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The positivity proportion effect: A list context effect in masked affective priming

KARL CHRISTOPH KLAUER, JAN MIERKE, and JOCHEN MUSCH
Rheinische Friedrich-Wilhelms-Universität Bonn, Bonn, Germany

In the evaluative decision task, participants decide whether target words denote something positive or negative. Positive and negative prime words are known to engender so-called affective priming effects in this task. Primes were sandwich masked, and the proportion of positive to negative target words was manipulated. In Experiment 1, prime valence and positivity proportion interacted, so that primes of the less frequently presented target valence caused larger priming effects. Experiment 2 rendered an explanation of this interaction in terms of response bias unlikely, Experiment 3 ruled out a peripheral locus of the effect, and Experiment 4 ruled out an account in terms of stimulus repetition. The effect is explained by means of an attentional bias favoring the rare kind of valence.

List context effects are reliably found in priming and Stroop-like tasks. For example, as the proportion of associatively related prime–target pairs becomes larger, semantic priming effects often are increased (Neely, 1991). Similarly, as the proportion of congruent distractor–target pairs increases, Stroop-like effects of congruence and incongruence become more pronounced (e.g., Logan & Zbrodoff, 1979). Proportion effects of this kind are often argued to reflect strategic, voluntarily controlled processing. In the context of semantic priming, for example, the relatedness proportion effect is restricted to conditions with relatively long (over 300-msec) prime–target stimulus onset asynchronies (SOAs; Hutchison, Neely, & Johnson, 2001; Neely, 1991), suggesting as one possibility that participants use the prime identity to predict potential targets when the relatedness proportion is high. In Stroop-like tasks, congruency proportion effects occur at short SOAs and even with simultaneous presentation of a distractor and a target. These effects do not occur when distractors are rendered difficult to detect by masking (Cheesman & Merikle, 1986; Musch, Klauer, & Mierke, 2002). This suggests that congruency proportion effects result from strategic shifts of the weight given to distractor information (Logan & Zbrodoff, 1979) in an information integration process in which response-related information from distractors and targets is aggregated. For the sake of brevity, we will refer to both distractors (representing the task-irrelevant features in Stroop tasks) and primes (representing the task-irrelevant stimuli in priming paradigms) as *primes* in this article.

List Context Effects in Masked Priming?

At the rebirth of modern research interest in the cognitive effects of masked visual stimuli, it was assumed that the effects of masked primes were free of strategic and contextual influences (Marcel, 1983). Recent research on priming by visible primes has yielded growing evidence for subtle influences of context on priming effects (Besner, 2001; Besner & Stolz, 1999a, 1999b; Smith, Besner, & Miyoshi, 1994). A natural further development for the masked priming domain has therefore been to raise the question that is also addressed by the present experiments: What are the extent and nature of context effects in masked priming?

As just has been explained, one cause of list context effects is strategic processing of prime information of a kind that is typically eliminated when primes are rendered difficult to see, through appropriate masks. Other possibilities are that altering the list composition changes which stimulus features are relevant for the participants' tasks or that it changes participants' subjective construal of their task. According to Holender (1992), a necessary condition for Stroop-like congruity effects is an overlap between the ensemble of task-relevant attributes of the target stimuli or the required responses, on the one hand, and the attributes of the irrelevant primes, on the other. The overlap endows irrelevant primes with the power to prime a response from the set of responses, either the same response as that required by the target or a different one, thereby facilitating or inhibiting, respectively, the appropriate response. Manipulations of list context that change participants' task set may, therefore, change the pattern of Stroop effects.

For example, suppose as a thought experiment that the task is to discriminate animal names from various other words. If the other words always refer to plants, the task will likely be construed as one of discriminating animals from plants. If, in another list context condition, the other words always refer to inanimate objects, the task will

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probably be construed as one of making *animate* versus *inanimate* decisions. When the task is seen as one of discriminating animate from inanimate objects, strong effects of primes denoting inanimate objects are expected on the basis of the argument above, because inanimate objects are directly related to one of the response options, as construed by participants. If, in contrast, the task is seen as one of discriminating animal names from plant names, reduced effects of such primes are expected, because inanimate objects are not directly related to either response option. Similarly, opposite effects of distracting plant names can be expected to occur in both list context conditions.

As an empirical example, consider a study by Ferrand and Grainger (1996) in which the lexical decision task was used. Different types of nonword targets (pseudohomophones, orthographically regular nonwords, and orthographically irregular nonwords) were mixed with the word targets and gave rise to different patterns of priming effects by homophones. As was argued by Ferrand and Grainger, different kinds of target features were adopted as the basis for responding in the lexical decision task in the different list contexts, and priming effects depended on whether these response criteria were "sensitive to the informational overlap between prime and target" (p. 518). Interestingly, the results by Ferrand and Grainger were obtained even though the primes were rendered difficult to see by appropriate masks, and thus, their results provide one of the rare examples of list context effects in masked priming. Context effects mediated by changes in the participants' task set may thus extend to the case of masked primes that are rendered difficult to see. Similarly, Dehaene et al. (1998) have proposed that congruity effects in masked Stroop-like priming tasks are obtained because participants unconsciously apply the task set for target processing to the prime words, and Greenwald, Abrams, Naccache, and Dehaene (2003) have recently provided another demonstration that priming effects in masked priming can change as a result of top-down induced changes in participants' task set.

Manipulations of list context can be quantitative or qualitative. We refer to a manipulation of list context as quantitative if the same classes of primes, targets, and prime–target pairs are sampled under all conditions, but in different proportions. Examples are the already described relatedness proportion effects and congruency proportion effects. In contrast, a manipulation is qualitative if it changes the classes of primes, targets, or prime–target pairs that are represented in the list (e.g., Ferrand & Grainger, 1996; Forster & Veres, 1998; Greenwald et al., 2003; McKoon & Ratcliff, 1995). Quantitative manipulations are a priori less likely than qualitative manipulations to alter which stimulus features are objectively relevant for the participants' task or to change participants' subjective construal of their task. In fact, there are few reports of effects of quantitative manipulations of list context with masked primes. To the contrary, quantitative list context effects that occur when primes are visible are usually eliminated when primes are rendered more difficult to see through masking (Cheesman & Merikle, 1986; Forster, 1998; Greenwald, Draine, & Abrams, 1996; Musch et al., 2002). Exceptions to this rule have, however, recently been provided by Bodner and Masson, who found effects of the proportion of repetition trials on repetition priming in lexical decisions under certain conditions (Bodner & Masson, 2001) as well as relatedness proportion effects in masked semantic priming (Bodner & Masson, 2003).

Another priming task in which masked priming effects are found is affective priming. Affective priming (Klauer & Musch, 2003) occurs when the processing of an evaluatively polarized target word (e.g., *love*) is facilitated—that is, proceeds faster and/or more accurately when it is preceded by an evaluatively consistent prime word (e.g., *sunshine*) than by an evaluatively inconsistent one (e.g., *death*). Affective priming is found even when primes are rendered very difficult to see by appropriate masks (e.g., Draine & Greenwald, 1998). In the evaluative decision task, participants are asked to classify target words as positive or negative as quickly as possible. In this task, as in other Stroop-like tasks, a congruency proportion effect occurs even with simultaneous presentation of (visible) prime and target (Klauer, Rossnagel, & Musch, 1997).

The point of departure of the present experiments was an experiment by Musch et al. (2002), in which the findings by Cheesman and Merikle (1986) were replicated: In Experiment 5 by Musch et al., a congruency proportion effect was found with visible primes but was eliminated when masked primes were used, although even masked primes engendered a significant priming effect. The experiment employed an evaluative decision task in an affective priming paradigm.

In the present experiments, instead of congruency proportion, we varied the proportion of positive targets, relative to negative ones, in participants' lists. That is, different groups of participants saw lists that contained more positive than negative target words (high positivity proportion [PP]), equal proportions of positive and negative target words (medium PP), or more negative than positive target words (low PP). Equal proportions of positive, neutral, and negative primes preceded both positive and negative targets in each group. The same masking procedures were used as those that eliminated the congruency proportion effect in the above-mentioned study by Musch et al. (2002). The question was whether PP would nevertheless interact with the priming effects associated with positive and negative masked primes. The demonstration of such a PP effect and its explanation are the topics of the present paper. Like the effects reported by Bodner and Masson (2001, 2003), the PP effect explored in the present paper is an effect of a quantitative manipulation of list context in masked priming. Such effects present an interesting theoretical challenge, because it seems difficult to explain them a priori in terms of context-induced changes in the stimulus features that are adopted as a basis for prime and target processing as just explained.

Unidimensionality Versus Bidimensionality of Evaluations

Another goal of the present research was to contribute to the debate on the unidimensionality versus bidimensionality of evaluations (e.g., Cacioppo & Berntson, 1994; Russell & Carroll, 1999). If evaluation is assumed to be unidimensional, the relative positivity or negativity of a stimulus refers to its position on a single evaluative dimension, with positive on one end and negative on the other. The midpoint of the dimension is neutral. A stimulus may be either positive or negative. It cannot be both. A stimulus high in positivity is by definition low in negativity, and a stimulus high in negativity is by definition low in positivity. Alternatively, positivity and negativity might be different dimensions that can, in principle, be dissociated and need not be correlated perfectly negatively. Most of the evidence against unidimensionality comes from the use of unipolar, rather than bipolar, rating scales (Kaplan, 1972). In unipolar rating scales, positivity and negativity of given objects are rated on separate scales. The most compelling cases against unidimensionality are based on dissociations of ratings of positive and negative evaluations of given objects as a function of some manipulated or sampled variable (e.g., Goldstein & Strube, 1994; Katz & Hass, 1988).

We reasoned that if an experimental manipulation differentially affected priming effects by positive and negative primes in the affective priming paradigm, a dissociation of positive and negative evaluations would be implied. As will be elaborated below, finding a dissociation on the basis of a behavioral priming measure is desirable, because the use of rating scales and self-report measures has been critically discussed in the literature on the dimensionality of evaluations. It was our hope that a manipulation of PP would produce the desired dissociation.

Signal Detection Model

In the present experiments, the response window technique proposed by Greenwald et al. (1996) was used. The response window technique pushes participants toward responding within a narrow time frame after the presentation of the target. As Greenwald et al. (1996; Draine & Greenwald, 1998; cf. Klinger, Burton, & Pitts, 2000) pointed out, it has the major benefit of controlling for speed–accuracy tradeoff problems by reducing variance in the response latencies, thereby avoiding the dilution of the priming effect between response latency and accuracy. This typically leads to a large increase in the effect size of priming effects. The dependent variable with this procedure is the percentage of correct responses.

A signal detection model was used to analyze the data, in order to correct for the fact that PP is likely to shift the response criterion so that responses are biased toward the more frequent target valence (Macmillan & Creelman, 1991, chap. 3) and in order to map the priming effects for positive and negative primes on a common scale. Signal detection models are based on statistical decision theory. Statistical decision theory describes a decision maker who must choose between two or more options on the basis of evidence that is to some extent ambiguous. When there are only two alternatives, as in the present evaluative decision task, the decision process can be simplified, regardless of the complexity and dimensionality of the original evidence (Wickens & Hirshman, 2000). The evidence can be mapped onto a single strength-of-evidence axis on which the choice is made. For each alternative, there is a distribution of values on this axis. Large values favor one alternative, whereas small values favor the other alternative. The decision maker decides between the alternatives by applying a single decision criterion to this axis.

According to Wickens and Hirshman (2000), the classical signal detection model is overparameterized. Most important, the numerical scale underlying the strength-of-evidence axis is not determined. The placement of the two distributions and the criterion can be determined relative to each other, but not their absolute location. Assigning numbers to the axis can be accomplished on the basis of different scaling assumptions. When results from separate signal detection analyses of several conditions are to be compared, it is important that numerical values be assigned in a consistent manner so that the parameters from different signal detection analyses are mapped onto one common strength-of-evidence axis and can be compared.

Because parameter values from different PP conditions were to be compared in the present experiment, the response frequencies from the priming trials were analyzed jointly by means of the signal detection model shown in Figure 1. It is assumed that negative and positive targets in the baseline conditions with neutral primes are mapped onto the strength-of-evidence axis with values that are distributed normally with potentially different means and standard deviations. These distributions are shown as shaded areas in Figure 1. The distribution of negative targets was given a zero mean. The difference between the means of the two distributions (d) is the overall discriminability of positive from negative targets. Targets that evoke values on the strength-of-evidence axis to the left of the response criterion (c) lead to the (and ultimately, to the response) *negative* decision, and targets to the right of it lead to the *positive* decision. Separate standard deviations (s_p and s_n) were estimated for positive and negative target words, to allow for possible distributional differences between these two sets of stimuli. Note, however, that there is little statistical information on the standard deviations in the response frequency data, leading to large errors of estimation and, sometimes, unrealistically small or large estimates of the standard deviations when their ratio is free to vary between zero and infinitely large values. For this reason, the standard deviations of the distributions of positive and negative targets were allowed to differ only by as much as a factor of four. To fix the scale, their geometric mean was constrained to equal one.

The effects of primes are modeled as shifts of the target distributions. Negative primes are assumed to induce a small shift of each target to the left of average size (t_n),

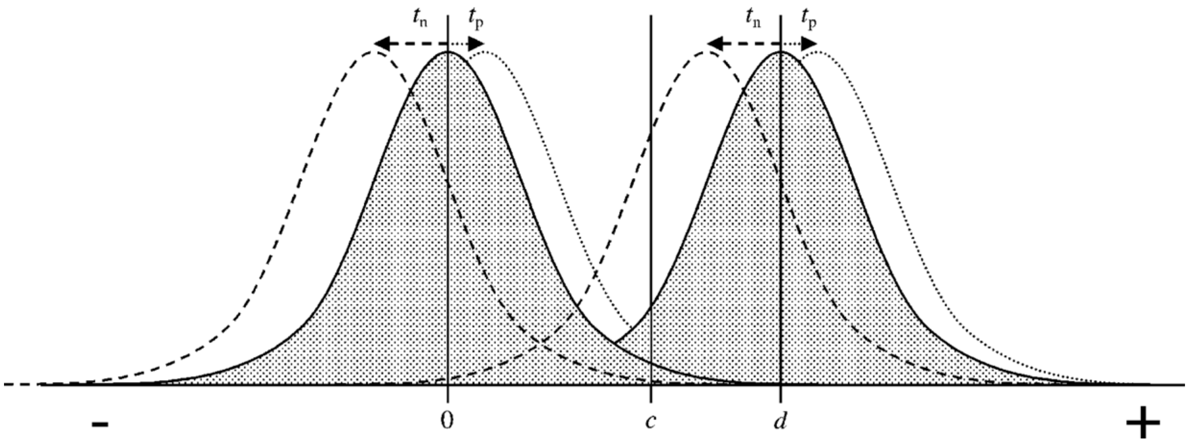

Figure 1. Graphical representation of the signal detection model and its parameters. The shaded distributions are the distributions of negative (left distribution) and positive (right distribution) targets preceded by neutral primes.

thereby making a negative response more likely. This shift is measured relative to the distribution of targets preceded by neutral primes. Analogously, positive primes are assumed to produce a small shift of each target to the right of average size (t_p), making a positive response more likely. The effects of positive and negative primes are thereby measured relative to the baseline condition of neutral primes. A mathematically equivalent signal detection model is obtained if the effects of positive and negative primes are modeled as shifts of the response criterion to the left and to the right, respectively, rather than as shifts of the targets' values on the strength-of-evidence axis (Wickens & Hirshman, 2000). This means that the model does not incorporate substantive assumptions about the locus of priming effects: Whether priming affects strength-of-evidence distributions or response strategies cannot be disentangled on the basis of the model per se but must be decided on the basis of other kinds of evidence.

The five model parameters (d, t_n, t_p, c, and the ratio parameter s_p/s_n) were estimated from each participant's data, using the maximum likelihood method. Each participant's data matrix consisted of six independent response frequencies. These were the frequencies of correct responses for each of the six different kinds of priming trials defined by crossing target valence (i.e., positive vs. negative) and prime valence (i.e., negative, neutral, or positive). Parameter estimates were determined on the basis of these data, using an iterative search algorithm that maximized the likelihood of the observed data.[1]

Four experiments are reported in which we used the signal detection model to examine the effect of list context on masked affective priming. Experiment 1 demonstrated that PP affected the priming effects associated with positive and negative masked primes differently. Experiments 2 and 3 investigated whether this surprising list context effect was due to peripheral response-related processes, and Experiment 4 evaluated whether the PP effect went back to a confounding with the number of stimulus repetitions.

EXPERIMENT 1

In Experiment 1, primes were masked, and different participants saw lists with low, medium, or high PP. Upon completion of the priming trials, the participants attempted to make evaluative decisions on the masked primes for a direct measure of prime visibility.

Method

The participants underwent two separate phases: a priming phase and a test of prime visibility. For the priming trials, they were to decide whether the target words were positive or negative. For the test of prime visibility, the participants were asked to decide whether the masked prime words were positive or negative. Assignment of positive and negative words to the response keys was counterbalanced so that half of the participants in each PP condition responded to positive (negative) words with a keypress initiated by their dominant (nondominant) hand, and the remainder responded to positive (negative) words with their nondominant (dominant) hand.

Participants. The participants were 60 University of Bonn students with different majors and nonstudent volunteers of a similar age range recruited by the experimenters. The students either received partial course credit or were paid DM 10 (approximately $6 at the time) for their participation. Nonstudent volunteers participated in exchange for detailed individual feedback on their results. All the participants were native speakers of German and had normal or corrected-to-normal vision. The participants were randomly assigned to one of three equal-sized groups with different PPs.

Materials and list construction. Seventy strongly positive and 70 strongly negative adjectives were selected from a pool of adjectives with unambiguously polarized valence, used in previous affective priming experiments (e.g., Klauer et al., 1997). Each word had between three and nine letters. For each participant, each block of 48 prime–target pairs was newly constructed by drawing a new random sample of positive and negative words to be used as primes and targets. The sampling of stimulus words was without replacement for any given block. Thus, a specific word occurred, at most, once in a given block. Blocks with low PP comprised 15 positive and 33 negative target words; with medium PP, there were 24 positive and 24 negative targets; with high PP, there were 33 positive and 15 negative targets. Primes were sampled from the sets of positive adjectives, negative adjectives, and neutral letter strings. Neutral primes were seven-letter strings of randomly sampled consonants (the letters *q*, *x*, *y*, and *z* were excluded because they rarely occur in the German language). Consonant strings were selected because they merged well with the letter strings that were used as masks, as will be described below. The positive, negative, and neutral primes preceded both positive and negative targets in equal proportions. The order of pairs was randomized within each block. All the blocks began with four additional warm-up trials based on the same materials. The warm-up trials were excluded from the analyses.

Response window procedure and presentation parameters. The participants had a window of 133-msec duration, initially centered at 400 msec after target onset, within which they were to respond to the target stimulus. To tailor the window center to each participant's performance, an adaptive procedure modeled closely after Greenwald et al. (1996) was used, in which the window center was adjusted depending on the participant's performance. The details of the adaptive procedure are described in Musch and Klauer (2001; cf. Musch et al., 2002).

The sequence of events on priming trials was as follows: forward mask for 300 msec, prime for 57 msec, backward mask for 14 msec, then target. The masks were letter strings composed of 13 randomly sampled consonants. The masks, primes, and targets were presented in black on a light gray background, centered on the middle of the screen. The primes were extended to a length of 13 letters by adding random consonants to the left and to the right. For example, the prime word *brave* thereby became *gkvfbravemltr*. The 71-msec interval between prime onset and target onset defined the SOA of the priming task. Onset and offset of the response window, as well as feedback on whether the response had occurred within the response window, were signaled by changes of the target color (see Musch & Klauer, 2001). Following the response, target offset occurred after 300 msec. The next trial was then initiated after an interval of 150 msec. The participants completed 10 blocks of 48 experimental trials, preceded by 3–10 practice blocks, as determined by the adaptive procedure.

Direct measure of prime visibility. For the direct measure, blocks of 48 trials with PP 50% and without neutral primes were presented. In all other respects, the trials were identical to those of the priming phase. The participants were instructed to decide whether the prime words were positive or negative taking as much time as they needed for each decision. There was no response window for this task, since time pressure decreases the sensitivity of the direct test (Draine & Greenwald, 1998). The participants underwent three practice blocks, followed by four experimental blocks. The first practice block presented the primes without masks and colored in red. The second block added masks. In the third practice block, the prime display reverted to normal black, as used in the priming phase. During practice, the word *falsch* (i.e., *false*) was shown for 300 msec following a wrong response; there was no such feedback in the experimental blocks. After each practice and experimental block, the participants were informed about the proportion of primes correctly classified.

Results and Discussion

Model-based analyses. Responses with latencies below 100 msec and above 1,000 msec were excluded from the analyses, thereby removing 1.6% of the data. The joint signal detection model was then fitted to the frequency data.

The means of the resulting priming parameters (t_n and t_p) are shown in Table 1 as a function of prime valence and PP. It can be seen that the priming effects engendered by negative primes increased as PP increased, whereas the impact of positive primes decreased. The priming parameters were submitted to an analysis of variance (ANOVA) with independent variables of PP and prime valence, with repeated measures on the latter variable. The interaction of PP and prime valence was significant [$F(2,57) = 7.08$, $MS_e = 0.04$, $p < .01$]. Separate analyses showed that the linear trends over PP for both negative and positive primes were significant [$t(57) = 3.36$, $p < .01$, and $t(57) = -2.18$, $p = .03$, respectively], but in opposite directions. Neither quadratic trend was significant (ts < 1).

The interaction of PP and prime valence is the major result of Experiment 1. The impact of primes of a given valence was larger when that valence was rare among targets. Thus, PP had opposite effects on priming effects caused by positive and negative primes. The interaction, termed the PP effect, has since been replicated in a further experiment in our laboratory. It is an instance of an effect of a quantitative manipulation of list context in a masked priming paradigm.

Table 1 also shows the mean values of the response criterion, the overall discriminability, and the standard deviation of the distribution of positive targets, relative to that of negative targets. These dependent variables were submitted to separate analyses of variance with independent variable PP. As was expected, there was a significant effect of PP on the response criterion c [$F(2,57) = 18.21$, $MS_e = 0.60$, $p < .01$], reflecting the fact that the responses were biased toward the more frequent kind of target valence. There was no significant effect of PP on the overall discriminability (d) of positive from negative targets

Table 1
Means and Standard Deviations of Parameter Estimates in Experiment 1 as a Function of Positivity Proportion

	Positivity Proportion					
	Low		Medium		High	
Parameter	*M*	*SD*	*M*	*SD*	*M*	*SD*
Priming effect by negative primes (t_n)	0.00	0.19	0.07	0.21	0.23	0.25
Priming effect by positive primes (t_p)	0.18	0.13	0.10	0.15	0.08	0.17
Response criterion (c)	1.51	1.08	0.64	0.57	0.04	0.55
Overall discriminability (d)	1.59	1.17	1.30	0.91	1.28	1.16
SD of positive relative to negative targets (s_p/s_n)	1.94	1.54	1.39	1.59	0.91	1.15

($F < 1$). Finally, a nonsignificant tendency [$F(2,57) = 2.94$, $MS_e = 0.32$, $p = .06$] indicated that s_p/s_n tended to decrease as PP increased. The standard deviation of the distribution of positive targets was larger overall than that of negative targets, justifying our choice of a model that allowed for the separate estimation of these two parameters. These results are further discussed in the General Discussion section. Accuracy data and response latencies are presented in the Appendix.

The direct test of prime visibility revealed a mean d' of 0.08 ($SD = 0.26$) for participants' ability to discriminate the masked primes' valence. The participants' performance only slightly, yet significantly, exceeded the chance baseline of $d' = 0$ [$t(59) = 2.43$, $SE = 0.03$, $p = .02$].[2]

Alternative accounts. There are a number of potential explanations of the PP effect. In the following sections, we will consider an account in terms of an asymmetry in costs and benefits of priming, an account in terms of response bias, an account in terms of satiation of motor programs associated with the different keypresses, and an account in terms of target repetitions. We will try to rule out these accounts one by one through appropriate analyses or experiments, leaving the interesting possibility that the interaction was due to subtle changes in the participants' task set, as will be elaborated later.

Turning to the first hypothesis, note that priming and Stroop effects often comprise benefits, or facilitation of the correct response if the prime and the target are congruent (i.e., both positive or both negative, in the present case), and costs, or inhibition of the correct response if the prime and the target are incongruent (i.e., one of them is positive and the other one negative). If the overall amount of inhibition exceeds that of facilitation, the impact of primes of the less frequent valence might appear larger, because that kind of prime is paired with a larger proportion of incongruent targets (66%) than are the primes of the more frequent valence (33%). For example, when there are many positive targets, negative primes precede a larger proportion of incongruent (i.e., positive) targets than do positive primes, for which negative targets are incongruent. Note that the signal detection model assumes equally large amounts of inhibition and facilitation—that is, priming effects are assumed to be equally large on both congruent and incongruent targets on the underlying strength-of-evidence axis. A violation of this assumption should be reflected in a poor model fit. For each participant, we computed the χ^2-distributed log-likelihood ratio statistic (G^2) with one degree of freedom, to assess model fit. The sum of these values over the 60 participants evaluates the overall goodness of fit of the model for the entire group of participants and is asymptotically distributed as χ^2 with 60 degrees of freedom. In Experiment 1, $\chi^2(60) = 75.15$, $p = .09$. Note, however, that this test is not specifically focused upon testing the hypothesis that the amount of inhibition exceeds the amount of facilitation, because any kind of model violation increases the χ^2 value and decreases the p value of the model fit. For example, if for a given participant the amount of facilitation exceeds that of inhibition, rather than vice versa, model fit would also be decreased as a consequence.

For a more focused test, we fitted a modified version of the signal detection model in which an asymmetry between costs and benefits by incongruent and congruent primes, respectively, was allowed for. For this purpose, a new parameter (i) was introduced. The priming effect parameters t_n and t_p were multiplied by i in the equations for incongruent prime–target pairs and by $1/i$ in the equations for congruent prime–target pairs. A value of this parameter of one means that facilitatory and inhibitory components of priming are of equal size; values larger than one indicate a predominance of inhibition; and values smaller than one indicate a predominance of facilitation. This model is saturated—that is, there are no degrees of freedom for a test of model fit. Applying this model to the present data, a t test of the hypothesis $i = 1$ also failed to reveal significant differences between inhibitory and facilitatory effects [$t(59) = 1.49$, $SE = 3.89$, $p = .14$]. Of importance, the priming parameters of the modified model assess the effects of positive and negative primes corrected for any differences in the size of inhibition and facilitation, and the interaction of PP and prime valence remained in full force in this analysis [$F(2,57) = 6.78$, $MS_e = 0.04$, $p < .01$].

Turning to the second potential explanation in terms of response bias, note that, as was expected, PP exerted a pronounced effect on the response criterion. The participants were biased toward the more frequent kind of target valence. The signal detection model explicitly corrects for response bias and appeared to fit the data satisfactorily. Nevertheless, we sought additional experimental evidence to rule out the hypothesis that shifts in response bias are sufficient to cause the observed interaction of PP and prime valence. For this purpose, we attempted to produce shifts in response bias without producing the interaction between PP and prime valence.

Changing the proportion of two kinds of targets that participants are to discriminate is an accepted method of manipulating response criteria (e.g., Macmillan & Creelman, 1991, chap. 3). Another method for changing response criteria is to manipulate the payoff schedule.

EXPERIMENT 2

In Experiment 2, different groups of participants received different monetary rewards for correct responses to positive versus negative targets. PP was 50% in each group. Members of a group with a low positive payoff received 4 Pfennig (approximately 2 cents) for each correct response to a positive target and 8 Pfennig (approximately 4 cents) for each correct response to a negative target; a group with medium payoff received 6 Pfennig (approximately 3 cents) for each correct response; a group with high positive payoff received 8 Pfennig for positive targets and 4 Pfennig for negative targets.

We expected the payoff schedule to affect the response criterion (e.g., Macmillan & Creelman, 1991, chap. 3), as PP did in Experiment 1. The purpose of manipulating the response criterion was to test whether a shift in response criterion would be sufficient to induce shifts in priming effects exerted by positive and negative primes, as had been found in Experiment 1. If an interaction of prime valence and payoff schedule were to be obtained that paralleled the interaction between prime valence and PP observed in Experiment 1, response bias would offer an explanation of the interaction of PP and prime valence. If on the other hand, no such interaction were to emerge, shifts in response bias would not be sufficient to cause the effect; this was our hypothesis.

Method

The method and procedures were closely parallel to those employed in Experiment 1. To provide frequent feedback about the accumulated reward between blocks, trials were grouped in blocks of 24 trials instead of 48 trials, however, and consequently, there were twice as many blocks as in Experiment 1. As has already been mentioned, PP was fixed at 50%. The procedures for the direct measure of prime visibility were the same as those in Experiment 1.

Participants. The participants were 86 University of Bonn students with different majors and nonstudent volunteers of a similar age range recruited by the experimenters. They received the money accumulated according to their payoff schedule for their participation. All the participants were native speakers of German and had normal or corrected-to-normal vision. The participants were randomly assigned to one of three approximately equal-sized groups with different payoff schedules.

Procedure. After the practice trials, the participants were informed that they were to receive a few Pfennig for each correct response that occurred within the response window during the experimental trials. They were informed of their payoff schedule and that they could earn a maximum of DM 28.80 (approximately $15). The amount already earned was always shown on the screen a few lines above center. In addition, after each block, the participants were told how much they had earned up to that time in response to positive targets and how much in response to negative targets.

Results and Discussion

Throughout Experiment 2, a significance level (α) of 10% was adopted. Increasing the α level increases the probability of detecting an interaction of prime valence and PP if it is there (i.e., it leads to an increase in test power). In the present context, increasing the significance level is a conservative procedure, because it decreases the likelihood that our hypothesis of an absence of such an interaction can be maintained (Erdfelder, Faul, & Buchner, 1996). Means of overall rewards in the groups with low positive payoff, medium payoff, and high positive payoff were DM 9.96 (*SD* = 3.52), DM 10.30 (*SD* = 2.68), and DM 11.41 (*SD* = 3.11), respectively. There was no effect of payoff schedule on overall reward in an ANOVA with an independent variable of payoff schedule [$F(2,83) = 1.72$, $MS_e = 9.68$, $p = .18$]. Responses with latencies below 100 msec and above 1,000 msec were excluded from the analyses, thereby removing 1.8% of the data. The overall goodness-of-fit test of the signal detection model yielded a value of $\chi^2(86) = 95.48$, indicating a satisfactory model fit ($p = .23$).

The priming parameters are shown in Table 2, along with the response criterion, the overall discriminability, and the standard deviation of the distribution of positive targets, relative to that of negative targets, as a function of payoff schedule. It can be seen that there is little evidence for effects of payoff schedule on the priming parameters. An ANOVA with independent variables of payoff schedule and prime valence, with repeated measures on the latter variable, revealed no significant effects or interactions in the priming parameters (all Fs < 1), but the overall priming effect itself was significant [$F(1,83) = 65.85$, $MS_e = 0.02$, $p < .01$].

The other model parameters were also submitted to separate ANOVAs with an independent variable of payoff schedule. As was expected, there was a significant effect of payoff schedule on the response criterion [$F(2,83) = 5.16$, $MS_e = 0.27$, $p < .01$], reflecting that responses were biased in favor of the more valuable target valence (see Table 2). There was no significant effect of payoff schedule on the overall discriminability of positive from negative targets ($F < 1$) or on the standard deviation of positive targets relative to that of negative targets ($F < 1$). Accuracy data and response latencies are presented in the Appendix.

The direct test of prime visibility revealed a mean d' of 0.12 (*SD* = 0.21) for participants' ability to discriminate the masked primes' valence. The participants' performance only slightly, yet significantly, exceeded the chance baseline of $d' = 0$ [$t(85) = 5.17$, $SE = 0.02$, $p < .01$].

In Experiment 2, altering the payoff schedule for positive, relative to negative, targets had the expected pronounced effect on the response criterion. Nevertheless, there was no effect of payoff on the priming effects, let alone an interaction of prime valence and payoff schedule. The effect size of the interaction observed in Exper-

Table 2
Means and Standard Deviations of Parameter Estimates in Experiment 2 as a Function of Payoff Schedule

	Payoff Schedule					
	Low Positive		Medium		High Positive	
Parameter	*M*	*SD*	*M*	*SD*	*M*	*SD*
Priming effect by negative primes (t_n)	0.10	0.20	0.09	0.16	0.13	0.19
Priming effect by positive primes (t_p)	0.07	0.15	0.10	0.21	0.08	0.19
Response criterion (c)	0.61	0.56	0.40	0.47	0.17	0.53
Overall discriminability (d)	0.80	0.59	0.76	0.47	0.85	0.63
SD of positive relative to negative targets (s_p/s_n)	1.20	1.16	1.24	1.44	1.32	1.43

iment 1 amounts to *f* = .49 (Cohen, 1988, chap. 8). The power for detecting an interaction of this size in the present experiment was .99. Even an interaction of half this effect size would still have been detected with a probability of .62. The interaction observed in Experiment 1 thus does not appear to be an artifact or consequence of shifts in response criterion. However, another possibility, appealing to relatively peripheral response processes, is that the higher frequency of one kind of keypress leads to a satiation of associated motor programs, so that it becomes increasingly more difficult to initiate that keypress relative to the less frequent kind of keypress. If so, diminished effects of the primes with the more frequent valence might simply reflect the fact that the associated response has generally become more difficult to initiate, due to its frequent prior execution. Although it might be argued that such an effect is difficult to reconcile with the observation that response bias favored the more frequent response category, rather than the less frequent one, in Experiment 1, we sought additional experimental evidence to rule out a motoric locus of the PP effect.

EXPERIMENT 3

In Experiment 3, PP was manipulated while the distribution of left versus right keypresses was kept balanced. For this purpose, in Experiment 3A, the response mapping was switched every 8 trials, so that the key formerly associated with negative targets was assigned to positive targets and vice versa. In Experiment 3B, the response mapping was kept constant, but additional trials were introduced in which the word targets *left* and *right* were presented and the participants were asked to press the left or the right response key in accordance with the word meaning. The distribution of these filler trials was such that they removed the skew in the distribution of the two response keys that was otherwise present in conditions with PP not equal to 50%.

If the observed effects of PP are caused by the confounded factor response distribution, effects of PP should not be observed in Experiments 3A and 3B. If on the other hand, the effects of PP are localized at a more central stage of processing, such as categorizing stimuli as positive versus negative, they should occur irrespective of response distribution; this was our hypothesis.

Method

The method closely followed that in Experiment 1. In both Experiments 3A and 3B, there were two groups, one of which received a low PP and the other a high PP.

Participants. The participants were 79 University of Bonn students with different majors, 40 in Experiment 3A and 39 in Experiment 3B. The students either received partial course credit or were paid DM 10 (approximately $6 at the time) for their participation. All the participants were native speakers of German and had normal or corrected-to-normal vision. In each experiment, the participants were randomly assigned to one of two groups of 19 or 20 participants with different PPs.

Experiment 3A. In Experiment 3A, each block of 48 priming trials consisted of 24 trials in which the *positive* response was mapped onto the left response key and the *negative* response onto the right key and of 24 trials in which this response mapping was reversed. In the group with low PP, 15 of each set of 24 trials presented negative targets, and 9 positive targets; in the group with high PP, there were 15 positive and 9 negative targets.

The 48 trials of a given block were administered in runs of 8 trials; the response mapping switched every 8 trials. The participants were informed of this fact, and in addition, the mapping governing each individual trial was shown on the screen. For this purpose, the *positive* response was symbolized by a *thumbs-up* icon, the *negative* response by a *thumbs-down* icon. One of these icons was positioned left of the screen center, the other one right of the screen center, thereby indicating whether its associated response category was currently mapped onto the left or the right response key, respectively. Note that over the 48 trials of a given block, the evaluative decision task required the participants to press the left key equally often as the right key, irrespective of PP. For the direct measure of prime visibility, the response mapping was kept fixed throughout all the trials.

Experiment 3B. In Experiment 3B, blocks of 48 trials were constructed in the same manner as in Experiment 1. These were augmented by 24 additional trials in which instead of positive or negative adjectives, the target words *left* or *right* appeared. The participants were instructed to press the left response key upon appearance of the word *left* and the right key upon appearance of the word *right* and to perform evaluative decisions for all other target words. The additional trials guaranteed a balanced distribution of (required) responses. For example, in the group with low PP, there were 33 trials with a negative target and 15 with a positive target. The additional trials were chosen so that over the entire block of 72 trials, 36 responses of each kind were required. These trials consisted of 3 (21) trials with a *left* target and 21 (3) trials with a *right* target, if the *positive* response was mapped onto the right (left) key and the *negative* response onto the left (right) key.

Like the critical trials with positive or negative targets, these filler trials were preceded by equal proportions of negative, neutral, and positive masked primes. The filler trials do not enter the analyses reported below. We did not obtain the direct measure of prime visibility in Experiment 3B, because the additional trials already considerably lengthened the priming phase. One session of Experiment 3A or 3B required about 50 min.

Results and Discussion

In Experiments 3A and 3B, 3.9% and 1.4%, respectively, of the responses fell outside the interval from 100 to 1,000 msec and were excluded from the analyses. The overall goodness-of-fit test of the signal detection model yielded a satisfactory model fit in Experiment 3B [$\chi^2(39) = 39.07, p = .47$] but indicated significant violations of the model assumptions in Experiment 3A [$\chi^2(40) = 71.45, p < .01$]. An outlier analysis using boxplots was therefore performed for Experiment 3A on the basis of the participants' individual goodness-of-fit statistics and revealed four outliers, whose fit statistics fell outside the group's interquartile range by more than 1.5 times that range. When these 4 participants were excluded from the analysis, overall model fit was satisfactory [$\chi^2(36) = 45.58, p = .13$], indicating that the model violations were concentrated in the outlying participants. The ANOVAs of the model parameters reported below are based on the restricted data set, but the same pattern of results emerged in analyses including all 40 participants.

Table 3 shows the priming effects as a function of prime valence and PP for both experiments. It can be

Table 3
Means and Standard Deviations of Parameter Estimates in Experiment 3 as a Function of Positivity Proportion (PP)

	Experiment 3A				Experiment 3B			
	Low PP		High PP		Low PP		High PP	
Parameter	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Priming effect by negative primes (t_n)	−0.06	0.15	0.10	0.17	0.02	0.16	0.12	0.19
Priming effect by positive primes (t_p)	0.08	0.14	0.05	0.15	0.12	0.17	0.03	0.17
Response criterion (c)	0.74	0.78	0.31	0.38	0.63	0.34	0.15	0.31
Overall discriminability (d)	1.13	0.86	1.12	0.85	0.94	0.50	1.30	0.63
SD of positive relative to negative targets (s_p/s_n)	2.56	1.70	1.63	1.62	1.83	1.63	2.22	1.58

seen that in both experiments, prime valence and PP interacted in the expected direction. The priming parameters for each experiment were submitted to ANOVAs with independent variables of PP and prime valence. The interaction of PP and prime valence was significant in both experiments [3A, $F(1,34) = 5.46$, $MS_e = 0.03$, $p = .03$; 3B, $F(1,37) = 4.79$, $MS_e = 0.04$, $p = .04$]. One-tailed t tests revealed that the impact of negative primes increased significantly with increasing PP [3A, $t(34) = 2.96$, $SE = 0.05$, $p < .01$; 3B, $t(37) = 1.79$, $SE = 0.06$, $p = .04$], whereas the impact of positive primes decreased significantly in Experiment 3B, but not in Experiment 3A [3A, $t(34) = -0.69$, $SE = 0.05$, $p = .25$; 3B, $t(37) = -1.73$, $SE = 0.05$, $p = .046$].[3]

Table 3 also shows the other model parameters. In both experiments, PP exerted a significant effect on the response criterion [in Experiment 3A, $t(34) = -2.10$, $SE = 0.20$, $p = .04$; in Experiment 3B, $t(37) = -4.55$, $SE = 0.11$, $p < .01$]. As in Experiment 1, the participants were biased toward the more frequent target valence. There were no significant effects on the overall discriminability [Experiment 3A, $t(34) = 0.00$, $SE = 0.29$, $p = .99$; Experiment 3B, $t(37) = 1.\ 97$, $SE = 0.18$, $p = .06$], nor were there significant effects on the standard deviations of the distribution of positive targets, relative to that of negative targets [Experiment 3A, $t(34) = -1.68$, $SE = 0.22$, $p = .10$; Experiment 3B, $t(37) = 0.77$, $SE = 0.52$, $p = .44$]. Accuracy data and response latencies are presented in the Appendix.

The direct measure of prime visibility obtained in Experiment 3A revealed a mean d' of 0.12 ($SD = 0.38$) for the ability to discriminate the masked primes' valence. The participants' performance only slightly, yet significantly, exceeded the chance baseline of $d' = 0$ [$t(39) = 2.03$, $SE = 0.06$, $p = .05$].

In Experiments 3A and 3B, prime valence interacted with PP even though the distribution of left and right keypresses required by the task was balanced. This rules out the possibility that satiation of motor processes involved in initiating and executing the keypresses was responsible for the interaction.

These results also shed some light on the processes underlying masked priming effects themselves. Effects of masked primes in Stroop-like tasks tend to increase as a function of the amount of practice with primes when they appeared as targets in previous trials of the same experiment, leading Damian (2001) to suggest that priming involves the automatic activation of practiced stimulus–response mappings. In contrast, Naccache and Dehaene (2001) recently found priming effects by novel primes that were never seen as targets, and Abrams, Klinger, and Greenwald (2002) demonstrated effects of masked primes even when the stimulus–response mapping was reversed between practice and test trials. Similarly, the present Experiment 3A contributes to this debate in that significant effects of masked primes were observed even though the stimulus–response mapping was in fact reversed every eight trials. This makes it difficult to attribute the present priming effects to the automatization of practiced stimulus–response mappings and suggests that a more central stage of categorizing primes (as positive or negative, in the present case) was involved.

EXPERIMENT 4

In the previous experiments, when PP was not equal to 50%, an individual target word of the more frequent valence was about twice as likely to appear in any given block and, thus, to receive repeated practice in being classified as positive or negative across blocks. As has just been explained, effects of masked primes in Stroop-like tasks have been found to increase as a function of the amount of practice with the primes when they appear as targets in previous trials of the same experiment (Abrams & Greenwald, 2000; Damian, 2001). Although such an effect should counteract, rather than cause, the present interaction of PP and prime valence, we sought to demonstrate the PP effect when stimulus repetition was constant across PP conditions.

For this purpose, PP was manipulated as before, but the likelihood of target repetition was kept constant. In addition, for a certain subset of trials—henceforth, called *balanced* trials—the likelihoods of both prime repetition and target repetition were kept constant. A number of modifications of the prime visibility test aimed at increasing the comparability of the priming phase trials and the visibility test trials.

Method

The procedures closely followed those of Experiment 1 unless otherwise noted.

Participants. The participants were 40 University of Bonn students with different majors. The students either received partial course credit or were paid Euro 5 (approximately $5 at the time) for their participation. All the participants were native speakers of German and had normal or corrected-to-normal vision. In each experiment, the participants were randomly assigned to one of two groups of 20 participants with different PPs.

List construction. For each participant, the stimulus pool of 70 positive words was randomly subdivided into two sets of 30 and 40 positive words; the stimulus pool of 70 negative words was analogously randomly split into two subsets of 30 and 40 negative words.

For each participant and each block of 48 trials, the primes and the targets of 30 trials, termed the *balanced* trials, were randomly sampled without replacement from the sets of 30 words; the remaining 18 trials (as well as the 4 additional warm-up trials that preceded each block; cf. the Method section of Experiment 1) were sampled without replacement from the sets of 40 words. Irrespective of the PP group to which a participant had been assigned, the balanced trials realized a PP of 50%. They comprised 15 trials with positive targets and 15 trials with negative targets. As before, these were preceded by equal proportions of positive, neutral, and negative prime words (i.e., five of each prime valence for each target valence). Since sampling was without replacement for each block, an individual word could appear only once in a given block. Note that the probability with which it appeared as a target in any given block was exactly one half (e.g., there were 15 positive targets in balanced trials, which were sampled from a set of 30 positive words).

The remaining 18 experimental trials of each block introduced the differences in PP. The primes and the targets for these trials were sampled from the pools of 40 positive and 40 negative words reserved for the nonbalanced trials. For the low-PP condition, the targets in these trials were always negative; for the high-PP condition, the targets were always positive. The target words were again preceded by equal proportions of positive, neutral, and negative prime words (i.e., 6 of each kind). Each individual word could appear only once in a given block, since sampling was without replacement for any given block. Note that the probability with which an individual target word was sampled to appear in a given block was again exactly one half: For example, in the group with low PP, there were 18 negative targets in the experimental, nonbalanced trials, as well as two negative targets in the four warm-up trials preceding each block, and thus a total of 20 negative target words were drawn from the pool of 40 negative words reserved for nonbalanced trials.

To summarize, the probability that a given target appeared was the same for each individual target, irrespective of PP and valence, and for the balanced trials, this was also true for the likelihood with which a given word appeared as a prime.

Direct measure of prime visibility. To maximize the comparability of the visibility test trials and the priming trials, the four blocks of the visibility test repeated one by one the prime–target pairs presented in the first four blocks of the priming phase. Note in particular that the visibility test trials thereby comprised trials with all three types of primes, whereas neutral primes had been excluded in the previous experiments.

As was pointed out by an anonymous reviewer, the target can be considered as a backward mask for the prime. Because the target remained on the screen until after a response had been made, its effective duration as a mask was terminated once the response had been made. The priming and visibility test phases can thus be seen as differing in the effective duration of the backward mask, since the priming trials required speeded decisions within an early response window, whereas the visibility test was unspeeded. For this reason, target duration in each trial of the visibility test was set to the participant's previous response latency as recorded for the same prime–target pair when it appeared in the priming phase.

Results and Discussion

Responses with latencies below 100 msec and above 1,000 msec were excluded from the analyses, thereby removing 2.7% of the data. We report the analyses based on balanced trials, but essentially the same pattern of results emerged when all the trials were included. The overall goodness-of-fit test of the signal detection model yielded a value of $\chi^2(40) = 31.79$, indicating a satisfactory model fit ($p = .82$). The priming parameters are shown in Table 4, along with the response criterion, the overall discriminability, and the standard deviation of the distribution of positive targets, relative to that of negative targets, as a function of PP. Considering the priming parameters, it can be seen that prime valence and PP interacted in the expected manner. Priming parameters were submitted to an ANOVA with independent variables of PP and prime valence. The interaction of PP and prime valence was the only significant effect to emerge in this analysis [$F(1,38) = 11.88$, $MS_e = 0.08$, $p < .01$]. One-tailed t tests revealed that the impact of negative primes increased significantly with increasing PP [$t(38) = 3.55$, $SE = 0.07$, $p < .01$], whereas the impact of positive primes decreased [$t(38) = -2.35$, $SE = 0.07$, $p = .01$].[4]

As was expected, there was a significant effect of PP on the response criterion [$t(38) = -5.11$, $SE = 0.19$, $p < .01$], reflecting that responses were biased in favor of the more frequent target valence (see Table 4). There was an effect of PP on the overall discriminability of positive from negative targets [$t(38) = 2.17$, $SE = 0.30$, $p < .05$] and on the standard deviation of positive targets relative to that of negative targets [$t = -2.37$, $SE = 0.18$, $p < .05$]. We return to these effects in the General Discussion section. Accuracy data and response latencies are presented in the Appendix.

The direct test of prime visibility revealed a mean d' of 0.09 ($SD = 0.18$) for the participants' ability to discriminate the masked primes' valence. The participants' performance only slightly, yet significantly, exceeded the chance baseline of $d' = 0$ [$t(39) = 3.26$, $SE = 0.03$, $p < .01$].[5]

Table 4
Means and Standard Deviations of Parameter Estimates in Experiment 4 as a Function of Positivity Proportion (PP)

	Low PP		High PP	
Parameter	*M*	*SD*	*M*	*SD*
Priming effect by negative primes (t_n)	0.03	0.21	0.29	0.25
Priming effect by positive primes (t_p)	0.14	0.21	−0.03	0.25
Response criterion (c)	1.05	0.68	0.07	0.52
Overall discriminability (d)	0.99	0.81	1.65	1.09
SD of positive relative to negative targets (s_p/s_n)	1.39	0.57	0.97	0.54

In Experiment 4, the interaction of PP and prime valence on priming effects was replicated even though the likelihood of target repetition and, for the subset of balanced trials, of prime repetition was kept constant. This rules out the possibility that differences in stimulus repetition were responsible for the interaction.

GENERAL DISCUSSION

The present experiments yielded a list context effect in a masked Stroop-like priming task. When the proportion of targets from the response categories was varied, primes from the less frequently presented category exerted a larger priming effect than did primes from the more frequently represented category. Experiment 3 ruled out a purely peripheral locus of that effect, Experiment 2 rendered an explanation in terms of response bias artifacts unlikely, and according to Experiment 4, stimulus repetition was not responsible for the effect.

Given the data on prime visibility, it is also unlikely that the present list context effects go back to strategic use of prime information. Although significantly larger than zero, the low d' values obtained in the direct measure of prime visibility suggest that the participants found it difficult to discriminate prime valence even when they were instructed to do so. Remember also that the present masking and presentation conditions were the same as those employed by Musch et al. (2002), who found that congruency proportion effects were eliminated under these conditions. Note in addition that the short (71 msec) SOA employed in the present experiments was not conducive to item-by-item strategic effects. Finally, when the direct measure of prime visibility was included as a covariate in the analyses, the PP effect remained significant (see note 2), suggesting that it did not depend on a residual amount of prime visibility (cf. Greenwald, Klinger, & Schuh, 1995).

Taken together, the pattern of results demonstrates that there are differential effects of PP on masked priming effects by positive versus negative primes independently of (1) overall shifts in the response criterion, (2) the distribution of responses, and (3) stimulus repetition. How can the PP effect be explained?

Accounting for the PP Effect

As was suggested in the introduction, list context effects of masked primes can arise if list context changes the participants' task set. It is clear that there are effects of PP on task set, as evidenced by the pronounced effects of PP on the response criterion. Yet the interaction of PP and prime valence is unlikely to be caused by shifts in the response criterion, as was shown in Experiment 2. Assume that in addition to a shift in response criterion, greater attentional weight is given to the rare target valence than to the frequent valence, so that participants are tuned to detecting target words of the rare valence. In terms of overall discrimination performance, which valence category is weighted heavier would not necessarily make a difference. A task set combining a liberal response criterion favoring the frequent valence and an attentional bias favoring the rare valence might, however, be a particularly efficient and parsimonious manner of dealing with the discrimination task in lists with skewed PP. If the greater weight given to the rare kind of valence also affects prime processing, a greater impact of primes of that valence results.

The bias favoring the rare valence might reflect a strategic focusing on the rare valence, as was just suggested. It might also reflect a more passive loss of sensitivity for the more frequent valence as a consequence of semantic, rather than motoric, satiation operating at the level of the categorizing of stimuli as positive or negative. A similar, yet different possibility is that PP affects a hypothetical evaluative scale underlying categorization. For example, in adaptation level theory (Helson, 1964), the effects of the list context can be represented by shifts of a single position on the evaluative scale, the adaptation level. The adaptation level is the scale position perceived as neutral, and in a simple version of the theory, it is given by the mean of the intensities of all stimuli perceived or judged. The intensity of subsequent stimuli is judged in relation to that neutral point. For example, when there are many positive targets, the neutral point would be expected to become displaced to the positive side, and as a consequence, positive targets would be perceived to be less positive and negative targets more negative, in comparison with the shifted neutral position. If the adaptation also affects the evaluation of prime stimuli, a greater impact of negative primes would be predicted, due to their increased evaluative extremity. One problem with the account in terms of adaptation level or an analogous account in terms of range frequency theory (Parducci, 1965) is that the evaluation of neutral prime stimuli should undergo the same shift, and since the effects of positive and negative primes are measured relative to those of neutral primes, an additive shift applied to all prime stimuli, whether positive, neutral, or negative, should cancel out, and the PP effect should not occur. It is conceivable, however, that the neutral prime stimuli, being nonwords, are somehow exempt from the shift implied by adaptation-level theory.

Attentional Bias and the Signal Detection Model

Returning to the as yet tentative and preliminary analysis in terms of differential attentional bias, consider how this analysis maps onto the signal detection model. In that model, targets are positioned on a strength-of-evidence axis according to the amount and direction of the evidence about the target valence that accumulates up to the response. In evidence accumulation, a greater attentional weight for the rare valence means an increase in the rate of evidence accumulation for that valence. As the rate increases, a greater amount of evidence can be accumulated until the response is required. As a consequence, the distribution of targets of the rare valence is shifted farther away from the neutral point (i.e., the point upon which a neutral target would be mapped) on the strength-of-evidence axis. It is nevertheless difficult to say whether the overall discriminability (d) of positive from negative targets would be affected: In a system with limited attentional resources, a greater attentional weight for the rare valence may imply a proportionally decreased weight for the frequent valence, and as a consequence, the distribution of the frequent kind of target would be shifted toward the neutral point. Taken together, the position of both target distributions relative to each other might remain relatively unchanged, although constellations exist under which such a change would be expected.[6] As a consequence, it is difficult to derive predictions for the model parameter d.

If the greater rate of evidence accumulation for the rare valence also extends to prime processing, greater priming effects (t_p or t_n) of primes of the rare valence would be expected as more evidence from such primes is activated and integrated with the target evidence, giving rise to the PP effect in priming that was the focus of this article. Furthermore, a greater rate of evidence accumulation for the rare valence (and a possibly reduced rate for the frequent valence) implies that the ratio of standard deviations of both target distributions changes: Evaluative differences between targets of the rare valence should be accentuated, because more evidence accumulates about them until onset of the response window. Another way to state this is that regression to the mean levels evaluative differences for the frequent targets in the strength-of-evidence distribution, since comparatively less evidence has accrued for them up to the response window. This means that the ratio parameter (s_p/s_n) should decrease as PP increases. As has been said, there is little information in the accuracy data about the standard deviations of the target distributions, and it is accordingly difficult to detect shifts in the ratio parameter. However, in Experiments 1 and 3A, the effect of PP on the ratio parameter approached significance and reached significance in Experiment 4. In each case, it was in the expected direction.

The Dimensionality of Evaluations

In conclusion, note that an interesting premise of this analysis is that, at some stage in the system, there must be a counter for accumulating evidence about the positivity of a given stimulus and a separate counter for accumulating evidence about its negativity. Otherwise, it would not be possible to have different rates of evidence accumulation for the two kinds of valence.[7] For this reason, the present findings also contribute to the debate on the unidimensionality versus bidimensionality of evaluations (e.g., Cacioppo & Berntson, 1994; Russell & Carroll, 1999).

As was explained in the introduction, the most compelling cases against unidimensionality are based on dissociations of separate ratings of positive and negative evaluations of given objects as a function of some manipulated or sampled variable (see, e.g., Goldstein & Strube, 1994; Katz & Hass, 1988). However, in this literature, a number of problems have been identified with the use of rating measures, such as response styles and random error (Green, Goldman, & Salovey, 1993), as well as response formats (Russell & Carroll, 1999), that may lead to spurious evidence for a dissociation of positive and negative evaluations. The present effect demonstrates differential effects of PP on the processing of positive versus negative evaluations on a behavioral measure that is not based on the traditionally used and critically discussed rating scales and self-report measures. In that way, the present results can be seen to support the bidimensionality hypothesis.

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NOTES

1. For each experiment, a more traditional analysis was also conducted in which individual d' values were computed to assess the separate effects of positive and negative primes. To assess the effects of negative primes, hits were defined as the response "negative" following negative primes; false alarms were defined as the response "negative" following neutral primes. Hits and false alarms were analogously defined for positive primes on the basis of the frequencies of the response "positive" following positive versus neutral primes. In addition, we also fitted several variants of the above signal detection model. For example, in one variant, the standard deviations of the distributions of positive and negative targets were constrained to be equal. Since the patterns of results were analogous in all of these analyses, we report only the results based on the described model.

2. We also computed the direct measure separately for trials with positive and negative targets for this and all of the following experiments, and we submitted the resulting d' values to ANOVAs with variables of PP (payoff schedule in Experiment 2) and target valence. This never yielded any significant results. In addition, the analyses of variance of the priming effects with variables of PP and prime valence were repeated (in this experiment, Experiment 3A and Experiment 4), with the direct measure of prime visibility as a covariate. In each case, the interaction of PP and prime valence remained significant, suggesting that it was not dependent on any residual ability to detect the prime valence (see Greenwald, Klinger, & Schuh, 1995).

3. We also fitted the extended signal detection model (see Experiment 1), which allows for differences in inhibitory versus facilitatory effects of incongruent and congruent primes, respectively. As in Experiment 1, the interaction of PP and prime valence emerged for both Experiments 3A and 3B even when the priming effect parameters were thus corrected for any asymmetries between costs and benefits of priming.

4. When the priming effect parameters were corrected for any asymmetries between costs and benefits of priming by means of the extended model (see note 3), the interaction of PP and prime valence nevertheless remained significant.

5. Since neutral primes were included in the visibility test, the data structure is the same as that for the priming phase trials, so that the same signal detection model can be fitted to the visibility test data. The t_n and t_p parameters then quantify how strongly the participants' responses discriminated between neutral versus negative primes and neutral versus positive primes, respectively. An ANOVA with variables of PP and prime valence on these measures of prime visibility revealed no significant effects. Descriptively, the interaction of PP and prime valence had the same form as that reported for the priming parameters, and it approached significance [$F(1,38) = 3.12$, $MS_e = 0.14$, $p = .09$], in support of the hypothesis of an attentional bias for the rare valence that also extends to prime processing.

6. Consider, for example, the case in which the positive words are much more polarized evaluatively than the negative words that might be only slightly negative. In this case, overall discriminability should be increased as attentional bias for the positive valence increases: Although this might lead to a small shift of the negative words toward the neutral point, the size of that shift is limited by their already relatively small distance to the neutral point. There is no such ceiling on the shift of the positive words to the right that is the consequence of an attentional bias for positively valenced stimuli. As a result, the distance between the two distributions on the strength-of-evidence axis should increase.

7. Note that the idea of separate counters for positivity and negativity is consistent with the unidimensional nature of the strength-of-evidence axis of the signal detection model: For decisions about two alternatives, even multidimensional evidence can be mapped onto one decision axis (Wickens & Hirshman, 2000). For example, a difference score of values attained by the positive and the negative counters might define the strength-of-evidence axis.

APPENDIX

Table A1
Accuracy and Latency Data in Experiment 1 as a Function of Positivity Proportion, Kind of Prime, and Kind of Target

		Positivity Proportion					
		Low		Medium		High	
Target	Prime	*M*	*SD*	*M*	*SD*	*M*	*SD*
Percentages of Correct Responses							
Negative	negative	91	8	71	11	57	20
	neutral	91	7	70	13	52	16
	positive	87	9	65	12	48	14
Positive	negative	51	15	70	13	85	7
	neutral	52	14	74	13	91	8
	positive	56	16	76	13	92	8
Response Latencies (msec)							
Negative	negative	464	44	496	61	486	57
	neutral	468	44	505	63	482	51
	positive	475	47	504	61	499	57
Positive	negative	521	56	495	54	436	36
	neutral	519	60	483	52	427	37
	positive	514	53	478	50	428	30

In the following, accuracy data and response latencies are presented for each experiment. Response latencies are further analyzed to check for the possibility of speed–accuracy tradeoffs in the interaction of PP and prime valence. The latency analyses are based on trials with correct responses, but similar results emerge when all the responses are used.

Experiment 1

Table A1 presents the accuracy data and the mean response latencies of correct responses as a function of PP, prime valence, and target valence for the priming phase trials. We tested whether there was an interaction of PP and prime valence in priming effects assessed on the basis of the response latencies. In particular, an interaction that is the mirror image of the interaction in the model-based priming parameters would suggest that speed–accuracy tradeoffs might have been responsible for the pattern of results.

On the basis of the response latencies, priming effects for positive and negative primes were separately computed for each participant. The effects of a given kind of prime were determined as the mean of the effect on congruent targets and of the effect on incongruent targets relative to the baseline conditions with neutral primes. For example, the effect of positive primes was assessed as the mean of the effect on congruent targets (i.e., the mean latency for positive targets preceded by neutral primes minus the latency for positive targets preceded by positive primes) and the effect on incongruent targets (i.e., the mean latency for negative targets preceded by positive primes minus the latency for negative targets preceded by neutral primes). The effects of positive and negative primes were then submitted to an ANOVA with independent variables of PP and prime valence, to test whether the interaction between PP and prime valence that was evident in the model-based priming parameters could be traced back to a speed–accuracy tradeoff. The interaction of PP and prime valence did not approach significance [$F(2,57) = 1.81$, $MS_e = 312.10$, $p = .17$], suggesting that speed–accuracy tradeoffs cannot explain the interaction in the model-based priming parameters.

Experiment 2

Table A2 presents the accuracy data and the mean response latencies of correct responses as a function of payoff schedule, prime valence, and target valence. The effects of positive and negative primes, estimated as just described for Experiment 1, were submitted to an ANOVA with independent variables of payoff schedule and prime valence. Again, the interaction of payoff schedule and prime valence was not significant in the latency domain ($F < 1$).

Experiment 3

Table A3 presents the accuracy data and the mean response latencies of correct responses as a function of PP, prime va-

Table A2
Accuracy and Latency Data in Experiment 2 as a Function of Payoff Schedule, Kind of Prime, and Kind of Target

		Payoff Schedule					
		Low Positive		Medium		High Positive	
Target	Prime	*M*	*SD*	*M*	*SD*	*M*	*SD*
Percentages of Correct Responses							
Negative	negative	71	10	60	13	56	16
	neutral	67	10	60	12	52	17
	positive	64	10	57	10	49	14
Positive	negative	52	14	62	11	67	13
	neutral	56	15	67	10	72	13
	positive	59	16	70	12	76	11
Response Latencies (msec)*							
Negative	negative	421	49	441	38	438	47
	neutral	428	44	452	39	445	49
	positive	421	42	452	42	446	46
Positive	negative	433	39	437	36	417	48
	neutral	435	43	443	36	419	52
	positive	429	41	429	36	415	50

*One participant was excluded due to no correct response in one cell.

Table A3
Accuracy and Latency Data in Experiment 3 as a Function of Positivity Proportion (PP), Kind of Prime, and Kind of Target

		Experiment 3A				Experiment 3B			
		Low PP		High PP		Low PP		High PP	
Target	Prime	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Percentages of Correct Responses									
Negative	negative	74	11	62	12	74	12	62	12
	neutral	76	10	63	12	74	13	56	13
	positive	73	13	56	11	71	11	54	10
Positive	negative	64	10	73	12	56	11	75	10
	neutral	61	13	79	13	59	12	78	9
	positive	63	12	78	12	64	11	79	11
Response Latencies (msec)									
Negative	negative	491	74	510	63	471	61	491	49
	neutral	491	75	502	66	479	56	508	49
	positive	496	76	519	78	478	63	486	44
Positive	negative	505	87	472	46	496	74	462	36
	neutral	512	81	457	47	484	68	457	30
	positive	512	78	459	52	472	73	450	34

APPENDIX (Continued)

lence, and target valence for Experiments 3A (the data are based on all the participants) and 3B. An ANOVA was performed on the effects of positive and negative primes in the latency domain, with independent variables of PP and prime valence. For Experiment 3A, the interaction of PP and prime valence did not reach significance ($F < 1$). For Experiment 3B, the interaction also was not significant [$F(1,37) = 2.46$, $MS_e = 408.78$, $p = .13$].

Experiment 4

Table A4 presents the accuracy data and the mean response latencies of correct responses as a function of PP, prime valence, and target valence. These values are based on the balanced trials, but the same effect pattern emerged when the data from all the trials were used. An ANOVA was performed on the effects of positive and negative primes in the latency domain with independent variables of PP and prime valence. The interaction of PP and prime valence did not reach significance [$F(1,38) = 1.27$, $MS_e = 1{,}033.34$, $p = .27$].

Table A4
Accuracy and Latency Data in Experiment 4 as a Function of Positivity Proportion (PP), Kind of Prime, and Kind of Target

		Low PP		High PP	
Target	Prime	*M*	*SD*	*M*	*SD*
	Percentage of Correct Responses				
Negative	negative	87	6	60	12
	neutral	87	9	51	16
	positive	82	11	52	13
Positive	negative	46	14	87	9
	neutral	50	16	94	4
	positive	54	13	93	4
	Response Latencies (msec)				
Negative	negative	462	65	506	70
	neutral	466	62	514	70
	positive	472	59	520	60
Positive	negative	525	71	446	42
	neutral	523	93	437	40
	positive	497	68	430	40

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