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With Great Expectations, Can Two "Wrongs" Prime a "Right"?

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The proportion of related prime–target pairs (relatedness proportion, RP) and prime–target stimulus onset asynchrony (SOA) was varied to determine the involvement of strategic priming mechanisms in the reduction in semantic priming that occurs when a target follows an unmasked prime that itself receives immediate repetition priming from a masked prime. At 300-ms and 1,200-ms SOAs, (a) strategic semantic priming was operating, in that priming from a nonrepeated prime increased as RP increased from .25 to .75, and (b) for both RPs, prime repetition reduced semantic priming. At a 167-ms SOA, (a) priming from a nonrepeated prime was unaffected by RP, suggesting that strategic priming was not operating, and (b) for both RPs, prime repetition did not reduce semantic priming. Because prime repetition did not reduce priming at the 167-ms SOA (when only spreading activation should have been mediating semantic priming), the reduction in semantic priming produced by prime repetition is not evidence against spreading activation automaticity. Possible mechanisms through which prime repetition reduces semantic priming are discussed.

In the standard single-word semantic-priming paradigm (see Neely, 1991, for a review), people read a prime word silently and then either pronounce aloud or make a lexical (*word/nonword*) decision to an immediately following target word. Typically, reaction times (RTs) are faster to a target word (e.g., *BUTTER*) when it is preceded by a semantically related prime word (e.g., *BREAD*) rather than an unrelated prime word (e.g., *WALL*). One account of this semantic-priming effect is that the presentation of the prime word automatically activates its representation in memory and this activation spreads from the prime's representation to the representations of semantically related *neighbors* (Anderson, 1983; Collins & Loftus, 1975; Neely, 1977b; Posner & Snyder, 1975), making them more quickly accessible. In accordance with Posner and Snyder's (1975) original criteria for automaticity, this activation of semantically related neighbors by a prime is thought to be (a) fast acting, (b) capacity free, and (c) strategy free (i.e., occurs without intention or awareness). Thus, the presentation of a prime word should facilitate the processing of related words to the same extent, regardless of one's conscious intentions and regardless of any concurrent attention-demanding tasks.

In experiments designed to test Posner and Snyder's (1975) claim that semantic activation is independent of conscious intentions, researchers have sought to influence people's intentional use of the prime by manipulating the proportion of related prime–target pairs (relatedness proportion, RP). When the RP is high, people presumably adopt the strategy of using a prime to generate potential targets that are related to that prime. Targets included in this generated "expectancy set" are recognized more quickly than those that are not (Becker, 1980, 1985). It is assumed that this expectancy strategy is under intentional control and that it takes 300 ms or more following the prime's onset for the expectancy set to be generated (Neely, 1977b; Posner & Snyder, 1975). When the RP is low, people should be less likely to generate expectancy sets, because engaging in such a process would facilitate performance on only the few related prime–target trials and might even hurt performance on the more frequent unrelated prime–target trials for which the target would be unexpected. (See Neely, 1991, for a review of studies showing that RTs are slower for an unexpected target than for a target that follows a neutral, XXXX prime.)

Numerous experiments have shown that when the stimulus onset asynchrony (SOA) between the prime and the target is long enough for people to generate expectancies from the prime (over 300 ms), semantic-priming effects are greater in high RP lists than in low RP lists (de Groot, 1984; den Heyer, 1985; den Heyer, Briand, & Dannenbring, 1983; Neely & Keefe, 1989; Neely, Keefe, & Ross, 1989; Seidenberg, Waters, Sanders, & Langer, 1984; Stolz, Carr, & Besner, 2000; Stolz & Neely, 1995; Tweedy, Lapinski, & Schvaneveldt, 1977). However, when the SOA is short (under 300 ms), RP often has little or no effect on semantic priming (e.g., den Heyer et al., 1983; Stolz et al., 2000; Stolz & Neely, 1995),[1] presumably because these short SOAs do not give people enough time before target presentation to use the prime to

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[1] The one study that found an RP effect at a SOA shorter than 300 ms was reported by Henik, Friedrich, Tzelgov, and Tramer (1994). However, in their study, people had to respond to the prime after responding to the target.

generate expectancies for related targets.[2] Because priming effects at short SOAs are just as large for low RPs—which should provide little or no motivation for people intentionally to use the prime to generate expectancies—as for high RPs—which should provide a high motivation for them to do so—these findings suggest that semantic priming is mediated by an automatic spreading-activation process that does not depend on intentional strategies.

Over the last two decades, the idea that spreading activation is automatic has been challenged and even recently called a myth (Besner, Stolz, & Boutilier, 1997; Stolz & Besner, 1999). One finding often cited as challenging the automaticity of semantic activation comes from experiments demonstrating that the priming observed when a prime is read silently or named aloud is eliminated when the prime is more "shallowly" processed (cf. Craik & Lockhart, 1972), as would be so when the prime is searched to determine if it contains a specific letter (e.g., Friedrich, Henik, & Tzelgov, 1991; Henik, Friedrich, Tzelgov, & Tramer, 1994; M. C. Smith, 1979; see Maxfield, 1997, for a review.) This result has been interpreted to show that when people engage in shallow processing of the prime, the amount of activation that spreads from that prime's representation to the representations of its related associates is reduced or eliminated. If this interpretation is correct, these results show that semantic activation is not automatic. However, in a recent review, Neely and Kahan (2001) challenged that interpretation and argued more generally that before using the finding of a manipulation's reducing or eliminating semantic priming to infer that semantic activation is not automatic, one must rule out two alternative explanations.[3] One such alternative explanation for the letter-search paradigm is that the automatic activation from the prime decays during the time that the response required to the prime is being made. Evidence for such decay comes from an experiment by Henik et al. (1994) in which the letter-search or naming response to the prime was made after the lexical-decision response to the target, which allowed Henik et al. to control the prime–target SOA. Averaged across RP conditions, Henik et al. found significant, statistically equivalent priming effects from named primes (54 ms) and letter-searched primes (52 ms) at the 240-ms SOA, whereas at the 840-ms SOA, they found significant priming (62 ms) from named primes but not from letter-searched primes (1 ms).[4] Thus, letter search on the prime may affect semantic priming through the decay of the semantic activation produced by the prime rather than through its initiation.

A second alternative explanation that Neely and Kahan (2001) give for why a manipulation might reduce priming appeals to that manipulation's reducing expectancy-based priming. The best support for an expectancy-based interpretation of a manipulation's reducing semantic priming comes from an experiment reported by Fuentes, Carmona, Agis, and Catena (1994). They found that a dual-task requirement reduced priming for "attended" (foveal) primes, which likely were used to generate expectancies, but not for simultaneously presented "unattended" (parafoveal or masked) primes, which presumably could not have been easily used to generate expectancies. The findings that (a) the dual-task requirement reduced but did not eliminate priming for the attended primes and (b) priming was significant and equivalent for unattended primes in both the dual- and single-task conditions suggest that priming in these conditions was being produced by automatic spreading activation and that the reduction in priming from the dual-task requirement was solely due to an interference with expectancy-based priming.

One example of a reduction in priming that seemingly cannot be attributed to activational decay or expectancy reduction (or to spatially focused attention; see Footnote 3) has been reported by Neely, VerWys, and Kahan (1998). In their experiment, Neely et al. (1998) used a two-prime procedure in which the first prime (P1) and the second prime (P2) were either the same word repeated or two unrelated words. In the repeated prime conditions, the repetition-primed P2 was followed, after a 300-ms SOA, by a target to which it was either semantically related (e.g., *salt–SALT–pepper*) or unrelated (e.g., *loan–LOAN–pepper*). In the nonrepeated prime conditions, the unrelated P1 and P2 were also followed by a target that was either semantically related to P2 (e.g., *wave–SALT–pepper*) or unrelated to P2 (e.g., *wave–LOAN–pepper*). In all four of these conditions, P1 was briefly presented and masked. Because this procedure required no response to P2 and enabled the use of a brief 300-ms SOA between P2 and the target, it should have minimized the role of activational decay. The role of expectancy should also have been minimized because a low RP of .25 was used in addition to the short 300-ms P2-target SOA. Nevertheless, Neely et al. (1998, Experiment 1) found that although the nonrepeated prime condition showed a substantial 42-ms priming effect, repeating the prime word significantly reduced the amount of semantic priming to a nonsignificant 11 ms. Because Neely et al. (1998) found that semantic priming was nearly eliminated under conditions that should have minimized the roles of decay and expectancy, their results potentially provide strong evidence against semantic-activation automaticity.

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[2] Attributing the strategic priming effects obtained in these RP studies to expectancies alone is an oversimplification because all but one of these studies (i.e., Neely et al., 1989) confounded the RP with the *nonword ratio* (NWR; the probability that a target is a nonword, given that it is unrelated to its prime). Neely et al. (1989) suggested that in the lexical-decision task, increases in the NWR lead to increased use of a strategic, retrospective semantic-matching mechanism (cf. Balota & Lorch, 1986; Chiarello, Senehi, & Nuding, 1987; Forster, 1981; Neely, 1976, 1977b, 1991; Neely & Keefe, 1989; Neely et al., 1989) that is more likely to occur at long than short SOAs (see Neely & Keefe, 1989, p. 218). Hence, some of the increase in priming that occurs with increasing RP could be due to a greater strategic utilization of semantic matching as well as to a greater strategic utilization of expectancy. However, to facilitate the exposition we will act as though the confounded changes in RP and NWR are having their effects by influencing only expectancy and defer until the General Discussion a consideration of the role that semantic matching might play in producing the results we report here.

[3] Neely and Kahan (2001) also considered a third alternative account of why a manipulation might reduce or eliminate semantic priming without affecting the initiation of spreading activation per se. Because this account only applies to situations in which attention is spatially focused on a single letter within the prime, as likely is so in the prime letter-search procedure, it is not directly relevant to the present research and hence will not be discussed any further.

[4] The priming effects we report here come from groups in which the prime letter-search and prime-naming tasks were given in the first block of the experiment. The results were different when each of these two tasks was presented in a second block that followed exposure to the other task in the first block.

Center-Surround Theory: Inhibition From a Masked Prime?

Rather than concluding that repetition priming reduces spreading activation, which would suggest that semantic activation is not automatic, Neely et al. (1998) appealed to a "center-surround" theory proposed by Carr and Dagenbach (1990) and Dagenbach and Carr (1994), in which a masked prime does indeed produce spreading activation automatically, as does an unmasked prime. However, when people have difficulty retrieving the activated meaning of a masked word, they focus their attention on that word's lexical representation (the *center*) such that the meanings of semantically similar words (the *surround*) are inhibited. As evidence for this, Carr and Dagenbach found that when people are induced to try to retrieve a masked prime's meaning, the masked prime produces a greater *facilitatory repetition* priming effect (for the center) but also produces *inhibitory semantic priming* (for the surround). However, when people passively process the masked primes and hence do not invoke the center-surround mechanism, facilitation occurs from masked primes for both repetition and semantic priming due to automatic semantic activation. (See Kahan, 2000, for an alternative interpretation of Carr & Dagenbach's, 1990, results.)

Neely et al. (1998) suggested that the reduction of semantic priming from repeated primes could be explained by the center-surround theory if "(1) [participants] tried to retrieve the masked P1's meaning and failed, (2) [the resulting] inhibition was not 'released' by the easy-to-see unmasked presentation of the nominally identical P2, and (3) the perseverating inhibition from the masked P1 summed with the [automatic] spreading activation facilitation from P2" (p. 38). An additional assumption that Neely et al. (1998) did not make explicit is that people engage the center-surround mechanism for the primes even when the RP is low, as was so in their experiments. However, this hidden assumption runs contrary to claims made by Stolz and Besner (1997). In their Experiment 2, with randomly intermixed short and long duration primes and an RP of .50, a 34-ms priming effect occurred for the long duration primes, but a smaller and nonsignificant 9-ms priming effect occurred for the short duration primes (see also M. C. Smith, Besner, & Miyoshi, 1994). To explain this, Stolz and Besner (1997) suggested that when people notice the relevance of semantics, they intentionally engage the center-surround mechanism to increase the accessibility of the meaning of the prime word. If the prime's meaning is not retrieved, as would be more likely for short duration primes, the resulting center-surround inhibition would summate with the facilitation from automatic spreading activation to produce an overall *null* priming effect. In support of this hypothesis, when Stolz and Besner (1997, Experiment 3) reduced the RP to .25 to make the relevance of semantics less apparent, priming effects of about 20 ms were now found for both the short duration and long duration primes. Hence, Stolz and Besner (1997) argued that with an RP of only .25, the center-surround mechanism did not operate so as to nullify the facilitation from automatic spreading activation for the short duration primes (see Bushell, 1996, for a related finding.) If this argument is correct, it undermines Neely et al.'s (1998) appeal to the center-surround mechanism as an explanation for reduced priming from repeated primes at their RP of .25.

Does Prime Repetition Reduce Expectancy-Based Priming?

Before concluding that Neely et al.'s (1998) results unequivocally refute the idea that the initiation of semantic activation is automatic, one must reconsider the possibility that the reduction in priming they observed for repeated primes was due to prime repetition's affecting expectancy or affecting the decay of semantic activation rather than its initiation. Neely et al. (1998) argued against a role for expectancy in their experiments because of two procedural features. The first was that their 300-ms P2-target SOA was presumably too short for people to have had time to use P2 to generate an expectancy for a related target. However, there are reasons to challenge this presumption, which is based on the earlier cited den Heyer et al. (1983), Stolz et al. (2000), and Stolz and Neely (1995) findings that an effect of RP on priming, which is considered to be the signature for expectancy operating, does not occur at SOAs of 300 ms or less. Although there was no hint of an RP effect on priming at den Heyer et al.'s (Experiments 2 and 3) 75-ms SOA (indeed for the weighted average of these two experiments, priming was 11 ms less at the high RP than at the low RP); at SOAs in the 200–350-ms range, the results were less clear cut. With SOAs ranging from 200–240 ms, de Groot (1984), Stolz et al., and Stolz and Neely failed to find a statistically significant RP effect on priming. However, in these three studies, respectively, priming was 16 ms, 8 ms, and 4 ms (averaged across bright and dim targets and weakly and strongly associated primes and targets) greater at their high (.50 or .75) RPs than at their low (.25) RPs. Moreover, with an SOA of only a slightly greater 350 ms, Stolz et al. found that priming was 22 ms and 8 ms greater for RPs of .75 and .50, respectively, than for an RP of .25. (The statistical significance of these two RP effects was not reported.) Hence, expectancy may operate to some small degree at SOAs in the 200–350-ms range, but individual experiments may not have enough statistical power for detecting an effect of RP. If so, expectancy may have been operating in Neely et al.'s (1998) experiments and repetition priming of the prime may have reduced semantic priming by affecting expectancy rather than spreading activation, in which case Neely et al.'s (1998) results would not be evidence against the automaticity of spreading activation.

But even if Neely et al.'s (1998) 300-ms SOA was enough time for people to generate expectancies from the prime, there was a second reason for Neely et al. (1998) to argue that expectancy was not operating. That is, the .25 RP should not have been high enough to motivate people to use P2 to generate an expectancy for a related target, since a majority of the primes were followed by unrelated targets. However, once again there was no direct evidence for this in the Neely et al. (1998) data. Indeed, we know of no evidence relevant to whether expectancy does operate to some degree even at an RP of .25. (Such evidence would take the form of priming being greater at an RP of .25 than an RP of .05, or of inhibition occurring in the unrelated priming condition relative to a neutral priming condition—see Neely, 1991—at an RP of .25.)

Experiment 1

The purpose of Experiment 1 was to obtain direct empirical evidence that expectancy-based priming effects were indeed not operating at Neely et al.'s (1998) .25 RP and 300-ms SOA. To obtain such evidence, one must manipulate RP and show that priming does not increase as RP increases (cf. Neely & Kahan, 2001; Stolz & Neely, 1995). If semantic priming is still reduced following repeated primes in the absence of an RP effect, only then can one be confident that the reduction is not due to expectancy, and only then would Neely et al.'s (1998) results be strong evidence against spreading-activation automaticity. However, if an RP effect were to be observed at Neely et al.'s (1998) 300-ms P2-target SOA, then expectancy generation is indeed possible within 300 ms in the repeated-prime paradigm and may even have occurred to some degree at Neely et al.'s (1998) .25 RP. That is, although expectancy-based priming is typically discussed in an "all or none" fashion, operating at high RPs and not operating at all at low RPs, perhaps expectancy occurs to some degree even at a .25 RP, but occurs more often or more strongly as the RP is increased to .75, producing an RP effect. If expectancies were indeed operating to some extent in Neely et al.'s (1998) experiments at their .25 RP, then the reduction in semantic priming from repeated primes could have been solely due to prime repetition's interfering with expectancy-based priming rather than to its decreasing automatic spreading activation. By this account, (a) the nonsignificant 11-ms priming effect observed for repeated primes was a genuine but undetected effect equal in magnitude to the spreading-activation priming that occurred for nonrepeated primes, and (b) what prime repetition did was to eliminate the remaining 31 ms of expectancy-based priming that occurred for nonrepeated primes. Under this scenario, Neely et al.'s (1998) results would not qualify as evidence against semantic-activation automaticity.

To determine if expectancy-based priming could have been at least partially responsible for Neely et al.'s (1998) finding of reduced semantic priming from repeated primes, we used Neely et al.'s (1998) 300-ms P2-target SOA and increased the RP for half of the participants from .25 to .75 in Experiment 1. If priming is the same for the .75-RP and .25-RP lists at this 300-ms SOA, indicating that expectancy is not operating, and we find an equivalent reduction in semantic priming from repeated primes for both RPs, this would rule out prime repetition's reducing semantic priming by interfering with expectancy-based priming. This would suggest that semantic activation following holistic prime processing is not automatic, contrary to Neely and Kahan's (2001) claims. Alternatively, if we find greater priming in the .75-RP list than in the .25-RP list, this would show that expectancy-based priming was indeed occurring at Neely et al.'s (1998) 300-ms SOA, contrary to their claims. If we then replicated the Neely et al. (1998) finding of reduced semantic priming at both the .25 and .75 RPs, it would be ambiguous as to whether this reduction was due to repetition reducing expectancy-based priming, spreading-activation priming, or both. Moreover, if we obtain an RP effect in Experiment 1, it would show that the widely held view that expectancy cannot operate within 300 ms is not generally correct, and that before one can rule out expectancy-based priming in a particular situation, one must test whether an RP effect occurs in that situation (cf. Neely & Kahan, 2001; Stolz & Neely, 1995).

Method

Design. The two prime words in each trial consisted of either two different words or the same word repeated. In addition, P2 was either related or unrelated to the target word. This 2 (prime repetition) × 2 (relatedness) within-subject design produced four conditions: repeated/related (e.g., *tale–TALE–story*), repeated/unrelated (e.g., *army–ARMY–story*), nonrepeated/related (e.g., *cope–TALE–story*), and nonrepeated/unrelated (e.g., *cope–ARMY–story*). The two (repeated vs. nonrepeated) unrelated priming conditions were used to assess priming in the corresponding related priming conditions. Participants were randomly assigned to either a .25-RP list or to a .75-RP list.

Participants. One hundred twenty-four undergraduate men and women from the University at Albany, State University of New York participated for partial completion of a research requirement for an introductory psychology class. All were native English speakers with normal or corrected-to-normal vision. Data from 4 participants who had error rates greater than 40% in any one condition were excluded from the analysis—2 each from the .25-RP and .75-RP groups—leaving 60 participants in each group.

Stimuli. Data are reported from 80 critical target words. For each critical target, two primes were selected. One was related to the target (and would serve as P2 for the related and unrelated priming conditions through counterbalancing) and one was not related to the target (and would serve as P1 in the nonrepeated prime conditions). Sixty of the critical targets, as well as their corresponding 120 prime words, were taken directly from Neely et al. (1998, Experiment 1). As in Neely et al. (1998), the University of South Florida word association norms (Nelson, McEvoy, & Schreiber, 1989) were used to select the remaining 20 target words and 20 related prime words, such that when a person is presented with the prime word, the word with the highest probability of being generated as an associate was selected as the target. The words used as P1 for the nonrepeated prime conditions were unrelated to both the target word and its related prime and were matched on frequency with the corresponding related prime word according to the Kučera and Francis (1967) printed word frequency norms (mean frequency = 111.2 and 112.9, for the unrelated and related prime words, respectively). A list of these critical items, from which all the data are reported, is shown in the Appendix.

Eighty filler trials were created using the Nelson et al. (1989) norms in the same way as for the critical trials. For half of the lists, 40 of these filler trials contained two different prime words that were unrelated to each other and to the target and 40 contained a single repeated prime that was unrelated to the target. These unrelated filler items were included to maintain the overall RP of .25 used by Neely et al. (1998). For the other half of the lists, the overall RP was raised to .75 by replacing P2 of the filler items (40 repeated, 40 nonrepeated) with words related to the target. Four .25-RP and four .75-RP lists were created so that (a) each critical target and (b) each P2 for a critical target appeared once in each condition across lists. This was accomplished by re-pairing the related P2 items with different targets to create unrelated P2-target pairs.[5] No prime or target occurred

[5] Ideally, one would want to counterbalance the items occupying the P1 position so that across participants the same items would be used as P1s for the repeated prime and nonrepeated prime conditions. However, we decided not to do this because we also did not want a given prime or target to appear on more than one trial in a given participant's experimental session (though a prime would necessarily appear twice within a repeated prime trial). To meet this latter constraint and to counterbalance the words that appeared as P1 across the repeated and nonrepeated prime conditions, one would need to halve the number of observations per participant, thereby requiring that either twice as many people be tested or that twice as many prime–target pairs be created to obtain stable data. (Both would be impractical to do and doubling the number of prime–target pairs would

more than once for a given participant, except for in the repeated prime conditions, in which case P2 was a repetition of its immediately preceding P1 on that trial.

In addition to the 160 word-target trials, 100 nonword-target trials were also created. Fifty of these nonword targets were preceded by a repeated prime and 50 were preceded by two nonrepeated primes. All nonword targets were derived from words unrelated to their primes. Overall, the probability of a nonword following a repeated or nonrepeated prime was .38, and the overall probability of a nonword target was also .38. The nonword ratio (proportion of unrelated prime–target pairs that contain nonword targets) was .53 in the .25-RP list and .77 in the .75-RP list (see Neely et al., 1989, and Footnote 1 in the present article). Prior to receiving the total 260 test trials, which were divided into six blocks of approximately 44 trials each, each person received two blocks of 18 practice trials each. The proportions of each condition in the practice trials were approximately equal to those of the following critical test list.

Procedure. Each individually tested participant, seated approximately 60 cm away from a video graphics array monitor, read a set of task instructions displayed on the monitor and then heard them paraphrased by the experimenter. Each trial contained the following events: a 250-ms fixation point (*), a 500-ms forward pattern mask (XXXXXXXX), a 33-ms P1, a 100-ms backward pattern mask (********), a 300-ms interstimulus interval (ISI), a 150-ms P2, a 150-ms ISI, and the target letter string. All stimuli were presented centered on the display monitor. Participants were to press the ?/ key with their right index finger for word targets and the *Z* key with their left index finger for nonword targets. Each target was presented for 2,500 ms or until a response was given. A 2,000-ms blank-screen interval preceded each new trial, and self-paced rest breaks occurred every 44 trials.

Results

Because our RT distributions were positively skewed, as is typical (Ratcliff, 1992), a geometric mean lexical-decision RT for correct *word* responses was calculated for each of the four critical conditions for each participant. Group arithmetic means based on individual participants' geometric mean RTs are presented in Table 1 along with the percentage of errors. Priming effects were computed by subtracting the geometric mean RT or percent errors in a *related* condition from the geometric mean RT or percent errors in its corresponding (repeated vs. nonrepeated) *unrelated* condition. Unless otherwise noted, each effect called statistically significant is associated with a two-tailed $p < .05$.

RTs were submitted to analyses of variance (ANOVAs) with prime repetition and relatedness as within-subjects factors and RP as a between-subjects factor. Because RP effects have been examined only for nonrepeated primes in all earlier studies, to determine if expectancy was operating at Neely et al.'s (1998) 300-ms SOA, we first tested the Relatedness × RP interaction for the nonrepeated primes. Surprisingly, even though the SOA was only 300 ms, a significant RP × Relatedness interaction was obtained for nonrepeated primes such that RTs in the .75-RP list yielded 37 ± 34 ms more priming than RTs in the .25-RP list (i.e., 71 vs. 34 ms, respectively), $F(1, 118) = 4.51$, $MSE = 2{,}225$. (Hereafter, when we report an $X \pm Y$ ms effect, Y refers to the 95% confidence interval.) This significant RP effect suggests that participants were indeed able to generate an expectancy at a 300-ms SOA, contrary to Neely et al.'s (1998) claims.

Table 1
Mean Reaction Times (RTs; in Milliseconds), Percent Errors (PEs), and Priming Effects for the 300-ms SOA Group in Experiment 1

	300-ms SOA				
	RP .25/NWR .45		RP .75/NWR .71		
Condition	RT	PE	RT	PE	RP effect (ms)
Nonrepeat/unrelated	641	5.2	685	3.7	
Nonrepeat/related	607	3.7	614	3.2	
Priming	**+34***	**+1.5**	**+71***	**+0.5**	**+37***
Repeat/unrelated	635	2.2	666	1.7	
Repeat/related	620	1.0	626	1.5	
Priming	**+15**	**+1.2**	**+40***	**+0.2**	**+25**
Reduction in priming[a]	+19		+31*		

Note. Bold numbers represent unrelated minus related difference scores. SOA = stimulus onset asynchrony; RP = relatedness proportion; NWR = nonword ratio.
[a] $M = 25$ ms.
* $p < .05$, two-tailed.

Given this significant RP effect, we examined how priming varied for the repeated versus nonrepeated conditions by first examining the two-way Prime Repetition × Relatedness interaction combined across the two RPs and then examining this interaction separately for the .75-RP and .25-RP groups. Consistent with Neely et al.'s (1998) results, across the two RPs, priming was reduced by 25 ± 21 ms following repeated primes (28 ms) relative to nonrepeated primes (53 ms), as indicated by a significant Relatedness × Prime Repetition interaction, $F(1, 118) = 5.54$, $MSE = 3{,}363$. When analyzed separately, the 31 ± 31 ms reduction in semantic priming from repeated primes for the .75-RP group was nearly significant, $F(1, 59) = 3.92$, $MSE = 3{,}644$, $p < .055$. Although the 19 ± 28.7 ms reduction for the .25-RP group was not significant, $F(1, 59) = 1.76$, $MSE = 3{,}081$, the significant 31-ms reduction that Neely et al. (1998) obtained under nearly identical conditions is well within its 95% confidence interval. Thus, the present 300-ms .25-RP data basically replicate Neely et al.'s (1998) results. Finally, the 12 ± 41.9 (i.e., 31 − 19) ms difference in the reduction in priming from repeated primes between the .75-RP and the .25-RP groups did not approach statistical significance, as indicated by the nonsignificant Prime Repetition × Relatedness × RP interaction, $F(1, 118) = 0.31$, $MSE = 3{,}363$.

The error rate for words was low, with an overall mean of 2.8%. A marginally significant priming effect was obtained with fewer errors following related primes than unrelated primes, $F(1, 118) = 3.66$, $MSE = 22.8$, $p < .10$. In addition, a main effect of repetition was obtained, $F(1, 118) = 27.37$, $MSE = 23.9$, with

make the experimental session so long that participants might not continue to make the effort to use the primes to generate an expectancy for related targets.) Because P1 was masked and separated from the target by P2, we think it highly unlikely that any unmatched subtle differences in the characteristics of the different words that served as P1s in the repeated and nonrepeated prime conditions (they were matched on frequency of occurrence in the language) would have influenced RTs to the targets that followed them so as to produce a spurious main effect of prime repetition or a spurious prime repetition by priming interaction.

participants making fewer errors following repeated primes than nonrepeated primes. (Neely et al., 1998, found no effect of prime repetition on error rates.)

Two 2-way mixed ANOVAs were used to examine the effects of prime repetition and RP on RTs and errors on nonword targets. The main effects of repetition and RP, as well as their interaction, were all nonsignificant for both RTs and percentage of errors. However, these data should be treated with caution as the nonword targets were not counterbalanced across conditions.

Discussion

The surprising result of Experiment 1 was that a reliable RP effect, which indicates the presence of expectancy-based priming (but see Footnote 1), was obtained with the relatively short 300-ms SOA used by Neely et al. (1998). Moreover, in the presence of this significant RP effect, we replicated the reduction in semantic priming from repeated primes obtained by Neely et al. (1998) for both a .75 and .25 RP. (Although, as noted earlier, the 19-ms reduction for the .25-RP group was not significant here, the significant 31-ms reduction that Neely et al., 1998, obtained under nearly identical conditions does not fall outside its 95% confidence interval.) Therefore, we are unable to determine if the reduction in priming from repeated primes obtained by Neely et al. (1998) and in Experiment 1 was due to reduced semantic activation, impaired conscious expectancy, or both. Thus, Neely et al.'s (1998) results and the present results are not compelling evidence against the automaticity of semantic activation. To obtain such evidence, one must observe reduced priming from repeated primes in the absence of expectancy, that is, in the absence of an RP effect. That was the purpose of Experiment 2.

Experiment 2

To isolate the effects of spreading activation and expectancy, we orthogonally manipulated the P2-target SOA along with RP in Experiment 2. To do this, we first increased Experiment 1's 300-ms P2-target SOA to 1,200 ms. This should increase expectancy's contribution to our priming effects by giving people more time to generate an expectancy from P2. Thus, there should once again be an increase in priming from nonrepeated primes as RP increases from .25 to .75. Increasing the P2-target SOA to 1,200 ms should also decrease the contribution that spreading activation makes to priming by giving activation more time to decay. Thus, to the degree that the reduction in semantic priming in Experiment 1 for repeated primes was due to prime repetition's having reduced spreading activation, expectancy-based priming in the .75-RP list at the 1,200-ms P2-target SOA could serve to offset that reduction. (However, see Balota, Black, & Cheney, 1992, for results from a somewhat different type of priming paradigm that run counter to this idea.) If expectancy did offset the effects of reduced spreading activation, the result would be that repetition priming of the prime should lead to little or no reduction of semantic priming in the .75-RP list. That is, with the great expectations engendered by a high .75 RP and a long (1,200 ms) SOA, one might see that two *wrongs* will now prime a *right*. However, if prime repetition reduces semantic priming by diminishing the effectiveness of the expectancy mechanism to produce priming, the opposite should occur. That is, the reduction in semantic priming produced by prime repetition should be greater in the .75-RP list than in the .25-RP list at the 1,200-ms P2-target SOA. Stated another way, if prime repetition reduces expectancy-based priming, the RP effect should be smaller for the repeated prime condition than for the nonrepeated prime condition.

In Experiment 2, we also shortened Experiment 1's 300-ms P2-target SOA to 167 ms. This should eliminate the contribution that expectancy makes to the observed priming effects by precluding people from having enough time to generate an expectancy from P2. If it does, the priming from nonrepeated primes should now be the same for the .75-RP and .25-RP lists. Decreasing the P2-target SOA to 167 ms should also increase the contribution that spreading activation makes to priming by giving activation less time to decay. Thus, to the degree that the reduction in semantic priming from prime repetition in Experiment 1 was due to reduced spreading activation, prime repetition should reduce semantic priming to the same degree for both RPs at the 167-ms SOA, and this reduction should be greater than that observed at the 1,200-ms SOA and perhaps especially for the .75-RP list, in which increased utilization of expectancy at the 1,200-ms SOA could work to offset this reduction. However, to the degree that prime repetition reduces semantic priming by diminishing the effectiveness of the expectancy mechanism, there should be no reduction in semantic priming in either RP list at the 167-ms P2-target SOA. This is predicted because expectancy would not have had time to become engaged at this very short SOA. If this latter result were obtained, the reduction in semantic priming that Neely et al. (1998) observed for repeated primes relative to nonrepeated primes would not qualify as evidence against semantic-activation automaticity after all.

In summary, the results of Experiment 2 should allow us to determine whether the reduction in semantic priming found in Neely et al. (1998) and in Experiment 1 for repeated primes was due to prime repetition's reducing spreading activation or the effectiveness of an expectancy-based priming mechanism. If they show that the semantic-priming reduction was due to reduced spreading activation, this would constitute strong evidence against Neely and Kahan's (2001) claim that semantic activation automatically occurs whenever a prime is holistically processed.

Method

Design, materials, and procedure. Experiment 2 was identical to Experiment 1 except that the 300-ms SOA was increased to 1,200 ms or decreased to 167 ms between-subjects. This was accomplished by increasing or decreasing the original 150-ms ISI between the offset of P2 and the onset of the target to 17 ms or 1,050 ms, respectively. Participants were randomly assigned to one of the four groups produced by the 2 (RP) × 2 (SOA) between-subjects component of the design.

Participants. Two hundred fifty-seven men and women similar to those tested in Experiment 1 participated for the same research credit given in Experiment 1. Data from 17 participants who had error rates greater than 40% in any one condition were excluded from the analysis—4 from the .25 RP and 167-ms SOA group, 5 from the .75 RP and 167-ms SOA group, 3 from the .25 RP and 1,200-ms SOA group, and 5 from the .75 RP and 1,200-ms SOA group—leaving 60 participants in each group.

Results

The data were treated the same as in Experiment 1 and are shown in Table 2. RTs were submitted to ANOVAs with prime repetition and relatedness as within-subject factors and RP and

Table 2
Mean Reaction Times (RTs; in Milliseconds), Percent Errors (PEs), and Priming Effects for the 1,200-ms SOA and 167-ms SOA Conditions in Experiment 2

	RP .25/NWR .45		RP .75/NWR .71		
Condition	RT	PE	RT	PE	RP effect (ms)
1,200-ms SOA					
Nonrepeat/unrelated	690	4.3	760	5.3	
Nonrepeat/related	656	3.5	686	4.5	
Priming	**+34***	**+0.8**	**+74***	**+0.5**	**+41†**
Repeat/unrelated	679	1.8	733	2.2	
Repeat/related	664	1.0	699	1.7	
Priming	**+15**	**+0.8**	**+34***	**+0.5**	**+18**
Reduction in priming[a]	+18		+40*		
167-ms SOA					
Nonrepeat/unrelated	704	3.8	699	4.3	
Nonrepeat/related	678	2.7	668	3.8	
Priming	**+26†**	**+1.6**	**+31***	**+0.5**	**+5**
Repeat/unrelated	695	1.5	674	1.2	
Repeat/related	662	0.7	646	1.3	
Priming	**+33***	**+0.8**	**+28†**	**−0.2**	**−5**
Reduction in priming[b]	−7		+3		

Note. Bold numbers represent unrelated minus related difference scores. SOA = stimulus onset asynchrony; RP = relatedness proportion; NWR = nonword ratio.
[a] $M = 29$ ms. [b] $M = -2$ ms.
† $p < .05$, one-tailed. * $p < .05$, two-tailed.

SOA as between-subjects factors. Because the Relatedness × Prime Repetition × SOA interaction was nearly significant, $F(1, 238) = 3.56$, $MSE = 4{,}129$, $p < .065$, we examined the data separately for the two SOAs.

RTs for the 1,200-ms SOA. To determine if expectancy was operating at our 1,200-ms SOA, we first tested the Relatedness × RP interaction for the nonrepeated primes. The 74 ± 34.1 ms priming effect in the .75-RP list was marginally (i.e., 41 ± 44.9 ms) greater than the 33 ± 30.7 ms priming effect in the .25-RP list, $F(1, 118) = 3.16$, $MSE = 3{,}855$, $p < .08$. This suggests that expectancy was operating at the 1,200-ms SOA.

We next examined the two-way Prime Repetition × Relatedness interaction to determine how priming varied for the repeated versus nonrepeated priming conditions. Consistent with Neely et al. (1998), priming was reduced by 29 ± 22.7 ms following repeated primes (25 ± 20.0 ms) relative to nonrepeated primes (54 ± 22.5 ms), as indicated by a significant Relatedness × Prime Repetition interaction, $F(1, 118) = 6.45$, $MSE = 3{,}942$. As outlined in the introduction to Experiment 2, this replication at our 1,200-ms SOA suggests that the reduction in priming from repeated primes may be caused by reduced expectancy and not by reduced semantic activation. Indeed, the hypothesis that expectancy-based priming is affected by prime repetition is further supported by the finding that the .75-RP list yielded a significant 40 ± 34.7 ms reduction in priming from repeated primes, $F(1, 118) = 5.282$, $MSE = 4{,}509$, whereas the .25-RP list yielded a nonsignificant 18 ± 30.0 ms reduction, $F(1, 59) = 1.50$, $MSE = 3{,}375$. However, as was also evident in Experiment 1, this 22 ± 45.4 ms difference in the size of the reductions observed for the two RPs failed to achieve statistical significance, as indicated by the nonsignificant Prime Repetition × Relatedness × RP interaction, $F(1, 118) = 0.878$, $MSE = 3{,}942$. Another perspective clarifying how this nonsignificant three-way interaction numerically supports the claim that prime repetition reduces expectancy-based priming is that prime repetition reduced the RP effect, which is the signature for the operation of expectancy. That is, the nonsignificant RP effect of 18 ± 39.9 ms for repeated primes was 23 ± 45.4 ms smaller than the nearly significant 41 ± 44.9 ms RP effect for nonrepeated primes.

In short, the 1,200-ms SOA data clearly demonstrate that the reduction in priming from repeated primes is not offset (and in fact may be increased) by encouraging the use of expectancy-based priming mechanisms. For a more direct test of the role that reduced semantic activation may play in the reduction of priming from prime repetition, we turn to data from the 167-ms SOA group.

RTs for the 167-ms SOA. The data from the 167-ms SOA group are shown in the bottom half of Table 2. As with the 300 and the 1,200-ms SOAs, we first tested the simple Relatedness × RP interaction for the nonrepeated primes to determine if expectancy was operating at our 167-ms SOA. Consistent with the prediction that participants would now not be able to mobilize expectancy at such a brief P2-target SOA, priming from nonrepeated primes was now not greater for the .75-RP list than for the .25-RP list, $F(1,118) = 0.091$, $MSE = 1{,}931$. In fact, it was 5 ms less.

Given the absence of expectancy at the 167-ms SOA, we next examined the two-way Prime Repetition × Relatedness interaction, both collapsed across RP and separately for the .75-RP and .25-RP lists to determine if priming was still greater for nonrepeated primes than for repeated primes. Of importance, there was now no difference between the 28 ± 15.9 ms of priming from nonrepeated primes and the 30 ± 20.9 ms of priming from repeated primes, as indicated by the nonsignificant Relatedness × Repetition interaction, $F(1, 118) = 0.034$, $MSE = 4{,}349$. Moreover, this pattern held for both RPs. For the .75-RP list, the 3 ± 26.3 ms reduction in priming from repeated primes did not approach significance, $F(1, 59) = 0.051$, $MSE = 2{,}586$. Similarly, for the .25-RP list, the 7 ± 40.4 ms enhancement in priming from repeated primes was not significant, $F(1, 59) = 0.135$, $MSE = 6{,}112$. Finally, the 10 ± 47.7 ms difference in the size of the reduction/enhancement between the .75-RP and .25-RP lists did not approach significance, as indicated by the nonsignificant three-way Prime Repetition × Relatedness × RP interaction, $F(1, 118) = 0.186$, $MSE = 4{,}350$. The direct evidence from the 167-ms SOA group therefore converges with the indirect evidence from the 300- and 1,200-ms SOA groups: Repeating a prime word does not affect the amount of semantic activation initially produced by that prime but instead appears to interfere with expectancy-based priming.

Error rates. The error rates for word targets were once again low, with an overall mean of 3.0%. As with Experiment 1, an overall priming effect was obtained with fewer errors following related primes than unrelated primes, $F(1, 236) = 4.36$, $MSE = 24.4$. Also, similar to RTs, a significant main effect of repetition was obtained, $F(1, 236) = 64.813$, $MSE = 25.5$, with participants making 2.6 % fewer errors on targets following repeated primes than targets following nonrepeated primes (1.4% vs. 4.0 %, respectively). This general effect of prime repetition on reducing error rates highlights the importance of using repeated unrelated primes as a baseline for assessing priming from a repeated related prime.

Nonwords. Two 3-way mixed ANOVAs were used to examine the effects of prime repetition, RP, and SOA on RTs and errors on nonword targets. For RTs, the main effects of prime repetition, SOA, and RP were all nonsignificant. There was a marginally significant RP × SOA interaction, $F(1, 236) = 3.23$, $MSE = 79{,}319$, such that RTs were faster overall in the .75-RP list than in the .25-RP list for the 167-ms SOA, whereas this pattern was reversed for the 1,200-ms SOA. However, neither of these individual effects was significant. As with RTs, the main effects of SOA and RP on error rates were both nonsignificant. However, there was both a significant Prime Repetition effect, $F(1, 236) = 4.81$, $MSE = 15.6$, and a significant Prime Repetition × RP effect, $F(1, 236) = 9.17$, $MSE = 15.6$. Overall, participants made significantly fewer errors on nonword targets following repeated primes (4.7%) than following nonrepeated primes (5.5%). Also, as indicated by the significant Prime Repetition × RP interaction, this effect of fewer errors following repeated primes occurred in the .25-RP list (4.1% vs. 6.0%), but it was numerically reversed (though nonsignificantly so) in the .75-RP list (5.4% vs. 5.1%). No other effects reached significance. Once again, however, these data should be treated with caution as the nonword targets were not counterbalanced across conditions.

Discussion

The results of Experiment 2 show that when the SOA was decreased to 167 ms and participants were not able to generate expectancies (as evidenced by the absence of an RP effect), prime repetition did not lead to a reduction in semantic priming. However, at a 1,200-ms SOA, when participants were given plenty of time to use an expectancy, we replicated the 300-ms SOA results of both Experiment 1 and Neely et al. (1998) and found a significant reduction in priming from repeated primes relative to nonrepeated primes.

We believe the finding that prime repetition did not reduce priming at the 167-ms SOA strongly supports the conclusion that prime repetition does not stop the initiation of spreading activation from P2. However, before drawing this conclusion, one must consider the possibility that the equal priming from repeated and nonrepeated primes at the 167-ms SOA might reflect residual activation from the masked P1 that compensates for any reduction in spreading activation from P2. This alternative explanation hinges on the assumptions that (a) the activation from the masked P1 influences responding to the target, and (b) this activation decays as the P1-target SOA is increased. However, we believe both of these assumptions to be invalid, as we now discuss.

Neely and VerWys (1996) included a condition in which only the masked P1 item was related to the target (e.g., *easy–MAIN–hard*). As with Neely et al. (1998) and our Experiment 1, Neely and VerWys (1996) used a 433-ms P1–P2 SOA and a 300-ms P2-target SOA. At this 733-ms total P1-target SOA, Neely and VerWys (1996) found only a nonsignificant 12-ms priming effect from the masked P1. However, it is still possible that this nonsignificant 12-ms priming effect would be larger when the P2-target SOA is reduced to 167 ms, with the P1-target SOA being concomitantly reduced from 733 ms to 600 ms. Indeed, if one agrees with the second assumption that activation from a masked prime rapidly decays, then the residual activation from the masked P1 should have been greater in our 600-ms P1-target (167-ms P2-target) SOA of Experiment 2 than at the 733-ms P1-target (300-ms P2-target) SOA used by Neely and VerWys (1996), Neely et al. (1998), and ourselves (Experiment 1), and certainly greater than at the 1,633-ms P1-target (1,200-ms P2-target) SOA. The problem with this analysis is that data on the time course of masked priming suggest that Assumption B (above) is wrong. Specifically, Fowler, Wolford, Slade, and Tassinary (1981) and Balota (1983) have both reported significant priming effects from masked primes at a long SOA of 2,000 ms, yet no significant priming effects from masked primes at short SOAs of 200 ms and 350 ms, respectively. Apparently, unlike for unmasked primes, semantic activation for masked primes takes longer to accrue because the original activation produced by the prime is weaker. Therefore, contrary to Assumption B, the activation from the masked P1 should have been greater at the longer SOAs. Thus, it is unlikely that the selective lack of a reduction in priming from repeated primes at our 167-ms P2-target (600-ms P1-target) SOA was due to residual activation from P1 in the repeated condition. On the basis of there being no reduction in semantic priming from prime repetition at a 167-ms P2-target SOA, we conclude that prime repetition does not affect the initiation of spreading activation.

General Discussion

The most critical and clear-cut finding in the present experiments was that the reduction in priming observed from repeated primes covaries with the RP effect. In Experiment 1, we replicated Neely et al.'s (1998) result of reduced semantic priming for repeated primes using their original 300-ms SOA, which was shown to be long enough for participants to use P2 to generate expectancies, as indicated by a significant RP effect. Thus, it is possible that reduced semantic priming from repeated primes is due to prime repetition's interfering with expectancy-based priming rather than to its stopping the initiation of spreading activation. In Experiment 2, we further tested the hypothesis that prime repetition affects the initiation of spreading activation and obtained results opposite to its predictions. Specifically, at a 1,200-ms SOA, when an RP effect indicated the presence of expectancy-based priming and when spreading activation's contributions to priming should have been minimized, we replicated Neely et al.'s (1998) results. However, at a 167-ms SOA, when the absence of an RP effect indicated the absence of expectancy-based priming and when spreading activation's contribution to priming should have been maximized, prime repetition did not reduce semantic priming. These findings converge to show that prime repetition does not hinder the initiation of semantic activation. Thus, prime repetition's reduction of semantic priming is not evidence against the automaticity of semantic activation.

Although the present results very clearly show that prime repetition does not affect the initiation of spreading activation, they are less clear in delineating the exact mechanism responsible for prime repetition's reducing semantic priming. In the remainder of the General Discussion, we first consider strategy-based interpretations of our findings and their limitations. We do this through a more detailed exploration of how our results could have been produced by prime repetition's interfering with expectancy or semantic matching (see Footnote 1) and by revisiting a center-surround account. We then close by turning to another quite different explanation of our results, which is that prime repetition affects the decay (but not the initiation) of spreading activation and has no effect on strategy-based priming mechanisms.

Prime Repetition and Expectancy

The claim that repetition priming of the prime reduces semantic priming by affecting expectancy-based priming and not spreading activation has several implications. First, as noted earlier, it leads to the prediction that when the P2-target SOA is long enough for expectancy to operate, as was so for the 300-ms and 1,200-ms SOAs, the reduction in semantic priming should be greater for the .75-RP list than for the .25-RP list (or alternatively, the RP effect should be smaller for repeated than nonrepeated primes). This prediction was affirmed numerically in that the reductions in priming from prime repetition were 12 ms (31 vs. 19) and 22 ms (40 vs. 18) greater for the .75-RP list than for the .25-RP list for the 300- and 1,200-ms SOAs, respectively. [Or alternatively stated, the RP effects were 12 ms (25 vs. 37) and 22 ms (18 vs. 41) smaller for repeated than nonrepeated primes for the 300- and 1,200-ms SOAs, respectively.] Because neither of these effects was statistically significant by itself, we sought to increase the statistical power by performing an ANOVA on the combined data from the 300-ms and 1,200-ms P2-target SOAs. However, the Prime Repetition × Relatedness × RP interaction still was not significant, $F(1, 238) = 1.14$, $MSE = 3{,}625$. Despite this lack of statistical support, we are reluctant to reject the expectancy account because the combined average reduction in priming from repeated primes was nearly twice as large in the .75-RP lists (a significant 35 ± 23.0 ms effect) as in the .25-RP lists (a marginally significant 19 ± 20.5 ms effect), and from the alternative perspective on this interaction, the combined averaged 21 ± 25.1 ms RP effect for repeated primes was about half as large as the 38 ± 27.9 ms RP effect for nonrepeated primes. Moreover, these differences in the reductions in priming and the RP effect from prime repetition, which provide support for the expectancy account, were independently replicated at two SOAs.

As previously noted, the claim that prime repetition's reduction of semantic priming is mediated solely by expectancy also implies that expectancy is operating to some degree even at an RP of .25 for which we and Neely et al. (1998) obtained the reduction. If expectancy increases with SOA, this further implies that priming from nonrepeated primes should have increased as SOA increased for the .25-RP list. However, priming for nonrepeated primes in the .25-RP list remained relatively constant with increasing SOA—that is, 26, 34, and 34 ms for the 167-, 300- and 1,200-ms SOAs, respectively—whereas in the .75-RP list, it increased (at least between the 167- and 300-ms SOAs) with increasing SOA—that is, 31, 71, and 74 ms, respectively. Because Neely et al. (1998, Experiment 1) obtained 42 ms of priming from nonrepeated primes at their 300-ms SOA, using procedures virtually identical to those of the present Experiment 1 (the only difference being the addition of 20 new critical items in the present Experiment 1), the best estimate of priming from nonrepeated primes at a 300-ms SOA with the two-prime procedure can be obtained by combining the data from these two experiments. When this is done, priming effects for the .25-RP lists are 26, 38, and 34 ms for the 167-, 300-, and 1,200-ms SOAs, respectively. These SOA-related changes in priming for nonrepeated primes are highly similar to those obtained by Stolz et al. (2000) in an experiment with only single primes and 96 people tested in each RP/SOA group. That is, they found priming effects of 28, 34, and 20 ms at SOAs of 200, 350, and 800 ms, respectively, in their .25-RP lists, and priming effects of 36, 56, and 51 ms in their .75-RP lists. Similarly, de Groot (1984) found priming effects of 58, 66, and 59 ms for her .25-RP lists at SOAs of 240, 540, and 1,040 ms, respectively, and priming effects of 74, 91, and 123 ms in her .75-RP lists. If the small but replicable 8–12-ms increases in priming from the 167–240-ms SOAs to the 300–540-ms SOAs for the .25-RP lists in these three studies are considered genuine, there is some, albeit weak, evidence that expectancy may indeed be operating at the .25 RP. (If the replicable 4–14-ms decreases in priming from the 300–540-ms SOAs to the 800–1,200-ms SOAs in these three studies are considered genuine, this could be due either to a waning of expectancy at the longest SOAs or to a constant or slightly increasing expectancy not offsetting the decay of activation that occurs between these intervals.)

If repetition priming of the prime affects only expectancy-based priming and spreading activation is automatic, then one should still find semantic priming from a repeated prime due to spreading activation. Indeed, a statistically significant 33-ms priming effect was observed for repeated primes at the 167-ms SOA. Although the individual 15-ms priming effects from repeated primes for the .25-RP lists at the 300-ms and 1,200-ms SOAs were not statistically significant—$t(59) = 1.38$ and $t(59) = 1.17$, respectively—nor was the 11-ms priming effect that Neely et al. (1998) obtained with their 300-ms SOA with the current materials—$t(59) = 1.30$—we believe that these three effects are likely real, given their similarity and consistency. Indeed, when the t tests for these three small but independent effects are combined (see Winer, 1971, pp. 49–50), the overall 14-ms priming effect obtained for repeated primes at an RP of .25, averaged over the 300- and 1,200-ms SOAs, is significant ($z = 2.186$).

Clearly, the statistics do not provide strong support for the claim that increasing the RP magnifies the reduction in semantic priming from repeated primes, as would be anticipated if this reduction were due solely to expectancy. Nevertheless, we believe that the weight of the converging evidence is sufficient for one to consider the expectancy account as one viable explanation of Neely et al.'s (1998) and our data. Of course, this raises the question of exactly how prime repetition reduces expectancy-based semantic priming. Because the present research was specifically designed only to determine if prime repetition's reduction of semantic priming was being mediated by its effects on the initiation of spreading activation, we can only offer highly speculative answers to this question. One general kind of explanation is that prime repetition affects the contents of the expectancy set. For example, it is possible that prime repetition automatically draws the focus of attention to the prime's lexical representation, which in turn probabilistically causes the expectancy set to sometimes contain only the prime itself. (If this were not probabilistic, the target would never have been in the expectancy set and hence there would have been no RP effect for repeated primes.) This account predicts that repetition priming of the prime should enhance the effects of repetition priming between P2 and the target (which is a prediction that center-surround theory also makes, though for different reasons). Indeed, unpublished data from our lab support this prediction (Neely & VerWys, 1996). Another highly related possibility is that prime repetition causes the expectancy set to be narrowed to only those items that share with the prime its most highly salient semantic features, which have been "hyperactivated" by repetition priming. A second general class of explanations for how repetition priming of the prime might affect expectancy is that it affects the probability that an expectancy set will be generated rather than affecting its contents. This class of explanation has the flavor of refractory-period accounts of repetition blindness (e.g., Luo & Caramazza, 1995), though in this case it would be semantic rather than repetition "blindness."[6] Because the present experiments were designed merely to determine if prime repetition could be influencing expectancy and not to delineate exactly how it might do so, the present data in no way discriminate among these various speculations.

Expectancy or Semantic Matching?

As noted in Footnote 1, a strategically invoked retrospective semantic-matching mechanism (e.g., Neely & Keefe, 1989) can also be used to explain our results. This retrospective semantic-matching mechanism presumably is engaged only (a) in the lexical decision task, (b) at longer SOAs, and (c) when a majority of unrelated prime–target pairs contain nonword targets (a high non-word ratio; NWR). According to Neely and Keefe (1989), a high NWR encourages participants to strategically "check-back," following the presentation of a target, to see if it is related to the prime word. If the target and prime are related, then people are biased to respond that the target is a word, facilitating responses to related word targets. If they are unrelated, the bias to respond that the target is a nonword facilitates responses to nonword targets and inhibits responses to unrelated word targets. Because NWR and RP are naturally confounded (but see Neely et al., 1989) and were also confounded in the present experiments, we cannot be sure whether the RP effect obtained at our 300-ms and 1,200-ms SOAs was due to participants' greater use of expectancy, greater use of semantic matching, or both, in the .75-RP/.71-NWR list relative to the .25-RP/.45-NWR list. To the degree that our RP effects were actually NWR effects, it is possible that repetition priming of the prime interferes with semantic matching rather than expectancy. However, we tend to support the expectancy-based explanation because it seems intuitively more plausible than the semantic-matching explanation. We are currently conducting experimental tests of these two explanations.

Center-Surround Revisited

Because our Experiment 1 results showed that it was inappropriate to assume, based on extrapolations from the literature, that expectancy was not operating at Neely et al.'s (1998) 300-ms SOA in their .25-RP list, we are reluctant to assume, based on Stolz and Besner's (1997) research, that the center-surround mechanism does not operate at all with an RP of .25. If one allows that the center-surround mechanism could have been operating to some degree at Neely et al.'s (1998) 300-ms SOA in their .25-RP list, the implications of the present data for a center-surround account must also be considered.

According to a center-surround account, a failed attempt to retrieve the masked P1's meaning produces inhibition for items related to P1. When P2 is the same word as P1, this inhibition summates with the facilitation that the clearly visible P2 would produce for a target related to it (and P1), thereby reducing priming. Because the center-surround mechanism is presumably more likely to be engaged with higher RPs (Stolz & Besner, 1997), the center-surround mechanism correctly predicts the numerically greater reductions in semantic priming from repeated primes observed for .75-RP lists relative to .25-RP lists. The absence of a reduction in semantic priming from repeated primes at the 167-ms SOA in both the .75-RP and .25-RP lists could be handled by assuming that with the 167-ms P2-target SOA , the corresponding 600-ms P1-target SOA was not long enough for people to engage the center-surround mechanism for P1 (and thereby produce inhibition for the related target) before the target appeared. If this assumption were correct, there should be no reduction in priming from prime repetition, which was what was observed . However, with P2-target SOAs of 300 ms or longer, the corresponding 733-ms or longer P1-target SOAs may now have become long enough for the center-surround mechanism to become engaged enough to produce inhibition for targets related to the masked P1, thereby reducing semantic priming from the repeated P2.

Although this embellished center-surround account of our data works, we prefer the expectancy (or semantic matching) account because the center-surround account does not provide a unified explanation of how prime repetition reduces semantic priming. That is, it cannot account for prime repetition's reducing semantic priming when P1 is unmasked and can be easily identified, because under those conditions there would be no reason to engage the center-surround mechanism. However, a reduction of semantic priming from prime repetition when both P1 and P2 are unmasked and easily identified has been independently observed in two laboratories, by Neely and VerWys (1996, Experiment 3) and Pitzer and Dagenbach (2001). Because an expectancy-based account of prime repetition's reduction of semantic priming can be used whether P1 is masked or unmasked, we prefer it to a center-surround account, which can account for this reduction only when P1 is masked. At the very least, we believe that acceptance of the center-surround account for conditions in which P1 is masked should await the results of experiments that provide more direct and independent measures of the inhibition that the masked P1 is presumably producing.

Prime Repetition and the Decay of Semantic Activation

Although the present results unequivocally show that prime repetition does not stop the initiation of spreading activation, they do not compellingly rule out the possibility that prime repetition leads to a faster decay of spreading activation and has no effect

[6] This class of explanation also has the flavor of a semantic-satiation effect (Esposito & Pelton, 1971). However, we discount semantic satiation as an explanation of prime repetition's reduction of semantic priming because (a) semantic satiation of the prime would require that it be repeated many times, not just once (Esposito & Pelton, 1971; see also Balota & Black, 1997), and (b) semantic-satiation effects are not observed in a lexical-decision task (Cohene, Smith, & Klein, 1978; Neely, 1977a; L. C. Smith, 1984) but are observed in semantic-categorization tasks (Balota & Black, 1997; L. C. Smith, 1984).

on strategy-based mechanisms such as expectancy (or semantic matching). Indeed, a pure-decay interpretation can easily handle the fact that semantic priming from repeated primes is not significant at the longer SOAs by merely assuming that activation completely decays within 300 ms when primes are repeated. A pure-decay account also receives some numerical support from the observation that in the .25-RP list the 15-ms priming effects from repeated primes at the 300-ms and 1,200-ms SOAs are less than half as large as the 33-ms priming effect from repeated primes at the 167-ms SOA. However, neither of these priming reductions approaches statistical significance. Specifically, for repeated primes, the reduction in semantic priming relative to that observed at the 167-ms SOA was 17.7 ± 39.8 ms for the 300-ms SOA and 17.9 ± 41.9 ms for the 1,200-ms SOA. If expectancy-based and activation-based priming effects are independent and algebraically additive (but see Balota et al., 1992), a pure-decay account would also predict that prime repetition should reduce semantic priming to the same degree for the .25-RP and .75-RP lists. This prediction is borne out if one chooses to follow the statistics and fails to reject the null hypothesis, thereby ignoring the fact that there were two independent replications of the reduction in priming due to prime repetition being nearly twice as large for the .75-RP list as for the .25-RP list. But even if one chooses not to ignore this, a pure-decay interpretation could be embellished to account for the greater reductions in priming for the .75-RP lists than for the .25-RP lists when expectancy was operating at the 300- and 1,200-ms SOAs. The embellishment would take the form of an additional assumption that expectancy effects are greater when activation-based priming effects are greater, as would be so for the nonrepeated prime conditions relative to the repeated prime conditions at the 300-ms and 1,200-ms SOAs, due to a greater decay of activation in the repeated prime condition. Such an assumption correctly predicts that RP (expectancy) effects should be greater for nonrepeated primes, which yielded a 39-ms RP effect averaged over the 300-ms and 1,200-ms SOAs, than for repeated primes, which yielded a corresponding RP effect of only 22 ms. Although the assumption that expectancy effects are greater when activation-based priming is greater is seemingly counterintuitive, there is some evidence for it (see Balota et al., 1992).

Because the inference that prime repetition influences strategically mediated priming mechanisms is so strongly invited by the finding that prime repetition's reduction of semantic priming covaries with RP effects, it is a bit surprising that these same findings are also congruent with an account that assumes that the only effect of prime repetition is that it leads to a greater decay of spreading activation during the first 300 ms following a prime's exposure. The reason this state of affairs exists is that longer SOAs allow both for greater expectancy and for greater decay of activation. The only way we know to isolate the effects of expectancy and activational decay in a compelling way is to use a design such as the one Neely (1977b) developed to have the effects of expectancy and relatedness operate in concert or in opposition to one another (cf. Jacoby, 1991). However, such a design requires that only a small number of different category-name primes be used so that the participant can keep track of which expectancies (i.e., in-concert or oppositional) to generate to each prime. This in turn requires that primes be repeated many times during the experiment. Thus, if the present finding that immediate prime repetition reduces semantic priming for primes not previously seen in the experiment does not generalize to the situation in which semantic priming is measured for primes that have previously been seen multiple times in the experiment, the Neely (1977b) paradigm could not be used to determine whether immediate repetition of a prime reduces semantic priming by affecting expectancy or activational decay. If that were the case, the issue may not be resolvable with currently available priming methodologies.

Conclusion

The present results make two important points, one methodological and one theoretical. The methodological point is that before concluding for any particular situation that priming effects observed at SOAs in the 200–400-ms range are not based on expectancy and hence are automatic, one must manipulate RP and show that it has no effect on the priming that is observed (cf. Stolz & Neely, 1995). The more important and novel theoretical point is that prime repetition does not affect the initiation of spreading activation. Hence, the reduction in semantic priming from prime repetition cannot be taken as evidence against the claim that spreading activation is automatic.

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Appendix

Critical Primes and Targets

Prime type: Unrelated	Prime type: Related	Target	Prime type: Unrelated	Prime type: Related	Target
1. believed	election	vote	41. project	success	failure
2. museum	debate	argue	42. minimal	acquire	get
3. step	army	navy	43. king	pain	hurt
4. cope	tale	story	44. south	job	work
5. inform	circus	clown	45. assigned	negative	positive
6. vexed	stump	tree	46. salads	pigeon	bird
7. units	touch	feel	47. aside	thick	thin
8. clear	front	back	48. art	top	bottom
9. annual	remain	stay	49. out	him	her
10. tilt	tuna	fish	50. lighter	compass	direction
11. wanted	future	past	51. index	watch	time
12. harvest	dentist	teeth	52. miles	read	book
13. bliss	marsh	swamp	53. rice	weak	strong
14. cost	full	empty	54. goods	alive	dead
15. bind	vine	grape	55. piers	scent	smell
16. dome	fuel	gas	56. atop	comb	brush
17. aid	hot	cold	57. key	add	subtract
18. toll	mare	horse	58. half	car	auto
19. hardy	reply	answer	59. piece	wrong	right
20. same	day	night	60. wear	cash	money
21. faulty	drawer	dresser	61. normal	quiver	shake
22. use	few	many	62. hobby	bread	butter
23. hence	minor	major	63. work	aunt	uncle
24. main	easy	hard	64. value	icing	cake
25. affect	lumber	wood	65. calm	film	movie
26. ticked	algebra	math	66. ribbon	bridge	water
27. trend	flight	airplane	67. honest	helium	balloon
28. wave	salt	pepper	68. tense	frame	picture
29. chimney	despise	hate	69. window	digit	number
30. tee	jog	run	70. try	ape	monkey
31. firm	deep	shallow	71. tar	rob	steal
32. bed	ask	question	72. concise	silence	quiet
33. risk	seat	chair	73. player	tavern	bar
34. know	last	first	74. tenant	scream	yell
35. hose	chef	cook	75. float	honey	sweet
36. gnome	blouse	shirt	76. cabin	bride	groom
37. yell	acre	land	77. painting	symphony	orchestra
38. dough	globe	world	78. joy	web	spider
39. chisel	petals	flowers	79. rip	hen	chicken
40. titan	hound	dog	80. northeast	reprimand	punishment

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