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"FORTUNATELY, I'M NO EINSTEIN": COMPARISON RELEVANCE AS A DETERMINANT OF BEHAVIORAL ASSIMILATION AND CONTRAST

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We investigated the determinants of whether activated knowledge leads to behavioral assimilation or to contrast. Whereas early studies concluded that behavior assimilates to activated categories but contrasts from activated exemplars, more recent results suggest that the comparison relevance of a prime may predict the direction of its effect. Thus, in four experiments, we manipulated the perceived relevance of category and exemplar primes. Behaviors consistently assimilated to irrelevant primes and contrasted from relevant primes, regardless of the primes' exemplar–category status. Furthermore, participants who explicitly rejected a series of irrelevant self–exemplar comparisons showed relative assimilation to subsequent exemplar primes, suggesting that experience can momentarily render extreme exemplars comparison irrelevant. The role of comparison relevance as a moderator of automatic behavior is discussed.

In recent years, a burgeoning literature has documented the automatic effects of perception on behavior (e.g., Bargh, Chen, & Burrows, 1996; Dijksterhuis et al., 1998; Dijksterhuis & van Knippenberg, 1998; Mussweiler & Förster, 2000; see Bargh & Fergu-

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son, 2000; Dijksterhuis & Bargh, 2001, for reviews). Strikingly, these findings show an unconscious influence of activated knowledge on all manner of responses, from basic functions such as walking speed (Bargh et al., 1996; Dijksterhuis et al., 1998) to seemingly higher level functions such as performance on general–knowledge tests (Dijksterhuis et al., 1998) and the ability to learn new information (Haddock, Macrae, & Fleck, 2002).

In a highly influential article, Dijksterhuis et al. (1998) attempted to organize the myriad findings, demonstrating quite compellingly that behavior assimilates to activated abstract categories (e.g., professors) but contrasts from activated concrete exemplars (e.g., Einstein). Although this principle aptly characterizes their results and the results of others (e.g., Haddock et al., 2002, Experiment 1; Nelson & Norton, in press; Stapel & Koomen, 2000, Experiment 2), these authors and others in the field have recently begun to recognize that the category–exemplar distinction is neither necessary nor sufficient to predict the occurrence of assimilation versus contrast (Dijksterhuis, Spears, & Lépinasse, 2001; Haddock et al., 2002, Experiment 2; Schubert & Häfner, 2003; Stapel & Koomen, 2001). In this article, we propose that the category–exemplar findings can be accommodated within a comparison–relevance framework, whereby the degree to which activated knowledge is seen as an appropriate standard for self–comparison (e.g., Stapel, Koomen, & van der Pligt, 1997) determines the influence such knowledge will have on behavior. We further use the comparison–relevance framework to make novel predictions about the direction of automatic behavior effects.

THE JUDGMENT–BEHAVIOR LINK

Research on the influence of knowledge activation, or priming, on behavior has traditionally been inspired by research on the influence of priming on judgment. Early research on priming and judgment suggested that activating certain traits for participants increased the likelihood that participants' subsequent judgments would assimilate to those activated traits (Higgins, Rholes, & Jones, 1977). For example, participants primed with the construct "adventurous" were more likely to then interpret a risk–taking target's behavior as adventurous, and to evaluate the target more positively, than were those primed with "reckless." Priming of a construct seemingly acts via spreading activation to heighten the accessibility of related knowledge. This heightened accessibility creates a temporary prime–re-

lated interpretive frame, such that subsequent information is perceived through that frame and judged in line with the activated construct (Higgins, 1996).

Bargh et al. (1996) hypothesized that the mental representations activated by a prime might also include the behavioral representations associated with the prime; he and his colleagues thus hypothesized that priming effects may extend beyond judgment to behavior (cf. Dijksterhuis & Bargh, 2001). Indeed, this hypothesis was supported by several experiments in which *behaviors* assimilated to activated traits and stereotypes. For example, participants primed with the trait "rude" interrupted an inattentive experimenter more quickly than did those primed with "polite," and those primed with thoughts of the elderly unwittingly walked more slowly than did those in a control group (Bargh et al., 1996; cf. Kawakami, Young, & Dovidio, 2002). Thus, behavior as well as judgment may assimilate to activated knowledge.

Accessible knowledge does not always yield assimilative judgment, however; judgments often contrast from extreme exemplar primes. For example, participants primed with an exemplar of hostility (e.g., Hitler) judged targets to be *less* hostile than they would have otherwise been judged (Herr, 1986). An exemplar prime, in addition to activating prime–congruent constructs (e.g., hostility), often serves as a comparison standard against which subsequent targets are evaluated. Most targets seem fairly moderate relative to most extreme exemplars; consequently, the implicit prime–target comparisons that follow from exemplar priming lead judgments about the targets to contrast away from the primes (Herr, 1986; see Higgins, 1996, and Dijksterhuis et al., 1998, for more extensive reviews).

Extending this research, Dijksterhuis et al. (1998) investigated whether, like judgments, behaviors automatically assimilate to traits and stereotypes (as instantiated by category labels) but contrast from exemplars. In one experiment, prior to taking a trivia test, participants were asked to list features of either a category associated with intelligence ("a professor"), an exemplar of that category ("Albert Einstein"), a category associated with a lack of intelligence ("a supermodel"), or an exemplar of that category ("Claudia Schiffer"). Although the category primes caused assimilation (higher test scores after *professor* than *supermodel*), the exemplar primes produced contrast (higher scores after *Schiffer* than *Einstein*). Also, within a priming direction (e.g., intelligent), the exemplar yielded relative

contrast, and the category provoked relative assimilation (e.g., lower scores after *Einstein* than *professor*).

Dijksterhuis et al. (1998) conducted a further experiment that showed an elevated association between stupidity–related words and the self after *Einstein*, but not after *professor*. This finding suggests that contrast from the exemplar may have resulted from automatic self–exemplar comparisons; the equivalent automatic self–category comparisons may not have arisen (Dijksterhuis et al., 1998). The authors concluded that activated exemplars serve as a comparison standard against which self–perceptions and behaviors are automatically contrasted, but that activated traits and categories,[1] not invoking such comparisons, serve as an interpretive frame to which self–perceptions and behaviors are assimilated. Others have replicated this pattern of results using a quite varied set of exemplars, categories, and dependent measures (Haddock et al., 2002, Experiment 1; Nelson & Norton, in press; Stapel & Koomen, 2000, Experiment 2).

COMPARISON RELEVANCE

One should not conclude from this prior research, and Dijksterhuis et al. (1998) do not contend, that behavior will *always* contrast from activated exemplars while assimilating to activated traits and categories. In fact, a particularly central moderator of the direction of a prime's influence appears to be its relevance as a comparison standard, and the effects of comparison relevance may override any effects of a prime being an exemplar or a category. Notably, in judgment, only exemplars that are perceived as *relevant* comparison standards lead to contrast. Early psychophysical studies initially established this principle; for example, participants asked to judge the heaviness of various target weights contrasted those judgments from heavy "anchor" weights, but judgments of the targets were unaffected by equally heavy anchor weights when the anchors were shaped differently and referred to as "trays" (Brown, 1953). Labeling and shaping certain weights as trays seemed to prevent those

1. Note that the terminology in prior articles is somewhat mixed, with some using the term "stereotype" (e.g., Dijksterhuis et al., 1998) to refer to what is activated when a construct such as *professor* is primed, but others using the term "category" (e.g., Haddock et al., 2002). We use the latter term in this article because "category" more clearly corresponds to what participants are presented with (e.g., the category name) in these studies.

weights from appearing as relevant comparison standards against which the target weights should be judged.

Such effects of the perceived relevance of comparison standards extend beyond the psychophysical domain. Thus, although judgments of one's own attractiveness are lowered after viewing pictures of attractive exemplars, self–judgments are elevated from baseline when those exemplars are labeled as "models" and are thus rendered irrelevant standards for self–comparison (Cash, Cash, & Butters, 1983). Similarly, although ratings of a person's hostility are generally lower after priming with a hostile human exemplar, those ratings tend to be higher (i.e., assimilative) after priming with a hostile nonhuman exemplar, such as a shark (Stapel & Koomen, 1997; Stapel et al., 1997). Presumably, a human exemplar constitutes a more relevant comparison standard for other humans than does a nonhuman exemplar. Such results suggest that, like category primes, exemplar primes activate prime–congruent knowledge and traits, and that once an exemplar is eliminated as a relevant comparison standard, perceptions and judgments assimilate to the activated knowledge, much like they typically assimilate after category primes (for a further example, see also Stapel, Koomen & Velthuijsen, 1998).

Although one may manipulate comparison relevance by using exemplars that naturally differ in relevance (e.g., animal vs. human), one should also be able to manipulate the comparison relevance of any given exemplar. To illustrate this point, Stapel and Winkielman (1998) manipulated an exemplar, an ape, to appear to be either a relevant or an irrelevant comparison standard for humans: Preceding the ape's description was either a discussion of the ways in which apes and humans are similar or of the ways in which apes are "animal–like" and quite different from humans. Judgments of a human target contrasted away from the ape's behavior in the similar–to–humans condition, presumably because the ape seemed like a relevant comparison standard for humans, but assimilated to the ape's behavior in the different–from–humans condition, when the ape seemed comparison–irrelevant. Thus, by manipulating comparison relevance, one can observe assimilation to and contrast from the same exemplar.

Given that the effects of knowledge activation have often been shown to extend beyond judgment to behavior, as reviewed above, a natural question is whether comparison relevance moderates the direction of knowledge activation effects on behavior, just as it moder-

ates the direction of such effects on judgment. A preliminary investigation of this question was undertaken by Aarts and Dijksterhuis (2002). Like Stapel and Winkielman (1998), Aarts and Dijksterhuis manipulated (and measured) the perceived comparison relevance of animal exemplars. After conceptually replicating Stapel and Winkielman's basic finding, Aarts and Dijksterhuis showed that when comparisons between humans and animals were made to seem legitimate, participants walked more slowly after three fast animal exemplars (antelope, greyhound, cheetah) than after three slow animal exemplars (snail, caterpillar, turtle), demonstrating a contrast effect. However, the reverse pattern (assimilation to the exemplars) obtained when human–animal comparisons were made to seem illegitimate.

Although this pattern of results is suggestive that behavior will not always contrast from exemplars and that the comparison relevance of an exemplar can be manipulated to affect *behavior* (as well as judgment), one should note a few points. First, in the Aarts and Dijksterhuis (2002) study, the effects on the behavioral measures did not attain conventional levels of statistical significance (ps = .08 and .09 for the high- and low-relevance conditions, respectively). Furthermore, Aarts and Dijksterhuis' participants considered three different exemplars per priming condition, and so it is not clear whether participants considered these as three distinct exemplars, or whether they abstracted from the exemplars a general category, such as fast (slow) animals (recent literature suggests that either construal is possible when multiple exemplars are encountered; cf. Dijksterhuis et al., 2001; Haddock et al., 2002). Thus, it seems desirable to consider more fully the role of comparison relevance as a moderator of the direction of priming effects on behavior in order to determine whether these effects are replicable and robust, and to ensure that even the comparison relevance of a single, distinct exemplar, as well as that of a more abstract category, can be manipulated in various ways to affect behavior.

THE CURRENT RESEARCH

Such questions about comparison relevance motivated the current experiments. The foregoing research leads us to predict that activated knowledge—whether a category or a distinct exemplar—can be made to seem more or less comparison-relevant. Participants will treat comparison–relevant activated knowledge as a standard of

comparison, and their behaviors will contrast from the activated knowledge. If the same activated knowledge is rendered comparison-irrelevant, however, participants will be freed from the comparison, and their behaviors will assimilate to the activated constructs and traits.

If the comparison relevance of both exemplars and categories can be manipulated, contrast or assimilation may follow from either type of prime, depending on the prime's comparison relevance. At first glance, these predictions seem to contradict Dijksterhuis et al.'s (1998) finding that behaviors assimilate to activated categories and contrast from activated exemplars. However, as described below, we suggest that the comparison–relevance framework can explain their original finding.

Consider Dijksterhuis et al.'s (1998) finding that behaviors contrasted from the exemplar (Einstein) prime. Although the authors note that the exemplar must be a "relevant standard of comparison" to yield contrast (p. 863), it seems odd that undergraduates—no matter how gifted or confident—would consider Einstein a relevant comparison standard. As Dijksterhuis et al. suggest, participants may have accepted the comparison to Einstein because they were simply unaware that the comparison occurred. People automatically compare themselves to others, especially distinct and extreme others (Gilbert, Giesler, & Morris, 1995). Such comparisons, automatic as they are, often go unnoticed. It is only when an inappropriate comparison to an extreme individual *is* noticed that the encountered individual ceases to serve as a target for self–comparison (Gilbert et al., 1995). Hence, the processes in Dijksterhuis et al.'s study become clear: Because Einstein is an extreme exemplar and a professor is not, the participants automatically compared themselves only to Einstein. And, because they were simply listing traits of Einstein, participants may not have noticed that this inappropriate comparison arose, and therefore may have been unable to prevent their implicit self–evaluations, and thus their behaviors, from contrasting from the prime.

If the comparison to Einstein were noticed, presumably participants would judge Einstein to be a highly irrelevant comparison standard, and his contrastive influence on self–judgments should cease. Comparison to a professor, however, if encouraged, is rather more likely to be accepted by participants as relevant. In addition, prior findings (Aarts & Dijksterhuis, 2002; Stapel & Winkielman,

1998) suggest that it should be possible to manipulate the relevance of any given prime to encourage or discourage its use as a comparison standard. Thus, in what follows, we aim to show that a prime's perceived relevance as a comparison standard, regardless of its exemplar–category status, determines whether ensuing behavior assimilates to or contrasts from that prime.

EXPERIMENT 1

In Experiment 1, we manipulated prime type and comparison relevance, for a 2 (prime type: professor or Einstein) × 2 (comparison relevance: high or low) + 1 (unprimed control group) between–participants design. Immediately before taking a trivia test, participants were primed with either the category *professor* or the exemplar *Albert Einstein*. Comparison relevance was manipulated by having participants list either similarities (high relevance) or differences (low relevance) between themselves and the prime. Our main prediction was that listing self–prime similarities should promote consideration of the prime as a relevant comparison standard, producing contrast. That is, participants should judge themselves to be less intelligent than the intelligent primes, and when approaching the trivia test, should exhibit worse trivia–test performance. Listing self–prime differences, however, should cause the prime to be disregarded as a comparison standard. Even if comparison–irrelevant, primes associated with intelligence should nevertheless activate traits, concepts, and behavioral representations related to intelligence. Thus, when faced with an irrelevant, intelligent prime, participants may assimilate self–evaluations to the activated traits and may approach the trivia test with more intelligent strategies (e.g., systematic thinking; cf. Dijksterhuis & van Knippenberg, 1998) than they would have otherwise. In sum, the predicted main effect of comparison relevance should manifest as those in the low–relevance condition outperforming their high–relevance counterparts, regardless of prime type.

A further prediction follows from a divergence between our similarity/difference–listing task and Dijksterhuis et al.'s (1998) feature–listing task. As discussed, the latter should have led to implicit self–prime comparisons for exemplars only. However, our request to list self–prime similarities or differences may explicitly promote self–prime comparisons (regardless of prime type) and may naturally provoke considerations of those comparisons' appropriateness.

If so, intuition suggests that the category *professor* would be considered a more relevant comparison standard than would the exemplar *Einstein*. Consequently, participants should be more likely to contrast their self–judgments, and to perform poorly, after priming with the category than with the exemplar. To gauge whether a professor is indeed generally perceived to be a more relevant comparison standard than is Einstein, we asked 60 undergraduates, "How appropriate is it for you to compare yourself to [the prime]?" Prime was manipulated between participants, who responded via a –5 (*completely inappropriate*) to +5 (*completely appropriate*) scale. As expected, comparisons to a professor ($M = .31$, $SD = 2.80$) were judged to be significantly more appropriate than were comparisons to Einstein ($M = –1.42$, $SD = 2.31$), $t(58) = 2.62$, $p < .01$. Because the category is considered a more appropriate comparison standard, we predicted a main effect of prime type such that the category should provoke contrast and cause worse performance than the (less relevant) exemplar.

METHOD

Participants. One hundred nine Princeton University undergraduates participated for payment.

Materials. The priming pages were designed to be as similar as possible to those of Dijksterhuis et al. (1998). In the high–relevance, professor–prime condition, participants were instructed, "Think about a professor. On the lines below, please list three ways in which you are similar to a professor." The words "different from" replaced "similar to" for the low–relevance conditions, and "Albert Einstein" replaced "a professor" for the Einstein–prime conditions. There were three lines for participants' responses. The control group received no priming page.

The trivia test consisted of 20 multiple–choice questions, each with four candidate answers. The test and its instructions, presented in the Appendix, were modeled after Dijksterhuis et al. (1998); the questions were adapted from the board game Trivial Pursuit (1984).

Procedure. This task was embedded in a questionnaire packet that required approximately 1 hour to complete. The priming page (for those in one of the four priming conditions), followed immediately by the trivia test, appeared in the middle of the packet. Because most questionnaires in the packet were not related to each other, there was no reason for participants to suspect a connection between the prime and the test; still, to further mask the connection, the prime and the

TABLE 1. Percentage of Trivia Questions Answered Correctly, Experiment 1

	Comparison Relevance					
	Low			High		
Prime Type	*M*	*SD*	*n*	*M*	*SD*	*n*
Professor	48.6	12.8	22	45.2	13.0	23
Einstein	56.2	11.1	21	48.0	13.5	22

Note. Participants in the "Low" group listed self–prime differences whereas those in the "High" group listed self–prime similarities. The unprimed control group scored 50.7% (*SD* = 16.7%, *N* = 21).

test employed different layouts and fonts. The packets were randomly distributed to ensure random assignment to condition.

RESULTS AND DISCUSSION

The percentage of questions answered correctly (see Table 1) served as the dependent measure.[2] To test whether the obtained percentages generalize beyond the sampled participants and items, separate 2 (comparison relevance) × 2 (prime type) ANOVAs were conducted, one with participants as a random factor (F_p), and one with items considered random (F_i; Clark, 1973). In both analyses, the interaction between comparison relevance and prime type was nonsignificant, both Fs < 1.50, *ns*. However, the predicted main effect of comparison relevance was consistently reliable, $F_p(1, 84) = 4.66$, $p < .03$, $F_i(1, 19) = 5.39$, $p = .03$. Participants in the low–relevance (difference–listing) condition performed significantly better (M = 52.3%, SD = 12.5%) than those in the high–relevance (similarity–listing) condition (M = 46.6%, SD = 13.2%). As hypothesized, when participants were encouraged to perceive themselves as unlike a prime, behavior assimilated to prime–activated traits, presumably because the prime was rejected as a comparison standard. When participants contem-

2. If a participant did not indicate a response for a question, the question was scored as incorrect. Omitted responses were very infrequent: In Experiment 1, three participants omitted one response, and one participant omitted two responses. No more than one participant in each condition omitted a response, and when the data are analyzed without these four participants, the pattern of results remains the same (with between–condition differences even slightly increasing). This pattern was the same in all subsequent studies: Omitted responses were very rare, and did not affect the results.

plated how they were similar to a prime, that prime was seemingly adopted as a relevant standard for self–comparison. With the current primes, such a comparison led to negative self–evaluations of intelligence and to lower test performance. Counterintuitively, participants who considered similarities between themselves and an intelligent prime exhibited *worse* performance than did participants who considered differences between themselves and an intelligent prime.

The main effect of prime type also approached significance, $F_p(1, 84) = 3.63$, $p < .06$, $F_i(1, 19) = 10.18$, $p < .01$. As predicted, participants primed with the exemplar ($M = 52.0\%$, $SD = 12.9\%$) outperformed those primed with the category ($M = 46.9\%$, $SD = 12.9\%$). Whereas prior studies found relative contrast from an exemplar and assimilation to a category, we found the opposite, further suggesting that a category–exemplar distinction will not always suffice to explain assimilation and contrast.

Comparison relevance, on the other hand, explains the findings. The listing of similarities or differences made the relevance of self–prime comparisons more focal here than in Dijksterhuis et al. (1998). As shown in pretesting, participants typically consider the category a more relevant comparison standard than the exemplar. The exemplar is therefore less likely to be accepted for comparison and self–evaluation, and more likely to yield assimilation to the activated traits. The category, on the other hand, is more likely to be accepted for comparison, to subsequently induce self–evaluation, and to yield contrast. Thus, manipulation of the prime type under the current similarity–difference instructions may itself be considered another manipulation of comparison relevance. Experiment 1 therefore suggests that comparison relevance, whether manipulated or inherent to the prime, affects performance such that behavior contrasts from relevant, but assimilates to irrelevant, primes.

Finally, note that in the absence of a prime, mean performance was 50.7% correct. Thus, it seems that we observed contrast from baseline when comparison relevance was high and assimilation when low, with Table 1 suggesting in particular that assimilation relative to baseline was strongest following the irrelevant ("list differences") exemplar, and contrast was most pronounced following the relevant ("list similarities") category. Although those two cells reliably differed from each other, $t(42) = 3.00$, $p = .005$, a series of pairwise comparisons revealed that the control group did not differ reliably from

any of the four experimental groups, $.22 < ps < .65$. Still, the relatively small cell sizes in the current experiment may be hindering our power to detect any true deviations from baseline. Experiment 2, which allows for a more powerful test of differences from baseline, addresses this and other issues.

EXPERIMENT 2

We argued above that the results of Experiment 1 demonstrate assimilation to comparison–irrelevant primes and contrast from relevant primes, and that contrast, when observed, stems from a self–prime comparison (cf. comparison contrast, Herr, 1986; Herr, Sherman, & Fazio, 1983). Performance following the similar–to–professor prime was lower, for example, because participants accepted that professor as a relevant standard for self–comparison, and self–comparisons—and thus performance—suffered. A possible alternative explanation for the contrast effect should be considered, however: If participants somehow felt that a prime might have an influence—especially an inappropriate one—on the trivia test, participants may have attempted to correct for the perceived biasing influence of that prime by behaving unlike the prime. Indeed, research has shown that when participants suspect that a prime may bias their impressions of a target, they correct for the presumed bias by attempting to "partial out" the prime's influence from their judgments and behaviors (cf. the set–reset model, Martin, 1986; Martin, Seta, & Crelia, 1990). However, because participants have difficulty discriminating the prime's influence from their own natural impressions and tendencies, this partialing out often amounts to *over*–correction and produces a contrast effect (Martin et al., 1990).

Consistent with this set–reset (or correction contrast) model is the idea that participants may be more suspicious of, and will correct more for, the influence of moderate primes than extreme primes. Essentially, participants may see extreme primes as relatively distinctive and unlikely to contribute to their impressions of targets, but they may be relatively suspicious of moderate primes' influence because those primes are more likely to share features with the targets in question (Moskowitz & Skurnik, 1999). Indeed, moderate trait or category primes have been shown to lead to correction contrast, as participants try to prevent such primes from influencing their judgments, whereas extreme trait or category primes lead to relative assimilation (Moskowitz & Skurnik, 1999).

Following this logic, in Experiment 1, it is possible that when the primes were made more comparison-relevant (i.e., when similarities were listed), participants saw those primes as more likely to bias self–judgment (and thus performance) than when the primes were rendered comparison-irrelevant. In particular, if participants suspected that the priming task could influence test performance at all, the contrast effect in the similar–to–professor condition may have arisen not from self–comparison (as the comparison–relevance framework claims), but instead from participants viewing the prime as less extreme and more likely to bias self–judgments and behavior. Participants may have thus seen a greater need to correct for the biasing influence of the similar–to–professor prime than the other primes; if they overcorrected for the influence of this prime, the observed contrast effect would indeed obtain.

Note, however, that such an explanation seems unlikely. First, Experiment 1 was specifically designed to conceal the connection between the two tasks: The prime and trivia tasks were embedded in the middle of several unrelated experiments, and moreover, the two tasks were presented in markedly different fonts and layouts. Also, for correction contrast to occur with an intelligent prime (e.g., a professor), participants must not only be suspicious of the potentially biasing prime, but must also then correct for that prime by judging the self to be less intelligent and by performing *less* intelligently. Such a correction seems disadvantageous. To the extent that participants were motivated to perform well on the test, they may have *wanted* the primes to influence them. In fact, correction contrast is predicated on the idea that participants feel that the prime's influence would disrupt "current processing objectives" (Martin et al., 1990, p. 29); it is not clear that such disruption would be perceived here. Thus, the correction contrast explanation seems unlikely on both methodological and theoretical grounds. Nevertheless, a careful check of participants' suspicions about any influence the prime could have had would be a more convincing test of the correction contrast explanation (cf. Dijksterhuis et al., 1998). To this end, Experiment 2 was a partial replication of Experiment 1, with an additional written "funnel" debriefing modeled after the one used by Dijksterhuis and van Knippenberg (1998; see also Dijksterhuis et al., 1998). This debriefing was intended to ascertain whether participants perceived any influence of the prime on the trivia task.

METHOD

Participants. One hundred eight University of Florida undergraduates participated for extra course credit.

Materials. Participants received one of three priming pages: a comparison–relevant category prime (self–professor similarities), a comparison–irrelevant exemplar prime (self–Einstein differences), or a control prime. The two experimental primes were selected from the four used in Experiment 1 on the basis that they produced the most extreme effects in that experiment. Thus, it seemed especially important to examine participants' suspicions following those primes. It was also important to replicate the effects of these two priming conditions because these particular conditions produced a reversal of earlier findings (e.g., Dijksterhuis et al., 1998): Whereas others found assimilation to the category prime and contrast from the exemplar prime, we obtained assimilation to the comparison–irrelevant exemplar and contrast from the comparison–relevant category.

The text of the experimental primes was exactly that used in Experiment 1. The text of the control prime was simply, "Think about a chair. On the lines below, list three features of a chair." Experiment 2 was conducted in a marketing department, and all primes were placed on letterhead with the logo of the University of Florida Marketing Department. (The letterhead was employed for the suspicion check described below.)

The trivia test was also taken directly from Experiment 1, with only a slight change in the instructions to explain that the scale, still ostensibly created by the psychology department, was being tested by the marketing department as a favor to the psychology department. A title, "Psychology Department General Knowledge Scale," was added to the scale, and the instructions (replicated in full in the Appendix) were changed to refer to the psychology department in the third person.

The funnel debriefing consisted of two pages. The first page was entitled "Follow–Up Questionnaire," and the instructions explained that the researchers were interested in "gauging your reactions to a few of the studies that you have just completed." Participants were assured of the anonymity of their responses and were encouraged to be honest. We first asked, "The task that you just completed was a multiple–choice trivia test. What do you think the purpose of this task was?" We gave participants four lines on which to respond. We also gave participants one line to respond to, "The instructions on the

trivia test mentioned that one particular department within UF had developed the test. Which department was that?"

The next page, entitled "Follow–Up Questionnaire, page 2," reminded participants that "immediately before the trivia test," they had completed a self–comparison task (or a feature–listing task for control participants). We first asked, "The letterhead at the top of the page identified this comparison (listing) task as being conducted by one particular department within UF. Which department was that?" Finally, participants were asked whether they thought the initial task "may have affected your performance on the trivia test." Participants circled "yes" or "no"; four lines were provided so that those circling "yes" could indicate how their performance may have been affected.

Procedure. This experiment was administered in the midst of several other tasks that participants performed in the laboratory. After completing an unrelated computer task, participants received a packet of questionnaires with the explanation that the questionnaires were compiled from several different research studies. The priming page was the first page in the packet. The trivia test immediately followed, and the debriefing questionnaire followed the test. For the debriefing questionnaire, participants were instructed not to look back at the prime or trivia pages. Questionnaires for unrelated studies completed the packet. As before, all questionnaires used different formats and fonts, and questionnaire packets were randomly distributed to participants to assure random assignment to condition.

RESULTS AND DISCUSSION

Overall Performance. An initial analysis included all participants. The percentage of questions answered correctly served again as the dependent measure. Although overall accuracy was lower than before, the results replicated those of Experiment 1: Participants considering differences between themselves and the exemplar outperformed ($M = 38.9\%$, $SD = 12.8\%$) those considering similarities between themselves and the category ($M = 30.7\%$, $SD = 9.9\%$), with the control group falling in between ($M = 36.6\%$, $SD = 13.0\%$). As before, participants (F_p) and items (F_i) were treated as random factors in separate one–way ANOVAs, which examined the effect of prime type on accuracy. The main effect of prime type was reliable, $F_p(2, 105) = 4.57$, $p < .02$ and $F_i(2, 38) = 7.39$, $p < .01$, as was the difference between priming with the comparison–irrelevant exemplar and the

comparison–relevant category, $t_p(71) = 3.07$, $p < .005$ and $t_i(19) = 3.56$, $p < .01$.

Correction contrast. To examine the role that corrective processes may have played in these results, we first examined participants' attempts to guess the purpose of the experiment. Virtually all participants guessed the purpose to be related to testing "general knowledge" or to otherwise validating the trivia scale. Only one participant (from the similar–to–professor condition) even mentioned a connection between the prime and the trivia test. Although this participant did not exactly guess the experiment's purpose, to be conservative, we removed the participant from the analysis. This removal barely affected the similar–to–professor mean (M = 30.8%, SD = 10.0%) and did not affect the reliability of the difference between the two priming conditions, $t_p(70) = 2.98$, $p < .005$ and $t_i(19) = 3.34$, $p < .01$.

Next, we examined participants' responses to the query about whether the prime could have affected performance on the trivia task, because such suspicions could lead participants to correct for the prime's influence. The set–reset model predicts that contrast is observed following the similar–to–professor prime because there is greater inherent overlap between that prime and the self than between Einstein and the self; thus, participants may feel that the comparison–relevant category prime introduces more bias (and requires more corrective processes) than the comparison–irrelevant exemplar prime (cf. Moskowtiz & Skurnik, 1999). Our results suggest otherwise. Seven participants thought there may have been an effect of the prime; of these, four were in the different–from–Einstein condition (of the 36 in that condition) and three were in the similar–to–professor condition (of the 37 in that condition).[3] Thus, suspicion of influence was not greater following the similar–to–professor prime, suggesting that the contrast effect obtained in that condition did not arise solely from attempts to correct for the influence of this moderate prime. Still, to be certain that our results were not otherwise due to participants correcting for some perceived influence, we removed these seven suspicious participants from the analyses. The pattern of

3. Two Einstein–primed participants thought that the prime hurt performance, one thought that Einstein improved performance, and one was not sure. None of the three professor–primed participants indicated that they thought performance was improved or damaged; they simply thought it may have been "somehow" affected by the prime, and they gave responses such as "because there are certain areas that you are known and some that aren't" (*sic*).

results remained unchanged, $M_{\text{similar–to–professor}} = 31.5\%$, $M_{\text{different–from–Einstein}} = 38.1\%$, $t_p(64) = 2.36$, $p = .02$ and $t_i(19) = 2.93$, $p < .01$. It thus seems unlikely that the priming effects arose because participants were trying to correct for a prime's influence.

As a final test for suspicion of the prime as an inappropriate influence, we examined participants' responses to the questions asking which departments had sponsored the priming task and the trivia test, respectively. The rationale was that participants who correctly remembered that two different departments had sponsored the two different tasks were unlikely to believe that the tasks were related (and were unlikely to suspect that the first task could influence the second). We therefore excluded from the analysis 48 participants who guessed the same department for both tasks. Even with those *potentially* suspicious participants excluded, the pattern of results remained unchanged, $M_{\text{similar–to–professor}} = 29.8\%$, $M_{\text{different–from–Einstein}} = 36.9\%$, $t_p(38) = 2.04$, $p = .05$ and $t_i(19) = 2.61$, $p = .02$.

According to the set–reset model, behavioral contrast is attributable to participants' perceptions of a prime–induced bias, for which participants overcorrect in their subsequent behavior. In terms of the present experiment, set–reset would suggest that this potential bias is more likely to be perceived in the relevant category (similar–to–professor) condition than in the irrelevant exemplar (different–from–Einstein) condition, thus producing correction contrast in the former but not in the latter condition. However, our results do not support this argument. By and large, participants neither guessed the purpose of the study nor suspected that *any* prime had influenced their trivia performance. Furthermore, exclusion of participants who were aware or suspicious, according to several different criteria, failed to affect the pattern of results. Plus, as noted, the set–reset model only predicts that participants correct for a perceived *inappropriate* influence of a prime (Martin et al., 1990); thus, it seems equally consistent with the set–reset model that participants perceiving a bias from the similar–to–professor prime might have welcomed the boost in performance. This would have led to a tendency that was actually opposite the results obtained here. Thus, the set–reset model seems to characterize our results less well than a standard–of–comparison model (cf. Herr, 1986; Herr et al., 1983) that holds that participants implicitly used the comparison–relevant category prime as a standard against which to compare themselves, with contrast following. Participants were seemingly freed from

such contrast effects (and were perhaps able to assimilate their performance to the activated construct of intelligence) when the exemplar Einstein was mentioned but rendered comparison–irrelevant.

Assimilation and contrast. The larger cell sizes in this study enable us to more effectively test whether performance in the priming conditions reliably differed from performance in the control condition. Performance after participants considered the ways in which they were similar to the category was reliably lower than performance after the control prime, 30.7% vs. 36.6%, $t_p(70) = -2.16$, $p < .04$ and $t_i(19) = -2.60$, $p < .02$, suggesting that the comparison–relevant category prime reliably led to contrast from baseline. On the other hand, performance after participants considered the ways in which they were different from the exemplar was indeed higher than control performance (38.9% vs. 36.6%), but not reliably so, $t_p(69) = .78$, $p = .45$ and $t_i(19) = 1.15$, $p = .27$.

Note, though, that in both Experiment 1 and Experiment 2, performance in the different–from–Einstein condition was better, although not reliably better, than in the control condition, hinting that there may in fact be slight but consistent assimilation to this irrelevant exemplar prime. Therefore, to provide a more powerful test, we combined the data from both experiments within the comparison–relevant category (similar–to–professor), comparison–irrelevant exemplar (different–from–Einstein), and control conditions after first standardizing the scores within each experiment. Across the two experiments, standardized scores following the comparison–relevant category prime were reliably lower than scores in the control condition, $z = -.37$ and $+.07$, respectively, $t_p(114) = -2.46$, $p < .02$ and $t_i(19) = -2.52$, $p = .02$, once again indicating a reliable contrast effect. Parallel analyses revealed that the assimilation following the irrelevant exemplar prime, relative to the control condition, was reliable across items, $z = +.33$ and $+.07$, respectively, $t_i(19) = 2.75$, $p < .02$, but not across participants, $t_p(111) = 1.36$, $p = .18$. The discrepant results from the participant and item analyses reflect that the within–condition variance was much greater across participants than across items, rendering less detectable the assimilation effect in the participant analysis.

Experiments 1 and 2 both demonstrate that an exemplar can provoke assimilation and a category can yield contrast, as long as the exemplar is rendered comparison–irrelevant and the category induces

self–comparison. Experiment 2 further shows that the contrast effect following the comparison–relevant category prime is unlikely to result from participants correcting for a perceived bias. And finally, combined analyses from the two experiments clarify that the effects indeed constitute negative and positive deviations from baseline. Thus, comparison relevance appears to reliably moderate the direction of automatic behavior. Experiments 3 and 4 serve to further establish the role that comparison relevance plays in bringing about such effects.

EXPERIMENT 3

We found in Experiments 1 and 2 that performance assimilates to irrelevant primes and contrasts from relevant primes, regardless of whether those primes are exemplars or categories. We argue that comparison relevance may reconcile our results with prior findings of behaviors assimilating to categories but contrasting from discrete exemplars (cf. Dijksterhuis et al., 1998; Nelson & Norton, in press), in that we manipulated whether the primes were considered to be relevant or invalid standards for comparison in a way that prior studies did not. However, subtle methodological differences existed between our research and prior research, and it is important to demonstrate that those differences did not produce the discrepant findings.

For example, Dijksterhuis et al. (1998) gave participants up to 5 minutes to list features of a prime; we limited our participants to listing three features of a prime. Those who listed only three features may have found the task to be easier than those who listed features for a full 5 minutes; the differential ease of feature–listing may be the source of the differences in the primes' impacts (cf. Schwarz et al., 1991). Alternatively, other minor discrepancies between our and prior studies could have caused the difference in results. Consequently, we sought to replicate the original category–assimilation/exemplar–contrast result, along with our result, within the same experiment.

As such, in Experiment 3 we manipulated the instruction type (listing or comparison) as well as the prime type (professor or Einstein) to create four between–participants conditions; in each condition, the number of items that participants were required to list was held constant. Prior to the trivia test, some participants were instructed to simply list three features of the prime, whereas others were instructed to list three similarities to or differences from the prime. Because simply listing features of

TABLE 2. Percentage of Trivia Questions Answered Correctly, Experiment 3

	Priming Instructions					
	Listing			Comparison		
Prime Type	*M*	*SD*	*n*	*M*	*SD*	*n*
Professor	51.9	9.7	13	53.3	13.8	15
Einstein	44.3	15.2	14	60.3	12.9	15

Note. In the "Comparison" conditions, participants listed three self–Einstein differences (when *Einstein* was the prime), but three self–professor similarities (when *a professor* was the prime).

the exemplar (but not of the category) is likely to induce automatic, unnoticed comparison (Dijksterhuis et al., 1998; Gilbert et al., 1995), we expected a replication of prior findings such that simply listing features of Einstein should cause contrast but listing features of professors should cause assimilation. For the comparison conditions, we predicted a replication of the key results of Experiments 1 and 2, such that listing differences from Einstein should render the prime an irrelevant comparison standard (promoting assimilation), whereas listing similarities to a professor should make comparison to the prime seem appropriate (promoting contrast).

METHOD

Participants. Fifty–seven Princeton University undergraduates participated for course credit.

Materials. To replicate Dijksterhuis et al. (1998), the "listing" conditions asked participants to "Think about [the prime]. On the lines that follow, please list three attributes of [the prime]." To partially replicate Experiments 1 and 2, the "comparison" conditions consisted of the low–relevance ("list three differences") Einstein prime and the high–relevance ("list three similarities") professor prime. Participants thus listed three features of Einstein, three features of a professor, three self–Einstein differences, or three self–professor similarities. Performance on Experiment 1's trivia test served as the dependent measure. The formatting of all materials was identical to Experiment 1.

Procedure. Over the course of an hour in the lab, participants completed several unrelated experiments, including a brief question-

naire packet. The priming page, followed immediately by the differently formatted trivia test, occurred approximately in the middle of the packet. Again, packets were randomly distributed to ensure random assignment to condition.

RESULTS AND DISCUSSION

Results are presented in Table 2. As before, separate 2 (instruction type) × 2 (prime type) participant (F_p) and item (F_i) ANOVAs were conducted, with percentage scores as the dependent measure. These analyses revealed no effect of prime type, both Fs < 1, *ns*, but a significant main effect of instruction type, $F_p(1, 53) = 6.28$, $p < .02$ and $F_i(1, 19) = 12.19$, $p < .01$. This main effect of instruction type was qualified by a significant prime × instruction interaction, $F_p(1, 53) = 4.42$, $p < .04$ and $F_i(1, 19) = 8.76$, $p < .01$. As expected, the listing conditions replicated prior results (e.g., Dijksterhuis et al., 1998; Nelson & Norton, in press): Listing features of Einstein yielded worse performance than listing features of a professor. Our failure to obtain that result in Experiments 1 and 2, then, was not attributable to any minor methodological differences, such as limiting participants to three features in the priming task.

Additionally, the comparison conditions replicated Experiments 1 and 2: Listing differences from Einstein provoked better performance than listing similarities to a professor. Thus, a very minimal manipulation—whether participants listed features, similarities, or differences—yielded rather drastic effects: In the Einstein condition, for instance, listing differences rather than features increased scores by 16 percentage points, for a 36% boost in performance.

Comparison relevance can explain the results in both the listing and the comparison conditions. Because exemplars automatically induce comparison (Gilbert et al., 1995), simply listing features of Einstein results in an unbidden, unnoticed comparison. Listing features of professors, however, does not induce such comparison. In the listing conditions, then, the exemplar prime leads to implicit negative self–evaluation and poor performance, whereas the category prime allows assimilation to the prime–activated traits. In the comparison conditions, on the other hand, comparisons to both primes are made explicit. The natural irrelevance of the extreme exemplar is heightened by difference–listing, whereas the natural relevance of the more moderate category is increased by similarity–listing. The pattern of

results then reverses, suggesting that comparison relevance better predicts assimilation and contrast than does exemplar–category status.

EXPERIMENT 4

To augment the evidence for comparison relevance as a key moderator of automatic behavior effects, in Experiment 4 we introduced a new manipulation of comparison relevance. Our aim was to highlight to one group of participants the inappropriateness of using an exemplar prime as a self–comparison standard by giving that group a chance to explicitly compare themselves to a series of extreme exemplars (e.g., Gandhi, the Pope, and so on). Given that most people would consider these extreme exemplars to be inappropriate standards for self–comparison, and given the explicit nature of the comparison questions, we predicted that participants would uniformly reject comparisons to those exemplars. Those participants might then be more likely to be alert for—and to reject—comparisons to subsequent extreme exemplars.

To this end, one group of participants received a Social–Comparisons Questionnaire, whereas another group completed a filler task. The comparisons questionnaire queried participants about how they compared to a series of extreme exemplars; participants in both groups subsequently were primed with Einstein before finally completing the trivia test. We predicted that participants who were encouraged, via the Social–Comparisons Questionnaire, to recognize the inappropriateness of comparisons to extreme exemplars would be more likely to disregard comparison to Einstein, and to therefore perform better on the test than those who were not similarly "inoculated" against irrelevant comparisons.

METHOD

Participants. One hundred twenty–six Princeton University undergraduates participated for payment.

Materials. The Social–Comparisons Questionnaire asked those in the social–comparisons condition ($n = 63$) questions such as, "Relative to Gandhi, are you: more peaceful, less peaceful, or about as peaceful?" Participants answered five such comparison questions, none of which involved comparisons on the dimension of intelligence. (The other exemplars and comparison dimensions were

Mother Theresa, caring; Steven Spielberg, wealthy; Pablo Picasso, creative; and the Pope, religious.) The remaining participants (n = 63), assigned to a control condition, completed a filler questionnaire of similar length. Next, the priming page asked all participants to list three self–Einstein differences, using the instructions from Experiment 1. The trivia test was identical to that used in Experiment 1.

Procedure. This experiment was embedded in a questionnaire packet that required approximately 1 hour of completion time. The materials were located approximately in the middle of the packet, with the Social–Comparisons Questionnaire (for those in the social–comparisons condition) being followed immediately by the priming page and trivia test. All questionnaires used different formats and fonts. The packets were randomly distributed to ensure random assignment to condition.

RESULTS AND DISCUSSION

Because we were attempting to emphasize to participants that self–comparisons to extreme exemplars should be rejected because they are generally inappropriate, we examined whether those in the social–comparisons condition actually considered the exemplars in the Social–Comparisons Questionnaire to be inappropriate for self–comparison. Indeed, the majority of participants in that condition (n = 48) indicated that they possessed less of each target trait than did the listed exemplars (e.g., they indicated they were less peaceful than Gandhi, less religious than the Pope, and so on). Eleven respondents indicated that one of the exemplars was appropriate for self–comparison (e.g., by indicating that they were as religious as, or more religious than, the Pope), and four indicated that two or more of the exemplars provided an appropriate basis for self–comparison. We concluded that the manipulation failed for these latter four participants, and excluded them from further analysis.

As before, we examined percentage correct on the trivia test, conducting parallel analyses to ensure that our results generalize beyond both the given items (t_i) and the sampled participants (t_p). Our predictions were upheld; those who experienced many irrelevant social comparisons prior to the prime outperformed (M = 53.1%, SD = 13.0%) those who did not (M = 49.0%, SD = 12.5%), $t_i(19) = 2.52$, $p = .02$ and $t_p(120) = 1.77$, $p = .08$. This is a small effect (Cohen's d = .32), but

given the weak manipulation, a small effect is not surprising. It is notable that such a subtle manipulation, under otherwise identical conditions, produced any observable effect (cf. Prentice & Miller, 1992). Furthermore, notice that the present experiment used the prime (different–from–Einstein) that yielded the best performance in each of the three preceding experiments. Despite this, rejecting a series of irrelevant comparisons reliably *accentuated* this already powerful manipulation. Thus, a task that allowed participants to note the inappropriateness of comparisons to extreme exemplars encouraged those participants to more fully disregard subsequent such comparisons (to Einstein), and allowed behavior to assimilate to a construct (intelligence) that had been incidentally activated. This alternative manipulation further highlights the role of comparison relevance in bringing about assimilation and contrast.

GENERAL DISCUSSION

These four experiments suggest that the degree to which a primed construct is judged to be a relevant comparison standard determines the direction of that construct's influence on behavior. Experiment 1 showed that the relevance of a given construct can be manipulated by encouraging participants to consider themselves as similar to or different from that construct. When similarities were elicited, comparison and self–evaluation ensued, and performance contrasted from the activated traits. However, the consideration of self–prime differences liberated participants from self–comparisons, and behaviors then showed relative assimilation to the activated constructs. This relationship held regardless of whether a category or an exemplar was primed; furthermore, performance tended to be better following exemplar than category primes, presumably because participants were more likely to accept the category than the exemplar as a relevant standard of comparison.

In Experiment 2, we successfully replicated the effect of comparison relevance while also presenting evidence that it was unlikely that the results were attributable to correction contrast. In Experiment 3, we showed that specific changes in the primes' comparison relevance (as instantiated by the "similarities" and "differences" instructions) were responsible for our pattern of effects: We were able to replicate the earlier pattern of contrast from an exemplar and assimilation to the category by substituting our instructions with the feature–listing instructions used in prior studies. In Experiment 4, we

showed that an alternative manipulation of comparison relevance produced similar results: By presenting one group of participants with a series of irrelevant exemplars, we enabled them to further disregard comparison to Einstein and to consequently perform better on the trivia test than those who did not first disregard prior irrelevant exemplars. The evidence therefore points to an important role for comparison relevance in determining the direction of knowledge–accessibility effects. We now consider the theoretical significance of these results.

THEORETICAL CONTRIBUTIONS AND CONTEXT

Our results augment the prior literature in several ways. Although researchers have long recognized comparison relevance as an important consideration in judgment (Brown, 1953; Stapel et al., 1998; Stapel & Winkielman, 1998; see also Lockwood & Kunda, 1997; Meyers–Levy & Sternthal, 1993), little evidence had existed until now that manipulations of comparison relevance were potent enough to moderate the direction of automatic behavior. Thus, we build on Aarts and Dijksterhuis's (2002) suggestive results to show that one must consider the perceived comparison relevance of primes to predict the effect of those primes on behavior. Furthermore, we extend work on the comparison relevance of exemplars (Aarts & Dijksterhuis, 2002; Stapel & Winkielman, 1998) by showing that the effects of comparison–relevance manipulations extend to the perceived relevance of other constructs, such as categories.

In addition, we showed that comparison relevance can be manipulated with subtler procedures than those used in prior studies. Rather than providing participants with a paragraph implying that an exemplar should or should not be considered a relevant standard of comparison, in Experiments 1 through 3 we simply asked participants to consider similarities to or differences from the primes. That slight variation in instruction sufficed to produce differences in comparison relevance (as manifested in behavior). Experiment 4 highlighted yet another manipulation of comparison relevance, demonstrating that participants can, via a prior "unrelated" task, be encouraged to perceive subsequently encountered individuals as irrelevant for comparison.

Furthermore, the present experiments show that a prime's comparison relevance can be more important than its exemplar–category

status in determining the incidence of assimilation versus contrast. Comparison relevance provides a framework that incorporates prior and current findings alike. In Experiment 3, we demonstrated that behavior assimilates to the category and contrasts from the exemplar when comparison to the exemplar is implicit and comparison to the category does not spontaneously arise (cf. Dijksterhuis et al., 1998; Haddock et al., 2002, Experiment 1; Nelson & Norton, in press; Stapel & Koomen, 2000, Experiment 2). But when comparison to the primes is made explicit, behavior contrasts from relevant primes and assimilates to irrelevant primes, regardless of whether relevance is manipulated or is determined by the prime's more general status as a (relevant) category or an (irrelevant) exemplar.

Other prior research is supportive of a comparison–relevance interpretation. For example, judgmental contrast is more likely when a comparative (as opposed to interpretive) goal has been primed, regardless of prime type (Stapel & Koomen, 2001), corroborating the critical role that comparative processes play in yielding contrast effects. Similarly, behavioral contrast from a prime is more likely when words related to the self are presented during priming, highlighting the importance of *self*–comparison for behavioral contrast (Schubert & Häfner, 2003). Still others have found that a collection of exemplars (e.g., a group of supermodels) may provoke contrastive behavior when participants are encouraged to think of each exemplar as unique, but may provoke assimilative behavior when participants are encouraged to think of the exemplars in terms of their commonalities (Haddock et al., 2002; cf. Dijksterhuis et al., 2001, Experiment 3). This is consistent with the idea that distinct impressions of exemplars will more naturally facilitate implicit contrastive self–comparisons than will diffuse impressions of the same exemplars. And naturally, a myriad of factors orthogonal to comparison relevance, such as cognitive capacity (Dijksterhuis et al., 2001), will also play a role in determining whether assimilation or contrast obtains (see Dijksterhuis & Bargh, 2001; Ford & Thompson, 2000, for reviews).

CONCLUSION

In this article we have demonstrated that, to the degree that a given construct is perceived to be comparison-relevant, behavior contrasts from that construct; conversely, constructs perceived to be irrelevant standards for self–comparison lead to behavioral assimilation. This pattern occurs because activating any construct, whether category or

exemplar, activates associated traits, but accepting the activated construct as a relevant comparison standard causes contrast from the activated traits. Acceptance as a relevant standard may be the default when one is faced with an extreme exemplar (Gilbert et al., 1995), but to the extent that the standard is rejected, judgment and behavior assimilate to the activated knowledge. We are thus left with the surprising conclusion that performance is most facilitated by keeping in mind an exemplar of excellence, but reminding oneself that, fortunately, one is in a completely different league.

APPENDIX

TRIVIA TEST

INSTRUCTIONS TO PARTICIPANTS, EXPERIMENTS 1, 3, AND 4:

Researchers in the Psychology Department are currently developing a "general knowledge" scale. The scale has five subscales that vary in terms of difficulty, ranging from 1 (*very easy*) to 5 (*very difficult*), and we are trying to validate each scale. You'll be receiving subscale number 5 (*very difficult*). Please answer all of the questions below by circling one of the four options. Even if you aren't sure of an answer, please just give your best guess.

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TRIVIA TEST

1. What river is the Tennessee River the main tributary of? Missouri; Rio Grande; *Ohio*; Mississippi
2. What European capital's main street is Alcala? *Madrid*; Paris; Rome; London
3. How is the year 1010 written in Roman numerals? LX; *MX*; MC; MM
4. Which of Picasso's periods of style followed his "blue" period? impressionism; gray; *pink*; cubism
5. Who replies "I know" to Princess Leia's confession of love in *The Empire Strikes Back*? Yoda; C3PO; Luke Skywalker; *Han Solo*
6. How many degrees Fahrenheit is equal to 10 degrees Celsius? *50*; 10; 40; 25
7. What's the best body position to be in if you fall into quicksand? feet first; *on your back*; on your stomach; the fetal position
8. What are bottles of Chianti wine traditionally covered with? *wicker*; cotton; paper; grapes
9. Who wrote *Das Kapital*? Josef Stalin; *Karl Marx*; Adam Smith; Vladimir Lenin
10. How many valves control the sounds of a bugle? 3; 8; 4; *0*
11. What city boasts the world's highest subway? New York City; Chicago; London; *Mexico City*
12. How many mules were there in a 20–mule team? 21; 17; *18*; 19
13. What city was linked to New York via the first regular air mail service in 1918? *Washington, DC*; Chicago; Philadelphia; Los Angeles
14. Where do Rhodes Scholars all go to school? Harvard; *Oxford*; Rhodes College; Cambridge
15. What European country has the most multimillionaires, calculated in its own currency? *Italy*; Sweden; England; Switzerland
16. How many red stripes run completely across the U.S. flag? 12; 5; 13; *3*
17. What's the capital of Maryland? Bethesda; *Annapolis*; Baltimore; Columbia
18. What comedian debuted on *Saturday Night Live* in 1980? Richard Pryor; Arsenio Hall; *Eddie Murphy*; Bill Cosby
19. How many lines are there in a limerick? 7; 4; *5*; 3
20. What chemical discovered in 1939 was effective as an insecticide? Alar; Napalm; Thalidomide; *DDT*

Note: The correct answers for the test (adapted from Trivial Pursuit, 1984) are italicized.

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