials used to establish the identity benefit primarily contained dominant property forms, the magnitude of the identity benefit for weak forms remains unknown. Presumably, it would be at least as large as for dominant forms.[9]

Second, consider the same form benefit. This context effect occurs when the same property form is verified for two different concepts (e.g., *HORSE-mane*, then *PONY-mane*). Specifically, it is the facilitation that a same-form context trial produces on a later target trial relative to the baseline condition in Experiment 3 (e.g., *STOVE-burner*, then *PONY-mane*). Not surprisingly the same form benefit is smaller than the identity benefit, given that only the property overlaps, not the concept as well. For example, the same form benefit in Experiment 3 ranged from 53 to 82 ms compared to the 154 ms identity benefit. The same form benefit in Experiment 3 also took the form of a .11 decrease in error rates for weak property forms.

Same form benefits illustrate the complexity of verifying properties. Rather than being a matter of simple look-up, verifying a property can result from accessing a similar property in a *different* concept. Interestingly, the similarity of the concepts on the context and target trials did not affect this process. In Experiments 1 and 2, the dissimilar concept conditions produced same form benefits at least as large as those in the similar concept conditions.

[9] The identity benefit was not a superficial result of subjects recognizing exact repetitions of earlier trials. Because false trials also included identically repeating materials (e.g., *ELEPHANT-pouch* and *ELEPHANT-pouch*), subjects could not automatically respond ''true'' after seeing an exact repetition. Instead, they had to assess whether the property was actually true of the concept.

For example, *LION-belly* produced the same benefit as *HORSE-belly* on later verifying *PONY-belly*. Thus, the facilitation that results from repeating a local form appears to operate independently of overall concept similarity.[10]

Property alignment provides one account of this surprising result (cf. Gentner & Markman, 1994; Goldstone, 1994; Wisniewski, 1997). During analogical verification, a property form in one concept can be used to verify the same form in another concept—no matter how different the two concepts are—as long as the two forms reside in analogous regions of their respective objects and are reasonably similar in form. When the shared alignable structure between objects constrains comparison to just the right regions, analogical verification can proceed (Solomon & Barsalou, 2001). From informally examining the materials, dissimilar concepts generally appeared as alignable as similar concepts. Thus, the region for *belly* in *LION* is readily alignable with the region for *belly* in *PONY*. Because the properties for most dissimilar concepts resided in analogous regions, they may have supported analogical verification as much as the properties for similar concepts.

Third, consider the different form cost. This context effect occurs when different forms of the same property are verified on context and target trials (e.g., *LION-mane*, then *PONY-mane*). Specifically, it is the interference that a different-form context trial produces on a later target trial relative to the baseline condition in Experiment 3 (e.g., *STOVE-burner*, then *PONY-mane*). Different form costs only occurred here for weak property forms, not for dominant ones, and they only occurred for error rates, not for verification times. Although we only assessed different form costs for dissimilar concepts, it would be interesting to assess them for similar concepts as well. Although it might seem counterintuitive that a property could take different forms in similar concepts, such cases exist. For example, *neck* varies considerably for the similar concepts *DUCK* and *SWAN*, as do *teeth* for *BEAVER* and *OTTER*, and *propeller* for *AIRPLANE* and *HELICOPTER*.

Finally, consider weak form amplification. These effects occur when contexts have more impact on weak forms than on dominant forms. In Experiment 3, the same form benefit for verification times was larger for weak properties than for dominant ones (+82 ms vs +53 ms), although this difference was not significant. An analogous amplification effect was significant for errors, namely weak forms exhibited a same form benefit (+.11), whereas dominant forms did not (.00). Similarly, weak forms exhibited a significant different form cost (−.05), whereas dominant forms did not (−.03). To-

[10] Technically, Experiments 1 and 2 did not demonstrate facilitation because they did not include baselines. However, Experiment 3 demonstrated that the same form benefit reflects facilitation, so it is probably safe to assume that the same form benefits in the similar and dissimilar concept conditions of Experiments 1 and 2 reflected facilitation as well. Solomon (1997, Experiment 5) directly assessed same form benefits in these latter conditions against a baseline and found that they do indeed reflect facilitation.

gether, this pattern of findings suggests that context has more impact on weak forms than on dominant forms. Again, further research is necessary.

Additional issues and possibilities. Some local forms may be so weak in the dominance order for a property that they never become active. As a result, explaining how they could ever be verified accurately is problematic. Consider verifying *tail* for *PEACOCK.* Because *peacock tail* is so unusual and therefore weakly established in the dominance order for *tail*, generating it may be difficult if not impossible. The difficulty of accessing such weak forms may explain their high error rates in Experiment 3. Nevertheless, it seems intuitive that if subjects were required to wait, say, 10 s, before responding, that they would eventually verify such properties accurately.

One way to explain the verification of these very weak property forms is through the use of property *names.* During the verification process, subjects could rehearse the property name in working memory and compare it to names retrieved for property regions examined in the object simulation. When verifying *tail* for *PEACOCK*, the first few forms of *tail* simulated may not produce an analogical match (e.g., *dog tail* and *cat tail*). Over time, however, the word associated to the analogous region of *PEACOCK* (i.e., ‘‘tail’’) may eventually become active and match the word for the target property in working memory, producing a true response. The time to access the name for the property may be long such that it only becomes accessible after several incorrect property forms have been tested. A delay in the time to generate the property’s name seems necessary to explain why it takes so long to verify weak property forms and why so many errors occur. If name comparison occurred quickly, weak forms should not exhibit such long latencies and produce so many errors. More research is needed to explore this possibility.

In the peacock example, the name strategy works because *peacock tail* resides in the same region as the tails for most other animals. When a weak property form resides in a *nonaligned region* of an object, however, subjects must resort to an additional strategy to find the property. This is because the dominant form accessed initially directs search to the wrong region of the target object. For example, when verifying *blade* for *PENCIL SHARPENER*, the dominant form for *blade* directs search to external protruding components, as for the dominant forms of *knife blade*, *propeller blade*, and so on. If the subject does not respond hastily in such cases, however, search to other regions of the object may proceed, with the subject comparing the name of the test property to the names of retrieved properties. Thus, the subject may eventually search the occluded internal region of *PENCIL SHARPENER*, thereby discovering its blade. The name for the property may also help direct search to this region via the association between them. Thus, scanning across the target object may constitute yet another strategy that subjects use to verify properties. Such scanning may be closely related to the scanning that subjects use to produce properties verbally in the property generation task (Wu & Barsalou, 2001). Further research is necessary to address these possibilities.

As the results reported here illustrate, the process of property verification is far from simple and primitive. With respect to representation, local property forms are distributed across concepts in a complex associative structure. With respect to processing, properties can be verified in many manners that include direct matching and analogical matching and that may further include name matching and scanning.

Implications for Theories of Representation

Thus far we have remained neutral on the issue of whether knowledge is represented perceptually or amodally. Regardless of how knowledge is represented, properties appear to exhibit local forms, dominant access, and analogical verification. We now explore implications of these findings for representational format.

As described in the introduction, perceptual theories of knowledge predict local form effects a priori. Because it is difficult, if not impossible, to perceptually represent a single property across all concepts (e.g., *mane*), a single property must be represented as a collection of local forms (e.g., *horse mane*, *pony mane*, and *lion mane*). As a result, verifying one form of a property in a concept only has a limited scope of facilitation in other concepts. Conversely, amodal theories traditionally adopt the global form assumption. Because amodal symbols do not have sensory-motor content, a single global symbol can stand for all the different local forms of a property. Similarly, because amodal theories typically stress the importance of symbolic abstraction over perceptual detail, they adopt the global form assumption, thereby achieving simplicity, generality, and context independence.

The results here clearly disconfirm the global form assumption. By implication, these results further disconfirm classic amodal theories that adopt it. As we saw in Fig. 2, however, amodal theories can implement the local form assumption by including a unique amodal symbol for each local property form. A variety of issues arises in evaluating these particular amodal theories (Barsalou, 1999). One issue concerns the a priori versus post hoc status of these proposals. Perceptual theories predict the local form assumption a priori—indeed, given the wide variation in a property's perceptual forms, they require it. In contrast, amodal theories do not predict the local form assumption a priori, nor do they require it. At the least, these theories have not adopted the local form assumption in representing properties thus far. Furthermore, the ability of amodal theories to implement the local form assumption post hoc reflects their unfalsifiability (e.g., Anderson, 1978). Because these theories can probably represent anything in principle, it is not surprising that they can implement local property representation. Thus, their ability to implement local properties is not particularly impressive, especially when their proclivity for abstracting over sensory-motor detail motivates the a priori prediction that properties take global amodal forms.

A related issue concerns the ability of distributed representational systems to implement the local form assumption. Because context-sensitive variability is an inherent aspect of distributed representations, these systems predict a priori that properties take local forms. Thus, empirical evidence for the local form assumption not only supports perceptual theories, it also supports distributed theories.

Increasing evidence, however, from cognitive neuroscience suggests that concepts are grounded in sensory-motor regions of the brain (e.g., Warrington & Shallice, 1984; Damasio, 1989; Damasio & Damasio, 1994; Gainotti, Silveri, Daniele, & Giustolisi, 1995; Martin, Haxby, Lalonde, Wiggs, & Ungerleider, 1995; Martin, Ungerleider, & Haxby, 2000; Martin, Wiggs, Ungerleider, & Haxby, 1996; McRae & Cree, in press; Pulvermüller, 1999; Rösler, Heil, & Hennighausen, 1995; Tranel, Damasio, & Damasio, 1997). Recent behavioral findings in the cognitive literature similarly suggest that concepts are grounded in sensory-motor systems (e.g., Barsalou, Solomon, & Wu, 1999; Glenberg, 1997; Glenberg & Robertson, 2000; Goldstone & Barsalou, 1998; Mandler, 1992; Solomon & Barsalou, 2001; Stanfield & Zwaan, in press; Wu & Barsalou, 2001; for a summary of related work, see Barsalou, 1999). Together, these findings question amodal theories—including distributed ones—that fail to ground knowledge, at least somewhat, in sensory-motor systems. We hasten to add, however, that some distributed systems *do* ground knowledge in sensory-motor mechanisms. As described by Pulvermüller (1999) and Barsalou (1999), perceptual representations can be defined as statistical patterns in sensory-motor systems that take different forms in different contexts (also see Damasio, 1989). On this view, perceptual representations implement the local form assumption in the spirit of distributed systems.

Finally, our intention is *not* to argue that knowledge is *solely* grounded in perceptual symbols, with amodal symbols playing no role. To the contrary, we only see our results and arguments as implicating sensory-motor grounding *at least to some extent*. It is possible that perceptual and amodal symbols work together to represent knowledge. Again, the primary point is that our experimental results, together with many other findings in the literature, implicate some form of perceptual grounding in the representation of concepts. Determining whether the entire conceptual system rests on perceptual symbols or whether it rests on a mixture of perceptual *and* amodal symbols requires much further research. For arguments about how far perceptual symbols can go in implementing a conceptual system, see Barsalou (1999).

Conclusion

These experiments demonstrate that properties are represented locally, not globally, and that searching through local forms during verification exhibits dominant, not constrained, access. As a consequence, subjects can perform analogical verification, using a local form in one concept to verify a similar form in another concept. Together, these mechanisms produce a variety of effects in property verification, including two dominance benefits (dominant form benefit and dominant concept benefit) and four context effects (identity benefits, same form benefits, different form costs, and weak form amplification). Together, all of these results indicate that the representation and processing of properties is complicated, not simple and primitive. The evidence for local property representation further suggests that concepts are grounded, at least to some extent, in perceptual simulations, given that perceptual views predict this finding a priori. Together with other behavioral and neural findings in the literature, as well as with various theoretical arguments, there is increasing evidence that sensory-motor simulations are central to human knowledge.

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