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Unconscious cognition isn't that smart: Modulation of masked repetition priming effect in the word naming task ☆

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Abstract

Masked repetition primes produce greater facilitation in naming in a block containing a high, rather than low proportion of repetition trials. [Bodner, G. E., & Masson, M. E. J. (2004). Beyond binary judgments: Prime-validity modulates masked repetition priming in the naming task. *Memory & Cognition*, 32, 1–11] suggested this phenomenon reflects a strategic shift in the use of masked prime as a function of its validity. We propose an alternative explanation based on the *Adaptation to the statistics of the environment (ASE)* framework, which suggests the proportion effect reflects adaptation of response-initiation processes to recent trial difficulty. Consistent with ASE's prediction, (1) stimuli that produce the proportion effect also produced an "asymmetric blocking effect", showing a smaller fall in response latencies of hard items than the rise of easy items when the two item types were intermixed

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relative to pure blocks comprised of only one item type, and (2) manipulation of prime validity was neither necessary nor sufficient to modulate the size of masked-priming effect.

Keywords: Masked repetition priming; Prime-validity effect; RT control; Blocking effect; Naming task; Unconscious cognition

1. Introduction

How smart is unconscious cognition? Unconscious cognition is studied most commonly using the masked-priming procedure. With this procedure, briefly presented and backward-masked prime is found to facilitate response to a target when it is the same word as the target (i.e., an *identity* or *repetition* prime) relative to an all-letter-different control prime (e.g., Forster & Davis, 1984), despite the fact that participants are unaware of the identity (and often the presence) of the prime. The fact that participants are not aware of the identity of prime is taken to mean that the observed priming effects are "automatic", and hence there is little scope for strategies that make use of the relationship between the prime and the target.

However, in recent years, Bodner and colleagues (Bodner & Dypvik, 2005; Bodner & Masson, 2001; Bodner & Masson, 2004; Masson & Bodner, 2003) reported a series of studies that challenge this assumption that masked priming is not subject to strategic influences. The phenomenon that formed the basis of their challenge is that the masked repetition priming effect is larger in a block containing a high proportion (0.8) of repetition trials (and hence a low proportion of unrelated trials) relative to a block containing a low proportion (0.2) of repetition trials. Bodner and colleagues found this effect of proportion manipulation in a number of tasks, including lexical decision (Bodner & Masson, 2001), naming (Bodner & Masson, 2004), and number judgment (both magnitude and parity judgment) (Bodner & Dypvik, 2005). The authors called the effect the *prime-validity effect*, and put forward the following *memory recruitment account* to explain the phenomenon.

The basic assumption of the memory recruitment account is that processing operations applied to the prime, whether it is masked or not, are encoded into a new memory resource (cf. Kolers & Roediger, 1984), which can later be recruited to assist with subsequent target processing if the test conditions foster its recruitment. Bodner and Masson (2001) referred to the studies in the domain of implicit memory that used the manipulation of proportion of unmasked repetition trials (e.g., Allen & Jacoby, 1990; Jacoby, 1983) to make a case for their claim. In these experiments, subjects first studied a set of words and later were given an identification task involving briefly presented masked words. The proportion of targets that had been previously studied was varied (e.g., 0.1 vs. 0.9). Jacoby (1983) found that prior study increased accuracy of target identification (i.e., repetition priming was found) and that this benefit was greater when a high rather than low proportion of trials were previously studied. A similar finding was reported by Allen and Jacoby (1990). According to Bodner and Masson (2001), the effect of the proportion of repetition trials found with masked primes has the same basis as these proportion effects observed with long-term priming. In their words, ''word recognition can become tuned to regularities in the prime context, causing primes to play larger or smaller role in the identification of targets depending on their validity'' (p. 619).

The term *prime-validity effect* implies that the prime's predictiveness of the response to the target is what is driving the effect of proportion of repetition vs. control prime trials. However, the fact that the proportion effect with masked primes empirically resembles the proportion effect observed with unmasked consciously available primes does not logically require that the same mechanism is involved. The memory recruitment account of proportion effect observed with masked primes suggests unconscious cognition is smart: it can adapt intelligently to novel situations. There are at least two conceptual reasons for questioning this view. First, the key assumption of the memory recruitment account that a masked prime establishes an episodic record is diametrically opposed to the generally accepted view that conscious awareness is a prerequisite for establishing an episodic record. This view forms a basis for a diverse range of concepts including the object file (Kahneman & Treisman, 1984), the token individuation account of repetition blindness (Kanwisher, 1987; see also Mozer, 1989) and Moscovitch's (1995) model of consciousness and memory. What is common to all of these perspectives is that although conscious awareness is not necessary to activate a pre-stored representation, it is involved in establishing an episodic token of an event. The idea that a memory episode is established for masked primes that are not consciously available is clearly at odds with these perspectives. Second, the notion that prime *validity* – the usefulness of the prime – drives the proportion effect presupposes that the prime and target are treated as separate perceptual events. Bodner, Masson, and Richard (2006) recently cast the memory recruitment account in terms of Anderson's rational analysis of memory (e.g., Anderson & Milson, 1989; Anderson & Schooler, 1991), drawing a parallel between masked priming and semantic priming. Anderson and Milson (1989) explained semantic priming in terms of the change in the probability of needing the target word (e.g., cat) in the context of the prime word (e.g., dog). Bodner et al. (2006) suggested that the proportion effect also reflects a change in the need probability. Explaining the proportion effect in this way logically requires that the masked prime provides a ''context'', distinct from the target. While this makes sense with supraliminal primes, this is unlikely with masked, subliminal primes. In particular, works by Huber and colleagues (Huber, Shiffrin, Lyle, & Ruys, 2001; Weidemann, Huber, & Shiffrin, 2005) demonstrating that spatiotemporal proximity of prime and target results in source confusion (prime features are confused with target features) and the failure to ''discount'' the influence of the prime strongly argues against this possibility.

In sum, we argue there are conceptual problems with the memory recruitment account of proportion effect with masked primes. Before accepting the memory recruitment account and consequently attributing a high level of intelligence to unconscious cognition – one that is indistinguishable from conscious cognition – we felt it essential to investigate an alternative account of the proportion effect observed with masked primes.

1.1. The ASE model

As an alternative to the memory recruitment account, we propose an account of masked repetition-proportion effect based on a model of response initiation put forward recently by Mozer, Kinoshita and Davis (2004; see also Kinoshita & Mozer, 2006; Mozer, Kinoshita, & Shettel, 2007), termed the *Adaptation to the Statistics of the Environment* (*ASE*) model. ASE characterizes the operation of control processes involved in response initiation in speeded response tasks and how these control processes are modulated by recent experience (i.e., the preceding sequence of trials). In the present context, the key point is that the ASE explains the repetition-proportion effect in terms of the adaptation of control processes to recent trial history.

The ASE model was originally proposed to account for *blocking effects* observed in naming, reported by Lupker and colleagues (Lupker, Brown, & Colombo, 1997; Taylor & Lupker, 2001). The blocking effect refers to the finding that the composition of a block of trials, in terms of the difficulty of items within the block, influences naming latency to a word. For example, naming latency for a high-frequency word increases when it is presented in a *mixed* block containing low and high-frequency words, relative to when it is presented in a *pure* block of high-frequency words. Conversely, naming latency for a low-frequency word decreases when it is presented in a mixed block relative to a pure block. Blocking effects have been observed in a wide range of speeded response tasks: visual search using traditional key press measure (Strayer & Kramer, 1994) and mouse tracking measure (Song & Nakayama, 2007); lexical decision task (Kinoshita & Mozer, 2006; Rastle, Kinoshita, Lupker, & Coltheart, 2003); as well as naming. It has been found not only with the manipulation of word frequency but with a variety of manipulations that influence stimulus difficulty: in the word naming (read aloud) task, high- vs. low-frequency words, words vs. nonwords, regular vs. irregular words have all shown blocking effects (Lupker et al., 1997; Taylor & Lupker, 2001). It has also been found when naming "easy" vs. "hard" pictures – where the difficulty manipulation is based on a combination of factors that affect picture naming latency such as frequency and length of name, with monosyllabic vs. disyllabic names (Meyer, Roelofs, & Levelt, 2003), as well as sums varying in difficulty (e.g., "$10 + 7 = ?$" vs. "$8 + 9 = ?$", Lupker, Kinoshita, Coltheart, & Taylor, 2003). The range of conditions that produce blocking effects led Rastle and colleagues (2003) to suggest that an account of blocking effect requires a general mechanism that is domain-free.

A mathematical description of RT adaptation which produces blocking effects is given in Mozer, et al (2004, 2007; see also Kinoshita & Mozer, 2006) and we refer the reader to these works; and here, we focus on explaining how the same mechanism that produces blocking effects leads to proportion effects. To this end, we first describe the basic tenet of the ASE, then show how the ASE predicts proportion effects when blocking effects are asymmetric. We then present two experiments to empirically test this claim.

The ASE mechanism proposed to explain blocking effects is cast in terms of a general response control process that operates across tasks, and determines the point in time at which subjects initiate a response based on the trade off between speed and accuracy. If it responds early it risks a higher likelihood of error, and if it responds late, the response is delayed. How it decides when to respond is based on a decision model suggested earlier by Mozer, Colagrosso, and Huber (2002). The main tenet of the decision model is that a response cost is computed that depends on both RT (i.e., the cost to waiting) and expected error rate (i.e., the cost of possibly making an incorrect response), and it responds at the point in time when a minimum in *total* cost is attained. To compute this cost, the model requires an estimate of its expected error rate as processing within a trial unfolds.

This estimate is obtained without knowledge of the correct response, in the following way. From the onset of a stimulus, the model gradually accumulates evidence supporting each of the various response alternatives, much as a random-walk or accumulator model would. The model can translate this evidence at any point in time into a probability distribution over responses by a formula that gives higher probability to responses garnering more evidence. Assuming that a response initiated at a particular point in time is chosen from this distribution, and also assuming that this distribution reflects the likelihood of a given response being correct, the model can compute an error estimate by taking the expectation of incorrect response under the response distribution. In practice, this is quite similar to assuming that the most probable response at any point in time is correct, and the expected error rate is the probability of making any other response. As is standard in RT models, evidence is assumed to accumulate gradually over time, so that a response emitted early is more likely to be incorrect, i.e., a speed-accuracy trade off is expected. The result of this error-estimation calculation is a curve like one of the four shown in Fig. 1: each curve depicts the expected error rate as a function of time from stimulus onset. The curves are monotonically decreasing, indicating that as evidence accumulates, the expected error rate drops. The value of additional evidence diminishes over time, causing the error curve to be negatively accelerated as it approaches its asymptotic value.

The model can respond at any point in time, and the corresponding value of the error curve indicates the model's internal estimate of producing an error. Roughly, the error curves in Fig. 1 can be viewed as the expected cost of an error. The dotted straight line in Fig. 1 can be viewed as a time cost – the cost of waiting that increases linearly with time. Mozer et al. (2002) showed that choosing the point in time that minimizes a total cost that depends both on RT and expected error rate is equivalent to finding the intersection of the error curve and the line. (Technically, the error curve and the time line are *derivatives* of the costs.) Thus, the point of intersection is the *optimal* time to respond with respect to a given speed-accuracy tradeoff.

A key claim in accounting for blocking effects is that the error estimate is noisy, either because the most probable response is not correct, or because the current response distribution fails to indicate the correct response, or because the error estimate is an expectation and response-initiation systems actually obtain this estimate via stochastic sampling. To overcome the unreliability of the error estimate, a sensible strategy in a stationary environment is to average the error estimate being com-

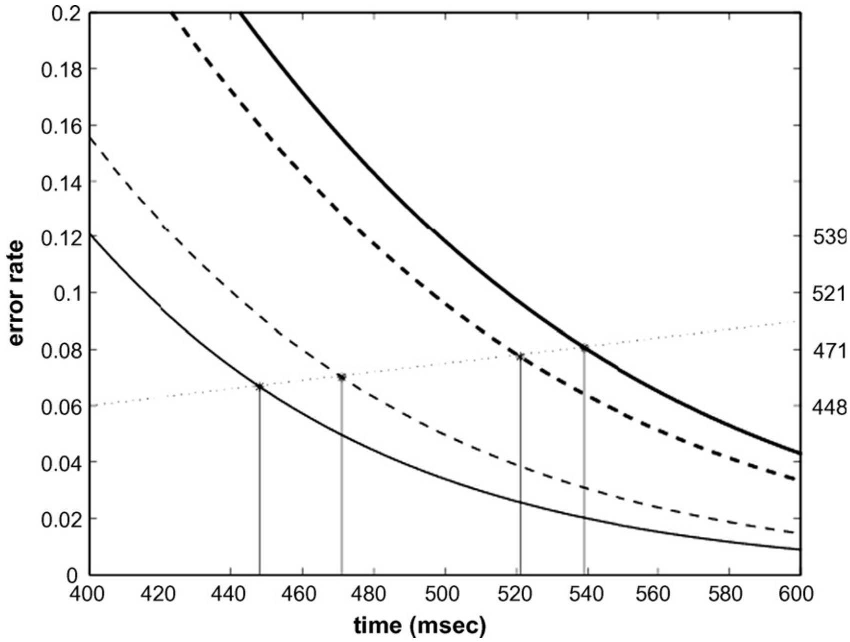

Fig. 1. *How the ASE model explains blocking effects*. Error curves for easy and hard stimuli are depicted by the solid lines in pure blocks and dashed lines in mixed block, along with the RT cost function depicted by the dotted straight line. The estimated optimal RTs are represented by where the RT cost function intersects the error curves (from left to right, easy items in pure easy block, easy items in mixed block, hard items in mixed block, and hard items in pure hard block).

puted for the current stimulus with the error estimate from recent trials. By ''stationary environment'', we mean an environment where item difficulty – and hence shape of the error curve – does not vary much from one trial to the next. By ''average'', we mean that the error curve used to estimate the cost is a combination of the current error curve and a *historical* error curve, which itself is an average of the error curves from recent trials. As shown in Fig. 1, the speedup of a hard item (relative to a pure block of hard items) and the slowdown of an easy item (relative to a pure block of easy items) when mixed together thus follows from the fact that the historical error curve in a mixed block would be the average of the historical error curves of the pure hard block and pure easy block. Mozer et al. (2007) have shown that through this adaptation mechanism, the ASE model simulates the pattern of blocking effect and the trial-by-trial sequence effect found in the naming task (e.g., Lupker et al., 1997; Taylor & Lupker, 2001).

How does the ASE's explanation of blocking effects pertain to the masked-priming repetition proportion effect? Essentially, one can view the high proportion (0.8) repetition trial block as consisting of 80% easy items (easy due to the repetition prime) and 20% hard items (harder due to the fact that the prime does not provide evidence for the target), whereas the low proportion (0.2 repetition trials) block as consisting of 20% easy items and 80% hard items. Thus, the two conditions involve different mixtures of easy and hard, and we know from the blocking effect and ASE that the composition of a block can lead to a prediction of different response times for the same item in the two blocks. Note that interpreting the prime-validity manipulation as a list composition manipulation allows us to explain the phenomenon without recourse to strategic processing of the masked prime; rather, the explanation is based on the fact that irrespective of whether or not the prime is consciously perceived, prime type (repetition or unrelated control) affects item (target) difficulty, and the response to a given target depends on the difficulty mix within a block.

The critical assumption of the ASE account that links the proportion effect with the blocking effect concerns the ‘‘symmetry’’ of blocking effects. A blocking effect is said to be symmetric when the easy items slow down (in a mixed block relative to a pure block) by the same amount that the hard items speed up. In naming tasks that used items of varying difficulty (e.g., high- vs. low-frequency words, words vs. nonwords) blocking effects have been typically symmetric (see Lupker et al., 1997). A virtue of ASE is that it can produce roughly symmetric blocking effects, and when it produces asymmetric blocking effects, the asymmetry is always in the direction observed in experimental studies, with more slowdown of easy items than speedup of hard items. This comes about because what makes an easy item easy is that information about the item is transmitted robustly and rapidly. The result of this more efficient and accurate information transmission is that the critical evidence in support of the item accumulates more rapidly, making the slope of the easy curve steeper than the slope of the hard curve. The important point to note is that the conditions that produce proportion effects are exactly those that produce asymmetric blocking effects.

The Appendix presents a mathematical explanation of why asymmetric blocking effects in the ASE imply repetition-proportion effects. Intuitive understanding of the link between the two effects is offered by Fig. 2.[1] The y-axis represents the (hypothetical) RT; the x-axis of the figure represents the proportion of easy items in a block. In the blocking manipulation, a pure easy block represents 100% easy items, a pure hard block 0% easy items, and a mixed block 50% easy items. Easy items are represented by the solid line; hard items are represented by the dashed line. In the proportion manipulation, a high-proportion (repetition trials) block represents 80% easy items and a low-proportion block 20% easy items. In both cases, the list composition manipulation (pure- vs. -mixed block manipulation or proportion manipulation) is regarded by the ASE as a manipulation of difficulty of the task environment. The y-axis of the figure shows hypothetical response times, consistent with the qualitative properties of both asymmetric blocking effects and repetition-proportion effects.

Both effects are observed when the task environment has a weaker influence on the hard items than on the easy items (i.e., the slope of hard items as a function of block composition is shallower than the slope of easy items, as depicted in Fig. 2). The blocking effects for easy items is represented by the difference in RT between the 50%-easy and 100%-easy conditions; and the smaller (asymmetric)

[1] We thank David Huber for suggesting the figure.

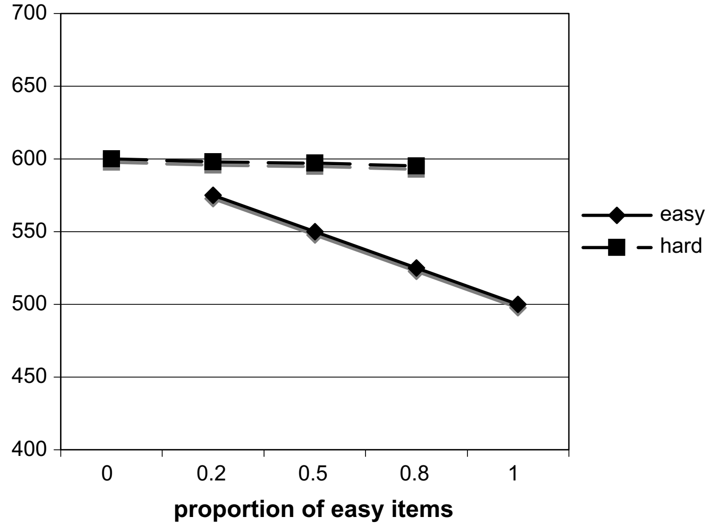

Fig. 2. *Asymmetric blocking effect and the repetition-proportion effect.* The slopes show the effect of task environment, going from hard to easy, as the proportion of easy item increases, from left to right. The blocking effect for easy item (indicated by the solid line) is represented by the difference in RT between the 1.0 easy (i.e., pure easy) and 0.5 easy (i.e., mixed) blocks; the blocking effect for hard items (indicated by the dashed line) is represented by the difference in RT between the 0.5 easy (mixed) and .0 easy (pure hard) blocks. The slowdown of easy items (in this hypothetical example, 500 ms vs. 550 ms = 50 ms) is greater than the speedup of hard items (597 ms vs. 600 ms = 3 ms), indicating an asymmetric blocking effect. The proportion manipulation involves the comparison between the 0.2 (low proportion of repetition trial) blocks and the 0.8 (high proportion of repetition trial) blocks. The increase in the repetition priming effect from the low-proportion block (598 ms vs. 575 ms = 23 ms) to the high-proportion block (595 ms vs. 525 ms = 70 ms) represents the proportion effect.

blocking effect for hard items is represented by the difference in RT between the 50%-easy and 0%-easy conditions. Repetition-proportion effects are reflected in the smaller difference between easy and hard items for the 20%-easy condition compared to the 80%-easy condition. Thus, both effects can arise from the task environment having a weaker influence on the hard items than on the easy items.

To summarize, ASE's prediction for the repetition-proportion manipulation goes hand in hand with the task yielding an asymmetric blocking effect. If the effect is symmetric (the two lines in Fig. 2 are parallel), ASE predicts a main effect of block type, with faster RTs in the high-validity block, but no repetition-proportion (prime validity) effect. If the effect is asymmetric, ASE predicts a greater benefit of easy stimulus environment (high-validity block) for easy items (repetition trials) than for the hard items (control trials). These two patterns – the prime validity (repetition proportion) effect and an asymmetric blocking effect – must occur together within the ASE account. We test this prediction of ASE by showing that the same stimuli that yield a prime-validity effect also produce an asymmetric blocking effect.

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The outline of the present study is as follows. Experiments 1 and 2 provide an empirical test of the ASE model's account of repetition-proportion effect outlined above. Experiment 1 establishes a repetition-proportion effect; Experiment 2 uses the same stimuli to test the prediction that an asymmetric blocking effect would be observed for these stimuli. Once the prediction of the ASE is confirmed, indicating that in principle the ASE model can account for the repetition-proportion effect and the asymmetric blocking effect in terms of the same adaptation mechanism, we then consider what is responsible for the asymmetric, as against the symmetric, blocking effect within the framework of the ASE model. Discussion of the mechanism along with a simulation within the ASE model is presented following Experiment 2. Finally, Experiments 3 and 4 test further predictions of the ASE model by showing that the manipulation of proportion of repetition trials (prime validity) is neither necessary nor sufficient condition to modulate the size of masked repetition priming effect.

2. Experiment 1

The aim of Experiment 1 was to establish the presence of a repetition-proportion effect in a naming task using a within-subject manipulation of block type (high vs. low proportion of repetition prime trials), which has been used in previous investigation of blocking effects.

2.1. Method

2.1.1. Subjects

Twenty volunteer undergraduate psychology students from Macquarie University and 20 from University of Arizona[2] participated in Experiment 1 for course credit.

2.1.2. Design

All experiments to be reported used the word naming (read aloud) task. In Experiment 1, subjects named 200 words in two blocks, each containing 100 words. The experiment constituted a 2 (*Prime type*: repeated vs. unrelated) $\times$ 2 (*Block type*: high vs. low proportion of repeated trials) factorial design, with both factors manipulated within subjects. Block order (high-proportion block first vs. low-proportion block first) was counterbalanced across subjects. The dependent variables were naming latency and error rate.

2.1.3. Materials

The stimulus materials used in this experiment were 200 low-frequency words. All items were five-letters long, and monosyllabic. The low-frequency words ranged

[2] The factor *Subject origin* (Macquarie University vs. University of Arizona) did not produce any main or interaction effect, hence data will be reported collapsed over this factor.

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between 1 and 20 occurrences per million (Kucera & Francis, 1967), with a mean of 5.55. Examples are *BATCH, CHEER, SPICE.*

In the repetition condition, the to-be-responded item was preceded by itself (e.g., *batch-BATCH*); in the unrelated condition, each was preceded by a different item as a prime that shared just the initial phoneme with the target (e.g., *boost-BATCH, chant-CHEER*, slang-*SPICE*).

The 200 words were divided into 10 sets containing 20 items each. The construction of high-proportion block (consisting of 80 repetition trials and 20 unrelated trials) and low-proportion block (consisting of 20 repetition trials and 80 unrelated trials) was as follows. Each block was designed as 20 sequences consisting of five trials. Within a sequence, the items were arranged so that in the high-proportion block, a sequence consisted of three repetition trials followed by a fourth repetition trial, then an unrelated trial. In the low-proportion block, a sequence consisted of three unrelated trials followed by a fourth unrelated trial then a repetition trial. The motivation for designing the sequence in this way was to maximize the effect of preceding item type on targets comprising the low-proportion item. The assignment of sets to the high- and low-proportion conditions was also counterbalanced across subjects so that each set occurred in each condition equally often (and more than once) across subjects. The order of 20 sequences within a block varied randomly across subjects. The order of blocks (*high*-proportion block first or *low*-proportion block first) was counterbalanced across subjects so that half of the subjects did the *high* condition first, and the other half, the *low* condition first. This, along with the counterbalancing of sets to conditions meant that full counterbalancing was achieved across every 20 subjects.

Prior to each test block, subjects were given 16 practice and buffer trials. These items were selected according to the same criteria as the test stimuli, and the trials contained only the prime type (repetition or unrelated) that was dominant in the block. These items were not included in the analysis.

2.1.4. Apparatus and procedure

Subjects were tested individually, seated approximately 40 cm in front of an Dell 19″ Flat Trinitron monitor, upon which stimuli were presented. Each subject completed both the *high* and *low* (repetition) proportion blocks.

Subjects were instructed at the outset of the experiment that they would be presented with a series of words, and their task was to read aloud each word as fast and accurately as possible. They were instructed to speak their response into a microphone. They were further told that each trial will start with a presentation of a warning signal consisting of five hash signs (#####), followed by a word in uppercase letters, and no mention was made of the primes presented in lowercase letters.

Stimulus presentation and data collection were achieved through the use of the DMDX display system developed by K.I. Forster and J.C. Forster at the University of Arizona (Forster & Forster, 2003) running on a Dell Optiplex GX240 computer running on an Intel Pentium III chip at 650 MHz. Stimulus display was synchronized to the screen refresh rate (11.7 ms). Subjects spoke their response into the Bayerdynamic microphone (Model MEM 194/TG-X45) held a constant distance from the mouth by means of a neckrest, and naming latency was measured by means of a software voice key contained within DMDX. The software voice key monitored the digital output from the soundcard (Sound Blaster Live!) and recorded the first time at which an amplitude value exceeded a preset threshold.

Each trial started with the presentation of a series of five hash signs (#####) for 583 ms. This was followed by a prime in lowercase letters for 59 ms, then a target word in uppercase letters, all in Courier 12 point font, in the center of the screen. The target remained on the screen for a maximum of 2000 ms, or until the subject's response. Following a blank screen for 300 ms, the next trial started. Subjects were given no feedback on either latency or error rate during the experiment.

2.2. Results and discussion

For this and subsequent analyses, the preliminary treatment of trials was as follows. Any trial on which the subject made an error or a trigger failure occurred with the voice key (either because the subject spoke too softly, or extraneous noise such as heavy breathing triggered the voice key or had a latency of less than 150 ms) was excluded from the latency analysis. For the remaining trials, to reduce the effects of extremely long and short latencies, the cutoff was set for each participant at 3 *SD* units from each participant's mean latency and those shorter or longer than the cutoff was replaced with the cutoff value. In Experiment 1, this affected 1.31% of the trials. Naming latencies and error rates were analyzed using a three-way analyses of variance (ANOVA) with *Block type* (high vs. low proportion), *Prime type* (repeated vs. unrelated) and *Block order* (high-proportion block first vs. low-proportion block first) as factors. The first two were within-subject factors, and *Block order* was a between-group factor. Effects were considered to be significant at the .05 level. Mean naming latencies and error rates are presented in Table 1.

For latency, the main effect of *Block type* was significant, $F(1,38) = 12.41$, $\text{MSe} = 1073.55$, as was the main effect of *Prime type*, $F(1,38) = 70.16$, $\text{MSe} = 202.06$. The interaction between the two, that is, the repetition-proportion effect, was also significant, $F(1,38) = 6.15$, $\text{MSe} = 108.02$. This interaction reflected a greater repetition priming effect in the high prime-validity block (23 ms) than in the low prime-validity block (15 ms). None of the other main or interaction effects reached significance, all $F(1,38) < 2.26$, $p > .13$.

Table 1
Mean naming latencies (RT, in ms, with standard deviation in parentheses) and percent error rates (%E) in Experiment 1

Prime type and example	Block type (proportion of repetition trials)					
	High		Low		Difference	
	RT	%E	RT	%E	RT	%E
Repetition *badge-BADGE*	504 (73)	1.6	526 (69)	2.0	22	0.4
Unrelated *boost-BADGE*	527 (82)	3.5	541 (76)	2.3	14	−1.2
Priming effect	23	1.9	15	0.3		

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For error rate, the main effect of *Prime type* was significant, $F(1, 38) = 9.98$, MSe = 5.07. The main effect of *Block order* was also significant, $F(1, 38) = 7.19$, MSe = 16.39. None of other main or interaction effects reached significance, all $F(1, 38) < 2.85$, $p > .10$.

The main result of Experiment 1 was that a repetition-proportion effect was found, replicating the result reported by Bodner and Masson (2004). The magnitude of the effect (8 ms) was comparable to the 10-ms effect reported by Bodner and Masson (2004, Experiment 1), suggesting that the difference in the way block type was manipulated (within-subject manipulation in the present experiment and between-subject manipulation in the Bodner and Masson study) did not impact on the finding of the prime-validity effect. The fact that Block order did not interact with this effect further suggests that there was no carry-over effect. On the basis of these observations, we suggest that the within-subject manipulation is more advantageous because as well as being more powerful, it allows a more sensitive assessment of slowdown vs. speedup between blocks that is not obscured by large variations in individual RTs. Accordingly, we will continue to use a within-subject manipulation of block type in all subsequent experiments. We note that in the present experiment, both repetition and unrelated trials speeded up in the high-validity block, but the speedup was more limited for the unrelated trials, consistent with the ASE model's account of the prime-validity effect.

3. Experiment 2: Pure- vs. -mixed blocks

Having found a repetition-proportion effect, Experiment 2 used the same stimuli to test for the pattern of blocking effect. As discussed earlier, the ASE model predicts these stimuli to show an asymmetric blocking effect such that the easy items (repetition trials) should slow down but the hard items (unrelated trials) should show limited speedup when they are mixed together, relative to when the are presented in respective pure blocks.

3.1. Method

3.1.1. Subjects

Twenty-four volunteer undergraduate psychology students from Macquarie University participated in Experiments 2 for course credit.

3.1.2. Design

Each subject named 200 words in three blocks containing 50, 50 and 100 words each, the blocks comprised of repetition trials only (pure repetition block), unrelated trials only (pure unrelated block) and a random mix of repetition and unrelated trials (mixed block), respectively. The experiment constituted a 2 (*Prime type*: repeated vs. unrelated) × 2 (*Block type*: pure vs. mixed) factorial design, with both factors manipulated within subjects. Block order was counterbalanced across subjects using a Latin square design. The dependent variables were naming latency and error rate.

3.1.3. Materials

The stimulus materials used in this experiment were the 200 low-frequency words used in Experiment 1. As in Experiment 1, each target was preceded by either a repetition prime (e.g., *batch-BATCH*) or initial-phoneme matched unrelated prime (e.g., *boost-BATCH*). The 200 words were divided into four sets containing 50 items each. They were assigned to four experimental conditions resulting from a factorial combination of *Prime type* (repeated vs. unrelated) and *Block type* (pure vs. mixed). The assignment of four sets to the four experimental conditions was counterbalanced across subjects so that each subject saw an item only once, and across every four subjects a set appeared in each experimental condition once. In addition, the order of the three blocks (pure repetition, pure unrelated and mixed) was counterbalanced across subjects so that for every three subjects each block appeared in each of the first, second and third position once. Thus, full counterbalancing was achieved with every 12 subjects.

Prior to each of the "pure" blocks, subjects were given eight practice and buffer trials, and 16 trials prior to the "mixed" block. These items were selected according to the same criteria as the test stimuli, and the trials contained the prime type (repetition prime or unrelated prime) that was representative of the block. These items were not included in the analysis.

3.1.4. Apparatus and procedure

Apparatus and the testing procedure were identical to Experiment 1.

3.2. Results

The preliminary treatment of trials was identical to Experiment 1. The 3 *SD* cutoff replacement procedure affected 1.30% of the trials. Target naming latencies were analyzed using a three-way ANOVA with *Prime type* (repeated vs. unrelated), *Block type* (pure vs. mixed) and *Block order* (pure repetition block first, pure unrelated block first, mixed block first) as factors. The first two factors were within-subject factors, and *Block order* was a between-group factor. Effects were considered to be significant at the .05 level. Mean naming latencies and error rates are presented in Table 2.

For latency, the main effect of *Prime type* was highly significant, $F(1,21) = 75.60$, $\text{MSe} = 164.61$. The main effect of *Block type* was not significant, $F(1,21) < 1.0$. The interaction between these two factors, which represents a blocking effect, was marginal, $F(1,21) = 3.66$, $\text{MSe} = 319.62$, $p = .07$. Critically, simple effect analysis showed that the 13 ms slowdown of repetition trials in the mixed block was significant, $F(1,21) = 5.56$, $\text{MSe} = 308.84$;, but the 3 ms speedup of the unrelated trials was not, $F(1,21) = .16$, $\text{MSe} = 308.84$. None of the other main or interaction effects reached significance, all $F < 1.22$, $p > 31$.

For error rate, none of the effects were significant: all $F < 1.54$, $p > .24$.

3.3. Discussion

The ASE model can explain the prime-validity/repetition-proportion effect (Experiment 1) as a variety of list composition effect. In order for this explanation to be coherent, however, ASE predicts that the stimuli used to obtain a prime-validity effect must also produce an asymmetric blocking effect. Experiment 2 provided support for this prediction. Specifically, using the stimuli used in Experiment 1, the pattern showed that in mixed blocks relative to the pure blocks, repetition trials slow down, whereas unrelated trials do not speed up.

What is responsible for the asymmetric blocking effect? As mentioned in Section 1, the asymmetric pattern of the blocking effect observed in Experiment 2 is in contrast to the symmetric pattern that has been observed consistently in a number of experiments using the naming task (e.g., Lupker et al., 1997, 2003; Rastle et al., 2003), in which the easy items slow down and hard items speed up by equal amounts when they are mixed together. Also as mentioned earlier, the ASE model can produce either a symmetric effect or an asymmetric effect (but always with more slow down of easy items than speed up of hard items). What then, is responsible for the asymmetric blocking effect observed here?

One difference between the present Experiment 2 and the earlier naming experiments is the nature of "difficulty" manipulation. In all of the earlier experiments, the manipulation was between-item and involved different stimuli: for example, high- vs. low-frequency words, or words vs. nonwords. In contrast, in the present Experiment 2, the difficulty manipulation was within-item, and involved different prime types (repetition vs. unrelated primes).

One possible explanation for why the difficulty manipulation involving the present priming procedure yields an asymmetric pattern is based on the idea of response conflict. This claim is consistent with the position that views masked-priming effects as due to task operations shared between the prime and target (e.g., Kunde, Kiesel, & Hoffmann, 2003). In the naming task, a locus of this effect of masked primes is in speech planning, as evidenced by the fact that priming due to an overlap between a prime and target shows a characteristic left-to-right incremental pattern – for example, *BELLOW* is primed by *bellom* but not by *dellow* – which is a signature of speech planning process (e.g., Forster & Davis, 1991; Kinoshita, 2000; Kinoshita, 2003; Schiller, 2004). Support for the view that a speech response is planned for the prime is also found in Forster and Davis' (1991) observation that sometimes subjects pronounce the masked prime, rather than the target. When the prime is a repetition prime, obviously the response is identical to the target; when it is an unrelated control prime, the response is different from that of the target.[3]

[3] The fact that the unrelated primes used here and by Masson and Bodner (2003) shared the onset with the target (e.g., *boost-BATCH*) may initially appear at odds with our claim that these unrelated primes induce response conflict. Schiller and Kinoshita (2007) however reported recently that such whole word primes with segments that mismatch the target beyond the onset (e.g., *boost-BATCH*) delayed naming responses to targets relative to letter primes containing the same initial letter followed by trailing % signs (e.g., *b%%%%*) when the prime duration was long (66 ms) but not when it was short (33 ms). They took the finding to suggest that response conflict from mismatching segments beyond the onset does occur, but that it occurs late, during the phonological encoding stage (the stage during which phonemic segments are inserted into a syllabic frame) in the speech production process. This finding is consistent with the present claim that the initial-letter matched unrelated primes induce conflict during the preparation of speech response.

Table 2
Mean naming latencies (RT, in ms, with standard deviation in parentheses) and percent error rates (%E) in Experiment 2

Prime type and example	Block type					
	Pure		Mixed		Difference	
	RT	%E	RT	%E	RT	%E
Repetition badge-BADGE	505 (87)	4.1	518 (86)	4.2	13	0.1
Unrelated boost-BADGE	538 (80)	4.3	535 (93)	3.8	−3	−0.5
Priming effect	33	0.2	17	−0.4		

At an intuitive level, how a conflict at the response level could produce an asymmetric blocking effect in the naming task can be explained as follows.[4] In the naming task using between-item manipulations of difficulty such as frequency (high- vs. low-frequency words) or lexical status (words vs. nonwords), the hard items do not typically lead to a large increase in error rate, but simply slows down processing of the target. In contrast, unrelated primes lead to some degree of certainty in the wrong response: for example, the unrelated prime *boost* would lead to the utterance "boost" rather than the target *BADGE*. This means speeding up the response to an unrelated prime trial is risky because the probability of error will increase. Within the ASE, this means that the error curve (error estimate over time) of unrelated-primed trials would have a steeper slope, which in turn limits the amount of speedup for those items when these are mixed together with easy items.

We performed a simulation of the ASE model to test the viability of this response-conflict account. The prime is modeled as producing residual activation that influences the processing of the target in the response pathway. The two left-hand panels of Fig. 3 show, for repetition-primed (valid) and unrelated-primed (invalid) targets, respectively, the probability of responding correctly to the target (thick line rising to probability 1) or responding incorrectly as the prime (thin line). (For the valid primed target, the probability of incorrect response is at floor.) The effect of the unrelated prime is manifested as an initial period in which the prime is a plausible response alternative. The rightmost panel shows the error curves that result from these response probability functions, corresponding to, from left to right, a repetition-primed target in the pure block, repetition-primed target in the mixed block unrelated-prime condition in the mixed block, and unrelated-primed target in the pure block. It can be seen that the error curves for the unrelated-primed targets have steeper slopes. Because the slopes are steeper, averaging the current error curve with the historical error curve results in smaller shift to the left (speedup) of unrelated-primed targets from the pure block to the mixed block than the shift to the right (slowdown) of repetition-primed trials from the pure block to the mixed block. As a consequence, the difference in the optimal RT (corresponding to where the RT cost function intersects the error curves, with the values 68, 81, 99 and 102 in this exam-

[4] We are grateful to David Huber for suggesting this description.

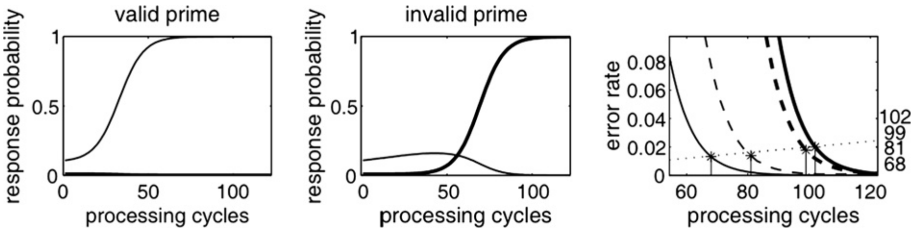

Fig. 3. *How the ASE model explains asymmetric blocking effects*. The two left-hand panels depict the response probability function over time (in processing cycles) for alternative responses in the valid (repetition) prime condition and the invalid (unrelated control) prime condition. The rightmost panel shows the error curves (from left to right) for valid-primed targets in a pure block, valid-primed targets in a mixed block, invalid-primed targets in a mixed block, and invalid-primed targets in a pure block. The estimated optimal RT for each condition (corresponding to where the RT cost function intersects the error curves) is indicated along the right-hand axis, with the values 68, 81, 99 and 102, respectively. The slowdown of valid primed targets in a mixed block is therefore 13 cycles (81–68), and the speedup of invalid primed targets, three cycles (102–99). (See text for further explanations.)

ple) between the pure block and the mixed block is smaller for the unrelated-primed targets (102–99 = 3 cycles) than the repetition-primed targets (81–68 = 13 cycles).

In sum, the asymmetric blocking effect in naming comes about in the ASE model because the hard items (targets preceded by unrelated primes) are associated with steeper error curves than the easy items (repetition-primed targets). The steeper slopes themselves are produced by the presence of response conflict, i.e., the fact that the response to the prime is different from the response to the target (and hence incorrect) in the case of targets preceded by unrelated primes.

4. Experiment 3

To summarize the findings so far, Experiments 1 and 2 have shown that the stimuli that produce repetition-proportion effects produce asymmetric blocking effects. Analysis of the properties of the ASE model suggested that the asymmetry comes about because the unrelated-primed targets are associated with steeper error curves, which constrain the amount of speedup because of a risk of a large increase in error rate. Thus, according to the ASE model, what is critical to observing the proportion effect is the proportion of *unrelated* prime trials (trials involving response conflict).

Experiment 3 tested the idea that a high proportion of repetition-primed trials is not *necessary* for a proportion effect to occur. According to the ASE model, a proportion effect should be observed as long as the easy and hard items differ in difficulty, with the hard items having steeper error curves than the easy items. Experiment 3 set up this condition by replacing the repetition trials of Experiment 1 with high-frequency words preceded by neutral primes (e.g., *%&?&%-BOARD*). High-frequency words should be responded to faster than low-frequency words, that is, these targets differ in difficulty. Further, because neutral primes cannot be pronounced, these primes would not engender response conflict, whereas the (initial-phoneme-matched) unrelated primes (e.g., *boost-BADGE*) have been assumed to engender response conflict. These stimuli were presented in two blocks: one containing a high proportion (0.8) of high-frequency words and the other containing a low proportion (0.2) of high-frequency words. The high-proportion block would be easier than the low-proportion block, that is, a main effect of block type is expected. The critical prediction concerned the interaction between these two factors. The ASE model predicts an interaction in which the unrelated-primed low-frequency word targets would not speed up as much as the neutral-primed high-frequency word targets, on the basis that these unrelated trials are associated with steeper error curves and hence cannot speed up as much without a large increase in error rate. In contrast, if the proportion effect is due to prime validity (usefulness of the prime), there is no reason to expect an interaction, because the easy block does not contain a higher proportion of valid trials.

4.1. Method

4.1.1. Subjects

Twenty volunteer undergraduate psychology students from Macquarie University participated in Experiments 3 for course credit.

4.1.2. Design

Each subject named 200 words in two blocks containing 100 words each. The experiment constituted a 2 (*Target type*: high-frequency words vs. low-frequency words) × 2 (*Block type*: high vs. low proportion of high-frequency words) factorial design, with both factors manipulated within subjects. Manipulation of target type was completely related to that of prime type, as all high-frequency words were primed by neutral primes (e.g., *%&?&%-BOARD*) and the low-frequency words were primed by unrelated primes (e.g., *boost-BADGE*). The dependent variables were naming latency and error rate.

4.1.3. Materials

The stimulus materials used in this experiment were 100 high-frequency words and 100 low-frequency words. All stimuli were five letters long. The high-frequency words had a frequency range between 81 and 672 per million (mean 208.89), and each was preceded by a neutral prime (%&?&%). Examples are *BOARD, CHILD, PLACE*. The low-frequency words were 100 of the low-frequency words used in Experiment 1. The low-frequency word targets were each preceded by the unrelated primes used in Experiment 1, namely, words that shared just the initial phoneme with the target (e.g., *boost-BADGE*).

The construction of high-proportion block (consisting of 80 high-frequency word targets and 20 low-frequency word targets) and low-proportion block (consisting of 20 high-frequency word targets and 80 low-frequency word targets) was similar to Experiment 1. Each block was designed as 20 sequences consisting of five trials. Within a sequence, the items were arranged so that in the high-proportion block, a sequence consisted of three high-frequency word targets followed by a fourth high-frequency word target, then a low-frequency word target. In the low-proportion block, a sequence consisted of three low-frequency word targets followed by a fourth low-frequency word target then a high-frequency word target. The 100 high-frequency words and 100 low-frequency words were each divided into five matching sets, and the assignment of sets to the high- and low-proportion conditions was counterbalanced across subjects so that each set occurred in each condition equally often (and more than once) across subjects. The order of 20 sequences within a block varied randomly across subjects. The order of blocks (high-proportion first or low-proportion first) was also counterbalanced across subjects. Thus, along with the counterbalancing of sets to conditions meant that full counterbalancing was achieved across every 10 subjects.

4.1.4. Apparatus and procedure

Apparatus and the testing procedure were identical to Experiment 1.

4.1.5. Results

The preliminary treatment of trials was identical to Experiment 1. The 3 *SD* replacement procedure affected 1.25% of the trials. Target naming latencies were analyzed using a three-way ANOVA with *Target type* (high vs. low frequency), *Block type* (high vs. low proportion), and *Block order* (high-proportion first vs. low-proportion first) as factors. The first two were within-subject factors, and *Block order* was a between-groups factor. Effects were considered to be significant at the .05 level. Mean naming latencies and error rates are presented in Table 3.

For latency, the main effect of *Target type* was significant, $F(1,18) = 6.32$, $\mathrm{MSe} = 296.28$, as was the main effect of *Block type*, $\mathrm{F}(1,18) = 5.21$, $\mathrm{MSe} = 850.16$. Critically, there was an interaction between these factors, $F(1,18) = 8.60$, $\mathrm{MSe} = 181.03$, indicating a proportion effect. This reflected the fact that the speedup in the high-proportion block was more limited for the unrelated-primed low-frequency word targets (6 ms) than the neutral-primed high-frequency words targets (24 ms). In addition, there was an interaction between *Target type* and *Block order*, $F(1,18) = 4.68$, $\mathrm{MSe} = 296.28$, reflecting greater stimulus type/prime type effect for the group who did the high-proportion block first. None of other main or interaction effects reached significance, all $F(1,18) < 2.29$, $p > .15$.

For error rate, none of the effects reached significance, $F(1,18) < 2.53$, $p > .13$ in all cases.

4.2. Discussion

The results of Experiment 3 showed that the stimulus/prime type effect (neutral-primed high-frequency words vs. unrelated-primed low-frequency word) was magnified in the block containing a high proportion of easy items (high-frequency targets). This interaction reflected the fact that the speedup of hard items (low-frequency words preceded by unrelated primes, i.e., the same “hard” items as in Experiments 1 and 2) was more limited than that for the easy items. This pattern is consistent with the ASE account, and shows that greater prime validity (high proportion of repetition trials) is not a necessary condition for observing the modulation of masked-priming effect by block type.

5. Experiment 4

Experiment 4 tested whether manipulation of proportion of repetition prime trials is *sufficient* to produce a proportion effect. Experiment 4 used the same high-frequency words and low-frequency words used in Experiment 3, but in this experiment, the high-frequency words were preceded by repetition primes (e.g., *board-BOARD*), and the low-frequency words were preceded by neutral primes (e.g., *%&?&%-BADGE*). These stimuli were presented in two blocks: one containing a high proportion (0.8) of high-frequency words and the other containing a low proportion (0.2) of high-frequency words. The critical prediction concerned the interaction between the block proportion and target type. If proportion effects are due to prime validity, on the basis that the high-proportion block contains more repetition-prime trials, it would be expected that the repetition priming effect should be magnified in this block. In contrast, the ASE model account predicts only the main effects of stimulus/prime type and block type and no interaction, because the slope of error curves for the two stimulus types would be similar, as neither the repetition nor neutral primes engender response conflict.

5.1. Method

5.1.1. Subjects

Twenty volunteer undergraduate psychology students from Macquarie University participated in Experiments 4 for course credit.

5.1.2. Design

Each subject named 200 words in two blocks containing 100 words each. The experiment constituted a 2 (*Target type*: high-frequency words vs. low-frequency words) $\times$ 2 (*Block type*: high vs. low proportion of high-frequency words) factorial design, with both factors manipulated within subjects. Manipulation of target type was completely related to that of prime type, as all high-frequency words were primed by repetition primes (e.g., *board-BOARD*) and the low-frequency words were primed by neutral primes (e.g., *%&?&%-BADGE*). The dependent variables were naming latency and error rate.

5.1.3. Materials

The target words used in this experiment were the 100 high-frequency words and 100 low-frequency words used in Experiment 3. The high-frequency word targets were preceded by repetition primes (e.g., *board-BOARD*), and low-frequency word targets were preceded by neutral primes (e.g., *%&?&%-BADGE*).

The construction of high-proportion block (consisting of 80 high-frequency word targets and 20 low-frequency word targets) and low-proportion block (consisting of 20 high-frequency word targets and 80 low-frequency word targets) was identical to Experiment 3.

5.1.4. Apparatus and procedure

Apparatus and the testing procedure were identical to Experiment 1.

5.2. Results

The preliminary treatment of trials was identical to previous experiments. The 3 *SD* cutoff replacement procedure affected 1.55% of the trials. Target naming latencies were analyzed using a three-way ANOVA with *Target type* (high vs. low frequency), *Block type* (high vs. low proportion), and *Block order* (high-proportion first vs. low-proportion first) as factors. The first two were within-subject factors, and *Block order* was a between-groups factor. Effects were considered to be significant when significant at the .05 level. Mean naming latencies and error rates are presented in Table 4.

For latency, the 51 ms main effect of *Target type* was significant, $F(1,18) = 77.96$, $\text{MSe} = 655.57$. None of other main or interaction effects reached significance, all $F(1,18) < 2.21$, $p > .15$. Critically, the interaction between *Block type* and *Target type* was non-significant, $F(1,18) < 1.0$, nor did it interact with Block order, $F(1,18) < 1.0$.

For error rate, none of the effects reached significance, $F(1,18) < 1.57$, $p > .22$ in all cases.

5.3. Discussion

The results of Experiment 4 showed no proportion effect (interaction between stimulus/prime type and block type). This is at odds with the idea that proportion effects are due to prime validity (usefulness). The absence of the interaction cannot be due to a weak manipulation of prime type, because the priming effect was highly significant and numerically it was much larger than the difference between the two prime types in Experiment 3. (Note that the larger effect is expected from the fact that in Experiment 4, the comparison involved repetition primes and neutral primes, whereas in Experiment 3 the comparison was between neutral primes and initial-phoneme matched unrelated primes, which is typically smaller in the naming task.) In contrast, the absence of the proportion effect is entirely consistent with the ASE model, because in this experiment neither prime type engendered response conflict, and hence the error curve slopes should be similar.

Table 3
Mean naming latencies (RT, in ms, with standard deviation in parentheses) and percent error rates (%E) in Experiment 3

Target type and example	Block type (proportion of high-frequency targets)					
	High		Low		Difference	
	RT	%E	RT	%E	RT	%E
High-frequency %&?&%-*BOARD*	493 (77)	3.3	518 (85)	4.8	25	1.5
Low-frequency *boost-BADGE*	513 (98)	5.8	521 (96)	4.3	8	−1.5
Frequency/priming effect	20	2.5	3	−.5		

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Table 4
Mean naming latencies (RT, in ms, with standard deviation in parentheses) and percent error rates (%E) in Experiment 4

Target type and example	Block type (proportion of high-frequency targets)					
	High		Low		Difference	
	RT	%E	RT	%E	RT	%E
High-frequency *board-BOARD*	456 (105)	2.8	469 (85)	3.3	13	0.5
Low-frequency %&?&%-BADGE	506 (92)	3.5	520 (78)	2.0	14	−1.5
Frequency/priming effect	50	0.7	51	−1.3		

6. General discussion

The findings of the present study are summarized as follows. Experiment 1 replicated Bodner and Masson's (2004) repetition-proportion effect (termed the "prime-validity effect" by Bodner and Masson) using the repetition (e.g., *badge-BADGE*) and (initial-phoneme-matched) unrelated primes (e.g., *boost-BADGE*) similar to those used by Bodner and Masson. Experiment 2 presented these stimuli using a blocking manipulation (pure block of repetition trials, pure block of unrelated trials, a mixed block with an equal number of repetition and unrelated trials). Unlike the symmetric blocking effect typically observed with the naming task in which the easy items slow down and hard items speed up by equal amounts in a mixed block, the repetition trials slowed down but the unrelated trials remained unchanged when the two types of trials were mixed together. Mozer et al.'s (2004) ASE model predicts these two effects – repetition-proportion effect and an asymmetric blocking effect – to go together, as both are explained in terms of an adaptation process which is guided by the statistics of recent trials. Essentially, when best to respond to an item is guided by RT for recent trials, moving towards the RT of recent trials. Thus, when easy (repetition prime) trials are mixed with hard (unrelated prime) trials, they slow down, and when hard trials are mixed with easy trials, they speed up. Both the asymmetric pattern of blocking effect, and the greater speedup of repetition trials than unrelated trials are explained by an additional assumption of steeper error curves for targets preceded by unrelated primes which engender response conflict. Steeper error curves limit the amount of speedup because responding earlier risks a large increase in error rate. Simulation results supported this account.

Experiments 3 and 4 tested further predictions of the ASE account. According to the account, a high proportion of repetition trials is neither necessary nor sufficient to observe an increase in the size of masked-priming effects. Experiment 3 used the same low-frequency words preceded by unrelated primes used in Experiment 1 as the hard items and replaced the repetition trials used in Experiment 1 with high-frequency words preceded by neutral primes (e.g., *%&?&%-BOARD*). This experiment showed a proportion effect, with the neutral trials (easy items) showing greater speed up in the easy environment (the block containing a high proportion of neutral prime trials) than the unrelated-primed trials (hard items). This result indicated that a high proportion of repetition trials is not *necessary* to observe a proportion effect. In

Experiment 4, the same high-frequency word targets and low-frequency word targets were used, but this time, the high-frequency words were preceded by repetition primes (e.g., *board-BOARD*) and the low-frequency words were preceded by neutral primes (e.g., *%&?&%-BADGE*). The ASE model predicted no proportion effect in this experiment, because neither the repetition prime nor neutral prime engender response conflict and hence the slope of error curves of these two types of stimuli would be similar. This can be contrasted with the view that the effect arises from list-wide prime validity (usefulness). The results of Experiment 4 showed no proportion effect, indicating that a high proportion of repetition trials (prime validity) is not *sufficient* to observe a proportion effect. These results show that the proportion effect with masked repetition primes is not a function of list-wide prime validity. Instead, the results are entirely compatible with the ASE account, together with the assumption that the unrelated prime trials have steeper error curves and hence more limited scope for a speedup.

6.1. Absence of prime-validity effect in other tasks

Bodner and Masson (2004) noted that a prime-validity effect has not always been found in studies that used a proportion manipulation. With Dutch stimuli, Brysbaert (2001) reported that the accuracy of identification of a degraded target (e.g., *IEP*) was facilitated by a homophone prime (e.g., *ieb*) relative to an unrelated prime (e.g., *gad*), but that the amount of facilitation was the same whether 0.72 or 0.14 of the trials involved homophone primes. Similarly, (also using Dutch stimuli) Pecher, Zeelenberg, and Raajmakers (2002) found that the increase in target identification following a masked associative prime (relative to an unrelated prime) was not reliably greater when 0.9, rather than 0.1 of the trials contained associatively related primes.

The memory recruitment account does not have a ready explanation for why these studies, unlike those by Bodner and Masson (2001,2004) were insensitive to the proportion manipulation. In contrast, the absence of a proportion effect in these studies is entirely expected by the ASE model, on the basis that a different dependent variable, namely, accuracy of identification, was used in these studies. The ASE model explains repetition-proportion effect in terms of adaptation of response-initiation process of *speeded* responses (which governs RT) to the statistics of the environment. When the task does not require speeded responding, as in the perceptual identification task used in the above studies, there is no basis to expect this adaptation to the stimulus environment to occur, hence no proportion effect is expected.[5]

[5] In addition to these studies which used accuracy as the dependent measure, studies using RT (in a lexical decision task) as the dependent measure (Grossi, 2006; Perea & Rosa, 2002) reported failures to replicate the modulation of associative priming effect as a function of proportion of valid primes reported by Masson and Bodner (2003). As we describe in the next section, findings of proportion effect in the lexical decision task have been mixed (cf. Bodner & Masson, 2001). As associative priming effects are generally smaller in size than repetition priming effects, the null finding of proportion effect with this manipulation may be due to the weakness of difficulty manipulation.

6.2. Prime-validity effect in other speeded tasks

Bodner and colleagues (Bodner & Dypvik, 2005; Bodner & Masson, 2001) have reported finding prime-validity effects in speeded tasks other than naming. In a parity (odd/even) judgment task, Bodner and Dypvik (2005) reported that masked-priming effect due to parity congruence (faster response to an odd/even number target preceded by a prime of the same parity, e.g., 1–3, relative to a target preceded by prime of different parity, e.g., 8–1) was greater in a block containing a higher proportion of congruent prime trials. We (Kinoshita, Mozer, & Forster, submitted for publication) replicated the proportion effect (provided that the manipulation of difficulty was sufficiently large) and offered an account of this effect in terms of the ASE model using the same framework adopted here. In brief, the incongruent primes cause conflict at the response level (see Damian, 2001; Dehaene et al., 1998 for evidence), and hence they are associated with steeper error curves, which limit the amount of speedup. Consistent with the prediction of the ASE model, we found that increasing the proportion of easy items (neutral-primed items), without increasing prime validity, magnified the size of congruence effect (provided that the manipulation of item difficulty was sufficiently large, also consistent with the ASE prediction).

Bodner and Masson (2001) also reported finding prime-validity effects in the lexical decision task. From the perspective of the ASE model, it is not clear that the effect is expected in this task. This is because currently there is little evidence that masked repetition priming effects in this task engender response conflict: For example, Perea, Fernandez, and Rosa (1998) found no response congruence effect (i.e., whether or not the prime belonged to the same response category – word or nonword – as the target did not modulate priming). Another reason why the ASE framework does not predict a repetition-proportion effect in this task is that masked repetition priming effects are generally weak or absent for nonword targets. Increasing the proportion of repetition trials therefore represents a weak manipulation of stimulus environment in this task (because it has an effect on only half of the trials). It is therefore likely that the prime-validity effect in the lexical decision task has a basis other than the adaptation mechanism assumed by the ASE. In any event, it should be noted that in Bodner and Masson (2001), the prime-validity effect was not present in all experiments: for example, the effect was absent when high- and low-frequency word targets were drawn from discontinuous bands of word frequencies (Experiment 2), or when only high-frequency words were used as word targets (Experiment 5A). At present, it is not entirely clear what the critical parameters are for finding the prime-validity effect in this task. Delineating the boundary conditions for finding the prime-validity effect in this task would be an important first step in providing an account for the effect.

6.3. Conclusion

The present study replicated, and provided an alternative explanation for Bodner and Masson's (2004) finding that the proportion of repetition trials modulates the size of masked repetition priming effect in naming. Unlike the memory recruitment account put forward by Bodner and Masson, suggesting that reliance on the *prime* is changed by prime validity, the ASE account presented here explains the proportion effect in terms of adaptation of speeded responses to the *target* to the difficulty of recent trials. As such, the ASE account removes the mystery of how masked-priming effects – effects of stimuli not available to consciousness and hence are assumed to reflect an automatic process – are amenable to what on the surface look like strategic influences. We take this to argue that unconscious cognition is not as smart as suggested by the memory recruitment account.

Appendix A

To understand how the repetition-proportion effects can be explained by the ASE model, consider a simplified account that incorporates the essential claim of the model: the current stimulus provides only unreliable evidence about when a response should be initiated; to increase reliability, evidence from recent trials are combined with evidence from the current trial. To make a *simplified* version of the ASE model that can be explored analytically, suppose that the observed response time on a given trial depends both on an *intrinsic* difficulty level associated with a target type (i.e., easy or hard) and on the recent history of intrinsic difficulties. To be concrete, we can express the intrinsic difficulty as a response time that would be appropriate for a trial of a given difficulty level. If RT_e and RT_h are intrinsic response times for easy (repetition prime) and hard (unrelated prime) trials, respectively, then the historical average of intrinsic response times in a block containing a proportion p of easy trials and $1-p$ hard trials, is expected to be

$$\overline{\mathrm{RT}}(p) = p\mathrm{RT}_\mathrm{e} + (1-p)\mathrm{RT}_\mathrm{h}$$

On a particular trial of prime type x in a block containing a proportion p of easy trials, the actual response time is a weighted combination of RT_x and RT

$$\mathrm{RT}(x,p) = \theta\overline{\mathrm{RT}}(p) + (1-\theta)\mathrm{RT}_x \tag{1}$$

where $\theta \in [0,1]$ is an averaging constant that determines the relative weighting of the recent history. In a block containing a proportion p of easy trials, the priming effect is

$$\mathrm{RT}(h,p) - \mathrm{RT}(e,p) = (1-\theta)(\mathrm{RT}_\mathrm{h} - \mathrm{RT}_\mathrm{e}) \tag{2}$$

Because this priming effect does not depend on p, it will not differ between low-proportion ($p = .2$) and high-proportion ($p = .8$) blocks. Thus, this model fails to predict a repetition-proportion effect. However, a minor extension of the model does. The modification involves the assumption that intrinsically fast RTs regress toward the mean *more* than do intrinsically slow RTs, i.e., θ in Eq. (1) must depend on the trial type (easy or hard)

$$\mathrm{RT}(x,p) = \theta_x \overline{\mathrm{RT}}(p) + (1-\theta_x)\mathrm{RT}_x \tag{3}$$

such that $\theta_e > \theta_h$. Under this condition, the priming effect (Eq. (2)) becomes

$$\mathrm{RT}(h,p) - \mathrm{RT}(e,p) = (1 - p\theta_h - (1-p)\theta_e)(\mathrm{RT}_h - \mathrm{RT}_e) \tag{4}$$

and a repetition-proportion effect is obtained so long as

$$\mathrm{RT}(h,0.8) - \mathrm{RT}(e,0.8) > \mathrm{RT}(h,0.2) - \mathrm{RT}(e,0.2) \tag{5}$$

Substituting Eq. (4) into Eq. (5), we obtain

$$(1 - 0.8\theta_h - 0.2\theta_e)(\mathrm{RT}_h - \mathrm{RT}_e) > (1 - 0.2\theta_h - 0.8\theta_e)(\mathrm{RT}_h - \mathrm{RT}_e) \tag{6}$$

which is satisfied under the key assumption $\theta_e > \theta_h$.

What makes this model interesting is that in addition to addressing proportion manipulations, it also predicts basic blocking effects as well as subtle features of the blocking effect. Specifically, the model allows us to compare the slow-down of an easy item in a mixed block relative to pure block, RT (e, 0.5) – RT (e, 1.0), against the speed-up of a hard item in a mixed block relative to a pure block, RT (h, 0.0) – RT (h, 0.5). The ratio is:

$$\frac{\mathrm{RT}(e,0.5) - \mathrm{RT}(e,1.0)}{\mathrm{RT}(h,0.0) - \mathrm{RT}(h,0.5)} = \frac{\theta_e}{\theta_h}$$

When $\theta_e = \theta_h$, slow down and speed up are comparable, and a symmetric blocking effect is predicted. However, when $\theta_e > \theta_h$, an asymmetric blocking effect is predicted, with more slow-down of easy items than speed-up of hard items. To summarize, this simple model predicts that the condition giving rise to a greater speedup of easy items in the fast environment must also yield an asymmetric blocking effect.

The ASE model makes the same prediction as the simple model, which is not surprising given that they operate by the same principle of determining when to respond on a given trial by incorporating evidence provided by recent trials. The key difference between the ASE model and the simple model is that the evidence used by the simple model is a scalar indicating intrinsic item difficulty, whereas ASE utilizes a time-varying trace of estimated accuracy. It should also be noted that θ_e and θ_h are not parameters of the true ASE model, but they represent the effect on RTs of parameters of the model. (The true ASE model cannot control the weighting constant separately for easy and hard items, because it does not know whether the current trial is an easy or hard item.) For the present work, the important claim is that manipulations that give rise to a repetition-proportion effect should also produce an asymmetric blocking effect.

The ASE model itself is silent with regards *why* the repetition and unrelated trials produce the asymmetric blocking effect. This issue will be explored, and discussion of likely factors will be taken up after Experiment 2. For now, what is relevant is that given the assumption $\theta_e > \theta_h$, the ASE model predicts that when the same items are used in a blocking manipulation, the same stimuli would show an *asymmetric* blocking effect such that the hard items (unrelated trials) would show a more limited speedup than the slowdown produced by the easy items (repetition trials) in a mixed block.

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