

Subliminal Number Priming Within and Across the Visual and Auditory Modalities

Sid Kouider^{1,2} and Stanislas Dehaene^{2,3}

¹Laboratoire des Sciences Cognitives et Psycholinguistique, EHESS/CNRS/ENS-DEC, Paris, France

²Cognitive Neuroimaging Unit, NeuroSpin Center, INSERM/CEA, Gif-sur-Yvette, France

³Collège de France, Paris, France

Abstract. Whether masked number priming involves a low-level sensorimotor route or an amodal semantic level of processing remains highly debated. Several alternative interpretations have been put forward, proposing either that masked number priming is solely a byproduct of practice with numbers, or that stimulus awareness was underestimated. In a series of four experiments, we studied whether repetition and congruity priming for numbers reliably extend to novel (i.e., unpracticed) stimuli and whether priming transfers from a visual prime to an auditory target, even when carefully controlling for stimulus awareness. While we consistently observed cross-modal priming, the generalization to novel stimuli was weaker and reached significance only when considering the whole set of experiments. We conclude that number priming does involve an amodal, semantic level of processing, but is also modulated by task settings.

Keywords: subliminal, priming, cross-modal, numerosity

What is the depth of processing of subliminal stimuli? While there is now little disagreement regarding the existence of unconscious perceptual processes, the participation of higher levels remains somewhat controversial (see Kouider & Dehaene, 2007, for an extensive review). More specifically, whether subliminal perception conveys semantic information has not yet been fully resolved. Although several neuroimaging studies have provided strong evidence for brain events associated with semantic-level processing of masked or blinked words (Gaillard et al., 2006; Kiefer, 2002; Luck, Vogel, & Shapiro, 1996), behavioral studies using masked priming have yielded debated results. Claims for semantic-level processing (Naccache & Dehaene, 2001a, 2001b; Reynvoet, Gevers, & Caessens, 2005) have been contested on grounds of lower-level interpretations (e.g., sensorimotor associations; Damian, 2001) or of partial prime awareness (Abrams & Grinspan, 2007; Kouider & Dupoux, 2007). In this study, we will address these aspects in the number domain which, as we shall review below, has been more promising than the domain of words in providing *some evidence in favor* of subliminal processing at higher levels of processing. In particular, we will test, through a cross-modal manipulation, whether subliminal number priming extends beyond perceptual domains. Before presenting our study, we review some of the key issues that have been outlined as confounds in demonstrating subliminal semantic priming.

The very existence of subliminal perception has remained controversial since the very first days of experimental psychology (see Kouider & Dehaene, 2007). After more than a century of research, full of replication failures,

experimental artifacts, and awareness underestimation issues, two independent studies provided several methodological improvements allowing for an unequivocal demonstration of subliminal influences (Dehaene et al., 1998; Greenwald, Draine, & Abrams, 1996). Greenwald and colleagues used an affective evaluation task where subjects classified target words as pleasant (e.g., “happy”) or unpleasant (e.g., “vomit”), and these words were preceded either by a congruent prime (i.e., a word from the same category, such as “love” preceding the target “happy”) or by an incongruent prime (“vomit” preceding “happy”). Subjects were faster for congruent trials compared to incongruent trials, even under conditions where they could not perform the affective evaluation on the prime, evidencing a semantic congruity priming effect in the absence of awareness. Dehaene and colleagues provided a similar demonstration in the number domain. In their study, subjects were asked to classify target numbers, presented as written word forms or in Arabic notation, as either smaller or larger than 5. These visible numbers were preceded by masked number primes that were also smaller or larger than 5 but that participants were unable to consciously detect. Subjects were faster when both the prime and the target belonged to the same category than when they belonged to opposite categories. In addition, using fMRI and ERPs, they found that subliminal stimuli can not only elicit a behavioral influence, but also neural activity in the motor cortex due to response competition. In addition, cross-notation (e.g., from Arabic digit to number word) repetition suppression was also observed in the bilateral intraparietal cortex, a region associated with

77 semantic-level number processing (Naccache & Dehaene,
78 2001a). Thus, by the end of the second millennium, the
79 issue of the existence of unconscious perception appeared
80 to be resolved with a positive outcome.

81 Nevertheless, it did not take long before other studies
82 (Abrams & Greenwald, 2000; Damian, 2001) revealed that
83 congruity effects, although they appear to be genuinely sub-
84 liminal, could be totally reframed and subsumed a nonse-
85 mantic interpretation. Because the two former studies by
86 Dehaene et al. (1998) and Greenwald et al. (1996) used a
87 restricted set of stimuli appearing several times both as
88 primes and as targets, response congruity effect could be as
89 well reflecting conflicting stimulus-response associations
90 (e.g., the prime 4 has been previously associated with the left
91 hand, while the prime 9 has been associated with the right
92 hand, resulting in a motor response conflict) rather than com-
93 petition between semantic categories. Damian (2001) asked
94 subjects to classify words in terms of the physical size of
95 the object they represented in reference to a 20 × 20 cm
96 frame (e.g., “spider” was smaller while “house” was larger).
97 Damian found that subliminal congruity effects were
98 restricted to practiced primes, that is to prime stimuli that
99 have previously been mapped to a response during the exper-
100 iment. Unpracticed primes did not give rise to any congruity
101 effect. Damian (2001) argued that the finding of Dehaene
102 et al. (1998) could be best interpreted in terms of the direct
103 motor specification hypothesis (Neumann & Klotz, 1994)
104 according to which subliminal congruity effects do not need
105 to be mediated by the semantic level. Instead, they reflect the
106 unconscious triggering of a motor response that has been
107 associated, through learning, with a given stimulus. Abrams
108 and Greenwald (2000) showed that subliminal priming not
109 only does not generalize to novel words, as in Damian’s
110 study, but also that it results from a learned association
111 between fragments of the word primes and a response. For
112 instance, in an affective evaluation task where the target
113 words “smut” and “bile” were repeatedly classified as
114 unpleasant, subliminal presentation of the prime word
115 “smile” (made of smut and bile) initiated an unpleasant
116 response (conversely for “tumor” which initiated a pleasant
117 response following practice with “tulip” and “humor”).

118 Yet, recent studies have shown that the sensorimotor
119 interpretation cannot be the whole story. Indeed, Naccache
120 and Dehaene (2001a) found that subliminal number priming
121 occurred not only for practiced primes (the numbers 1, 4, 6,
122 and 9) but also by generalizing to unpracticed primes (2, 3,
123 7, and 8), although the latter led to a weaker effect. The fact
124 that priming can extend to prime stimuli that have never
125 been seen as targets suggests that number priming is medi-
126 ated, at least in part, by semantic representations. Other stud-
127 ies (Greenwald, Abrams, Naccache, & Dehaene, 2003;
128 Reynvoet et al., 2005) have replicated this generalization
129 to unpracticed numbers, although here also not without trig-
130 gering some controversies. Indeed, Greenwald et al. used
131 two-digit numbers to be compared to 55 and found that,
132 depending on instructions and task context, subjects at times
133 extracted the meaning of a two-digit number prime, and at
134 other times treated the digits independently, sometimes
135 resulting in paradoxical fragment-based effects. For
136 instance, after practice with conscious trials in which the

digit 6 was seen when classifying 56 as greater than 55, 137
the masked prime 16 facilitated the greater-than-55 138
response. Reynvoet et al. have been criticized for not being 139
cautious enough regarding stimulus awareness (assessed in 140
participants that did not participate in the priming experi- 141
ments), their finding being then interpreted as potentially 142
reflecting supraliminal rather than genuinely subliminal 143
number processing by other researchers (see Elsner, Kunde, 144
& Kiesel, 2008; Kunde, Kiesel, & Hoffmann, 2005). In 145
addition, Elsner et al. (2008) recently found number priming 146
for practiced primes but without generalization to unprac- 147
ticed primes. Similar inconsistencies can be found in the 148
domain of words. Indeed, while the rule tends to be that 149
word priming is restricted to practiced primes, there also 150
exist a few exception studies showing that it can generalize 151
to unpracticed primes (Klauer, Eder, Greenwald, & Abrams, 152
2007; Van den Bussche & Reynvoet, 2007). As of today, it 153
remains unclear why some studies report a strong general- 154
ization to unpracticed primes while others only found 155
restricted effects. 156

157 The Present Study

158 The goal of the present study was twosome. First, we 159
wished to reassess whether one can obtain genuinely sub- 160
liminal number priming without experimental confounds 161
that are either due to residual stimulus awareness or due 162
to practice with the number stimuli. Our objective was to 163
reevaluate the seminal study by one of us (Dehaene et al., 164
1998) while following the rigorous methodological 165
approach to unconscious perception defended by the other 166
author (Kouider & Dupoux, 2001, 2004, 2007). Thus, we 167
decided to replicate the original finding while carefully 168
evaluating the generalization to novel primes, as well as 169
avoiding the possibility of any form of partial awareness. 170
A second objective was to establish whether the subliminal 171
analysis of masked number extends beyond the perceptual 172
domain by examining whether subliminal priming general- 173
izes across modalities. To our knowledge, the possibility 174
of cross-modal transfer in number priming has not been 175
tested so far. As such, we included a cross-modal (visual- 176
to-auditory) priming manipulation similar to the one devel- 177
oped by Kouider and Dupoux (2001) for words. Since the 178
original study by Kouider and Dupoux (2001), very few 179
studies have addressed the modality-specific or amodal 180
nature of unconscious processing. Yet, masked cross-modal 181
priming can be considered as a good index of the involve- 182
ment of higher-level representations. We return to these 183
aspects in the General Discussion section.

184 All of the present experiments use a method very similar 185
to that of Dehaene et al. (1998): Classification of target num- 186
bers 1, 4, 6, or 9 as larger or smaller than 5, where each target 187
is preceded by a numerical prime. Like Naccache and 188
Dehaene (2001a, 2001b), we used primes that were also 189
either from the target set 1, 4, 6, 9, or from the novel set 2, 190
3, 6, 7. We also incorporated a visibility manipulation 191
(masked vs. unmasked trials) and, crucially, cross-modal 192
trials with a visual prime and an auditory target. Importantly,

we also included here as many repetition trials (e.g., 6 → /six/) as congruent (e.g., 9 → /six/) and incongruent trials (e.g., 4 → /six/), resulting in one third of each type of trial, in order to evaluate independently the presence of repetition and response congruity effects. To fully separate these two effects, we compared the repeated trials with the nonrepeated congruent trials (thus providing a pure measure of the repetition effect, uncontaminated by response congruity), and, separately, the nonrepeated congruent with the nonrepeated incongruent trials (thus providing a pure measure of the response congruity effect uncontaminated by stimulus repetition). The distinction between these two effects is particularly important for cross-modal trials. Finding a cross-modal response congruity effect for practiced primes might still be attributed to direct sensorimotor pathways, separately converging onto the same motor preparation system from visual and from auditory input systems, without requiring any cross-modal semantic transfer. Cross-modal repetition priming, on the other hand, provides strong evidence for a locus of subliminal processing that extends beyond the perceptual domain. Experiment 1 is very close to a replication of Dehaene et al. (1998), with the addition of cross-modal trials. Experiment 2 introduces unpracticed primes, as well as a more rigorous method for assessing visibility. Experiment 3 uses only masked primes, in order to overcome potential limitations related to the visibility of supraliminal unpracticed primes. Finally, Experiment 4 relies on a different masking procedure for cross-modal trials, in order to avoid any potential influence of visibility on cross-modal priming.

Experiment 1

Method

Participants

A total of 11 university students recruited in Paris took part in this experiment. For this and all subsequent experiments, all participants had normal or corrected-to-normal vision, were aged between a minimum of 18 years and a maximum of 35 years, were native speakers of French, and were naive regarding the purposes of the experiment.

Stimuli and Apparatus

The stimuli were primarily constituted of numbers and masking stimuli. The numbers were 1, 4, 6, and 9, and were presented either as Arabic digits (e.g., 6), as French written words in uppercase letters (e.g., SIX) or as French spoken words by a male voice (e.g., /sis/). The masks were letter strings that are illegal in French and were constructed by randomly combining 6 upper- and lower-case consonant letters (e.g., mCzTrG). Visual events were presented in a white fixed-width font (i.e., Courier) against a black background and covered up the central area of a CRT monitor (70 Hz refresh rate) at a distance of about 60 cm from the

participant. Auditory stimuli were recorded by a male French native speaker, digitized on a PC computer using an OROS-AU22-A/D board, and presented to participants through headphones. The whole protocol was programmed and run with the EXPE software package (Pallier, Dupoux, & Jeannin, 1997).

Procedure and Design

On each trial, participants received a fixation cross, a forward mask, a prime, a backward mask, and a target (see Figure 1). The fixation cross appeared for 200 ms. The prime stimuli were presented only visually as Arabic or written word forms and for a 43 ms duration. The two masks temporally surrounded the prime differently during the masked and unmasked trials. For masked trials, the masks were presented for a duration of 57 ms (four screen refresh cycles). However, for unmasked trials, the prime was not directly surrounded by the masks (see Figure 1). Instead, it was surrounded by blank screens presented for 29 ms (two cycles) which were themselves surrounded by masks also presented for 29 ms. This procedure has the advantage of making the primes highly visible in these unmasked trials, as if they were popping out from the visual stream, while it also allowed us to keep the prime duration and the prime-target interval identical for both types of trials. During each trial, the backward and forward masks differed from each other and were constructed online by the experimental program. Following the backward mask, the target stimuli could appear in one of the three formats (as Arabic digit such as “6”, as a French written word such as “six”, or as a French auditory word /sis/). The prime was always a visual stimulus appearing either as an Arabic digit or as a written word. For trials with a visual target, the target duration was 200 ms. For auditory numbers, participants were presented with the auditory target along with a third visual mask consisting in a row of 6 hash marks (#####) and presented for 200 ms. This third mask was used because the preceding short backward mask on its own (i.e., without a visual target) was not strong enough to prevent prime visibility in the masked cross-modal trials (see Kouider & Dupoux, 2001). Thus, a trial could be masked or unmasked, and it could be within-notation (e.g., 6 → 6, SIX → SIX), cross-notation (e.g., 6 → SIX, SIX → 6), or cross-modal (e.g., 6 → /sis/, SIX → /sis/).

The experiment consisted in four successive blocks of 216 trials separated by a short break. Each block comprised either masked or unmasked trials. The order of the blocks could be either masked → unmasked → unmasked → masked, or it could be unmasked → masked → masked → unmasked. Whether subjects received the former or the latter block order was systematically alternated from one participant to the other. In addition, a separate small block of 12 training trials was performed prior to each block. In previous experiments (e.g., Dehaene et al., 1998), the experimental list was usually based on the full combination of the four primes and four targets, resulting in twice more incongruent trials than repetition or congruent (unrepeated) trials taken separately. Here, because we were as interested in

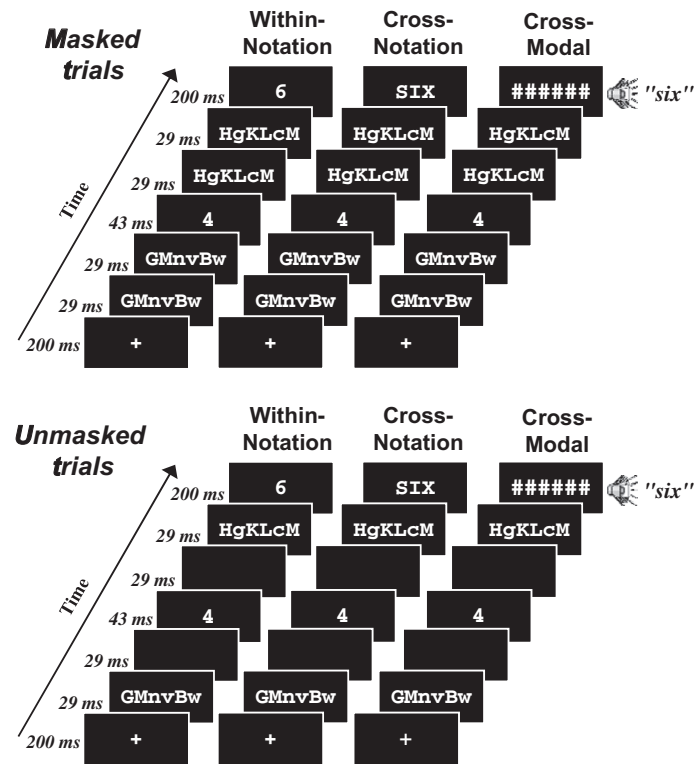


Figure 1. Schematic description of the priming method trials as a function of format change (within-notation, cross-notation, and cross-modal, from left to right) and masking (masked and unmasked trials, from top to bottom).

repetition priming (especially in the cross-modal condition) as in congruity priming, we equated the proportion of repetition, congruent, and incongruent trials (i.e., one third of the trials for each type of relation). We also ensure that participants received the same proportion of within-notation, cross-notation, and cross-modal trials, as well as the same proportion of primes corresponding to an Arabic digit or to a written word.

Participants were told that they would see or hear a target number between 1 and 9 (excluding 5), and that they would have to compare it to a fixed standard of 5. They were informed that prior to the target number, they would see some illegal letter strings and, on some trials (i.e., unmasked trials), a number flashed very briefly. They were instructed to ignore these preceding stimuli and concentrate only on the last event to perform the comparison task appropriately. Participants were instructed to make this decision as quickly and as accurately as possible. Performance was measured from a two-button response box in which participants used the left hand for numbers below 5 and the right hand for numbers above 5. Participants were forced to respond within 1,500 ms after the target onset, following which the next trial started with the fixation cross. The whole protocol for the main experiment lasted about 35 min.

Immediately after the main experiment, participants were explained that a number (i.e., the prime) actually preceded the target on each trial since the very beginning of the experiment. They were then instructed to perform the same task as in the main experiment (i.e., comparison to 5) now on

the prime and not on the target. Participants were instructed that they should focus primarily on accuracy, not on speed, and that they could now take as long as they wanted to respond. In order to familiarize participants with the new task, they first received a series of training trials ($N = 12$) where the prime was displayed for 200 ms under the same procedure as for masked trials. Then, they received two blocks of 64 trials randomly selected, both with the prime duration set back to normal speed (i.e., 43 ms) but with one block consisting of masked trials while the other was constituted of unmasked trials. The block order (masked trials first or unmasked trials first) was alternated from one participant to the other. In addition, each of these two blocks was preceded by another 12 training trials with the same respective display parameters.

Results and Discussion

Priming

Incorrect responses (12.75%) and reaction times (RTs) shorter than 100 ms (0.67%) or longer than 1,000 ms (0.52%) were excluded from the RT analysis. We first performed a $2 \times 3 \times 3$ analysis of variance (ANOVA) on median RTs by subject and by condition with the factors masking type (masked vs. unmasked), format change (within-notation, cross-notation, and cross-modal), and relation (repetition, nonrepeated congruent, and incongruent).

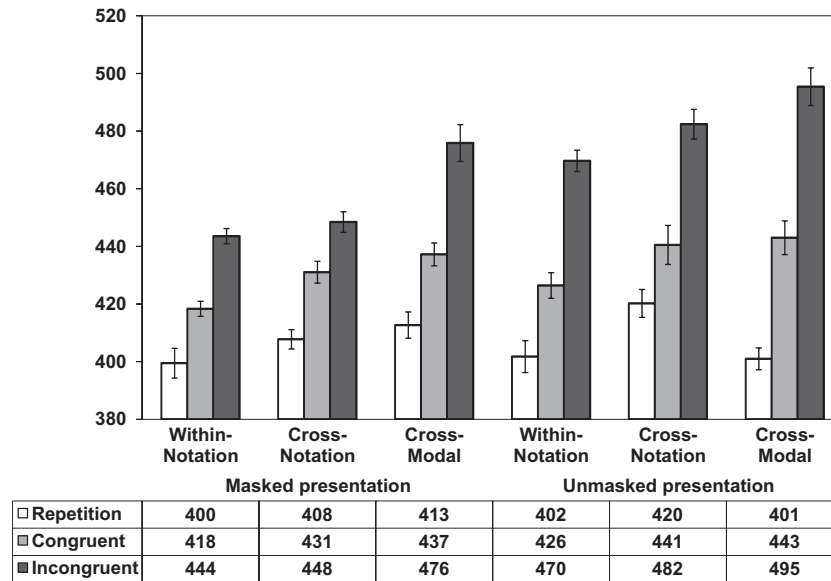


Figure 2. Average RTs for repetition, congruent, and incongruent trials in Experiment 1 as a function of format change and masking type. Error bars represent ± 1 standard error to the mean.

In the analysis below, we refer to global priming (i.e., potentially merging both repetition and congruity effects) as the difference between repetition and incongruent trials, repetition priming as the difference between repetition and nonrepeated congruent trials, and congruity priming as the difference between nonrepeated congruent and incongruent trials. The average median RTs are depicted in Figure 2. Unless otherwise stated, all the results reported in the analysis below are significant by having a p value below .05.

We first looked at main effects and found one of relation, $F(2, 20) = 55.30$, $p < .0001$, and one of format change, $F(2, 20) = 4.96$, $p < .05$. The main effect of masking type approached significance, $F(1, 10) = 4.63$, $p = .06$, with a 12 ms slowing for unmasked compared to masked trials. The relation factor interacted both with format change, $F(4, 40) = 4.74$, $p < .005$, and with masking type, $F(2, 20) = 4.63$, $p < .05$. None of the other interactions reached significance. We then performed planned comparisons by focusing primarily on the relation factor (collapsed across format change and masking type). We observed a significant effect for the three priming contrasts, that is not only for global priming, $F(1, 10) = 80.61$, $p < .0001$, but also when separately considering repetition priming, $F(1, 10) = 24.72$, $p < .001$, and congruity priming, $F(1, 10) = 42.54$, $p < .0001$. Only global priming revealed an interaction with masking type, $F(1, 10) = 9.57$, $p < .05$. Format change interacted with global priming too, $F(2, 20) = 7.66$, $p < .005$, and, in addition, with congruity priming, $F(2, 20) = 3.72$, $p < .05$. These interactions with format change resulted from the fact that global priming for cross-modal trials was higher than for both within-notation, $F(1, 10) = 7.48$, $p < .05$, and cross-notation trials, $F(1, 10) = 11.62$, $p < .01$, and from the fact that similarly congruity priming was also significantly higher for cross-modal trials compared to both

within-notation, $F(1, 10) = 5.87$, $p < .05$, and cross-notation trials, $F(1, 10) = 7.00$, $p < .01$.

We then split our analysis to separately study repetition and congruity priming as a function of masking type. The two types of priming were significant both for unmasked trials (repetition priming: $F(1, 10) = 13.55$, $p < .005$ and congruity priming: $F(1, 10) = 21.15$, $p < .001$) and, crucially, for masked trials (repetition priming: $F(1, 10) = 27.29$, $p < .0005$ and congruity priming: $F(1, 10) = 47.24$, $p < .0001$). Considered now separately as a function of masking type, none of the interactions between priming and format change described above reached significance, except for a greater masked congruity effect for cross-modal trials compared to cross-notation trials, $F(1, 10) = 5.62$, $p < .05$. Note also that unmasked repetition priming was also marginally greater for cross-modal compared to cross-notation trials, $F(1, 10) = 4.01$, $p = .07$.

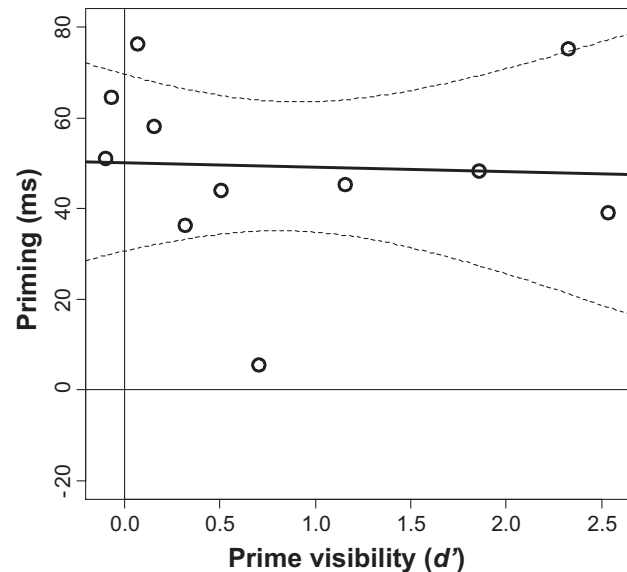
Finally, we further restricted our comparisons to priming effects as a function of both format change and masking type. This resulted in 12 contrasts that were all significant, except for one (unmasked cross-notation repetition priming) which fell short of significance ($p = .07$) (the detailed results are the following: unmasked within-notation repetition priming, $F(1, 10) = 8.06$, $p < .05$; unmasked within-notation congruity priming, $F(1, 10) = 27.47$, $p < .0005$; masked within-notation repetition priming, $F(1, 10) = 6.18$, $p < .05$; masked within-notation congruity priming, $F(1, 10) = 27.78$, $p < .0005$; unmasked cross-notation repetition priming, $F(1, 10) = 4.1365$, $p = .07$; unmasked cross-notation congruity priming, $F(1, 10) = 8.45$, $p < .01$; masked cross-notation repetition priming, $F(1, 10) = 14.12$, $p < .005$; masked cross-notation congruity priming, $F(1, 10) = 11.83$, $p < .01$; unmasked cross-modal repetition priming, $F(1, 10) = 17.00$, $p < .005$; unmasked cross-modal

422 congruity priming, $F(1, 10) = 22.26$, $p < .001$; masked
 423 cross-modal repetition priming, $F(1, 10) = 9.84$, $p < .01$;
 424 and lastly masked cross-modal congruity priming,
 425 $F(1, 10) = 19.61$, $p < .005$.

426 Prime Visibility

427 Participants' performances on the visibility measure were
 428 77.1% for unmasked trials and 57.7% for masked trials.
 429 We computed d' values for each participant as a function
 430 of masking type and format change by treating primes larger
 431 than 5 as signal and primes smaller than 5 as noise. For
 432 masked trials, the mean d' values across format change were
 433 0.30, 0.43, and 0.86 for within-notation, cross-notation, and
 434 cross-modal trials, respectively, while for unmasked trials,
 435 these values were 1.61, 1.43, and 2.52, respectively. When
 436 considering visibility as a function of format change,
 437 planned comparisons on unmasked trials revealed that the
 438 cross-modal trials led to significantly higher d' values com-
 439 pared both to cross-notation trials, $F(1, 10) = 18.15$,
 440 $p < .001$, and to within-notation trials, $F(1, 10) = 7.80$,
 441 $p < .05$, suggesting that unmasked cross-modal trials were
 442 more visible than the other format changes. However, none
 443 of the format change comparisons reached significance
 444 when considering masked trials (all $ps > .2$). Taken as a
 445 whole, the mean d' values were significantly different from
 446 zero both for unmasked trials ($d' = 1.85$; $t(10) = 6.71$,
 447 $p < .0001$) and for masked trials ($d' = 0.53$; $t(10) = 3.00$,
 448 $p < .05$), respectively. The effect of masking type was also
 449 significant, $F(1, 10) = 18.150$, $p < .001$, evidencing that
 450 the primes were more visible in the unmasked condition.
 451 Nevertheless, the fact that the d' values for masked trials
 452 were significantly higher than zero means that the primes
 453 in this situation were still somehow visible, at least for some
 454 of the participants. To deal with this recurrent problem,
 455 Greenwald, Klinger, and Schuh (1995) introduced a regres-
 456 sion method that allows to investigate whether priming is
 457 still reliable when extrapolated to null performance on the
 458 prime visibility measure. Figure 3 shows the regression of
 459 the global masked priming effect for each subject as a func-
 460 tion of prime visibility. This analysis revealed that, crucially,
 461 the intercept was large and highly significant (49 ms;
 462 $t(10) = 5.61$, $p < .0005$), suggesting the presence of a genu-
 463 inely subliminal locus for priming.

464 In sum, this first experiment suggests that both repetition
 465 and congruity priming occur not only for visible but also
 466 for subliminal number stimuli, although masking appears to
 467 reduce the magnitude of priming effects. In addition, we
 468 observed masked priming not only when the prime and target
 469 were in the same or in different notations within the visual
 470 domain, which is consistent with previous findings, but we
 471 also found both repetition and congruity priming when there



472 *Figure 3.* Regression of masked priming on prime
 473 visibility in Experiment 1. Each data point represents a
 474 participant. The regression functions (dotted lines indicate
 475 95% confidence intervals) show the association between
 476 the global priming effect found for masked stimuli and
 477 their prime visibility. Priming is interpreted as subliminal
 478 when the curve representing the lowest value in the
 479 confidence interval passes above the origin.

480 was a shift from the visual to the auditory modality. This latter
 481 result provides evidence that masked number priming
 482 involves amodal representations and extends its locus beyond
 483 the perceptual domain. Experiment 2 will address the question
 484 as to whether these amodal representations are of a semantic
 485 nature rather than involving other (e.g., episodic) types of
 486 information by introducing novel (i.e., unpracticed) number
 487 stimuli. In addition, we relied on a new, more rigorous mea-
 488 sure of prime visibility. Indeed, if participants' responses dur-
 489 ing the priming experiment are progressively influenced by
 490 partial (e.g., fragmentary) elements of the prime, through an
 491 overlearned stimulus-response mapping, then the numerical
 492 comparison task on the prime, which requires full identifica-
 493 tion of the prime stimulus, might actually underestimate prime
 494 visibility (see Abrams & Grinspan, 2007; Kouider & Dupoux,
 495 2007). Thus, we decided instead to rely on a two-alternative
 496 forced-choice (hereafter 2-AFC) in which participants must
 497 decide which one of two numbers displayed on the screen cor-
 498 responded to the prime in the preceding priming sequence.
 499 Here, if the participant relies on the identification of partial
 500 elements of the primes, then these elements are redisplayed
 501 in one of the two alternatives and they should, consequently,
 502 influence performance on the 2-AFC task.¹

¹ In addition, this new measure allowed to deal with a potential confound that might, conversely, overestimate prime visibility. Indeed, the inclusion of an equal number of repetition trials leads to a higher proportion of congruent trials collapsed across repeated and nonrepeated prime-target pairs. Thus, there were more trials in which response to the prime and response to the target were the same. As such, a participant who does not see any of the primes, but who responds on the basis of the magnitude of the target, that participant would actually be better than chance on the prime visibility measure used in Experiment 1. The new visibility measure performed in Experiment 2 allows to avoid this confound since the identity of the target becomes irrelevant in the 2-AFC task.

Experiment 2

Method

Participants

Fifteen students were recruited from Paris universities to take part in this experiment. None of them participated in the previous experiment.

Stimuli, Procedure, and Design

The same procedure and the same type of masking, and number stimuli were used in this experiment, except with the following three main aspects: First of all, the set of numbers from the previous experiment (i.e., 1, 4, 6, and 9) was extended to include 2, 3, 7, and 8 presented only as Arabic digit or written words. The former set of numbers could be presented as primes and as target, and then constituted the practiced set, while the latter set constituted the unpracticed set presented only in the prime position and thus never in the target position. Consequently, the priming experiment consisted in four blocks of 288 trials (instead of 216 trials in Experiment 1). Each block included 96 “unpracticed” trials (i.e., trials with an unpracticed prime) and 192 “practiced” trials. The whole protocol for the main experiment now lasted about 50 min. Secondly, the response deadline was extended from 1,500 to 2,000 ms, as it might have been too pressuring and decrease performance considerably in the previous experiment. Thirdly, the procedure for the visibility measure following the priming experiment was modified to become a 2-AFC task. The trial structure was exactly the same as in the priming experiment, except that the target (or the third mask in case of cross-modal trials) was immediately followed by the simultaneous presentation of a pair of choices, one on the left side and the other on the right side of the screen. One alternative corresponded to the prime whereas the other alternative was a different and randomly chosen number between 1 and 9 (excluding 5). Both alternatives appeared in the same format (i.e., both as Arabic digits or as written words). Participants were instructed to choose which of the two alternatives corresponded to the prime within the preceding sequence of events. They responded by pressing the left button if the correct alternative was on the left side and with the right button if it was on the right side. They were told that only response accuracy, not response speed, was important. The two alternatives remained on the screen until a response was made. As in the previous experiment, participants received training sessions with 200 ms primes and then at normal speed, and the same number of experimental trials ($N = 64$ for each masking type).

Results and Discussion

Priming

The rate of incorrect responses was now 6.12% (vs. 12.75% in Experiment 1), confirming that extending the response

deadline improved performance. We first ran a global ANOVA, similarly to the previous experiment, with the factors masking type, format change, and relation (collapsed across practiced and unpracticed primes). We observed main effects of relation, $F(2, 28) = 151.17, p < .0001$, and format change, $F(2, 28) = 6.39, p < .01$. The effect of relation interacted both with masking type, $F(2, 28) = 19.36, p < .0001$, and with format change, $F(4, 56) = 3.81, p < .01$. Further analyses focused on the relation factor and were performed separately for trials with practiced and unpracticed primes.

We started by focusing on priming for practiced primes (see Figure 4) and separated unmasked and masked trials. For unmasked trials (collapsed across format change), we observed a significant effect of priming for both repetition priming, $F(1, 14) = 35.25, p < .0001$, and congruity priming, $F(1, 14) = 32.74, p < .0001$. Further restrictions to each format change revealed within-notation repetition, $F(1, 14) = 13.38, p < .005$, and congruity priming, $F(1, 14) = 19.27, p < .001$, cross-notation repetition, $F(1, 14) = 34.49, p < .0001$, and congruity priming, $F(1, 14) = 21.60, p < .0005$, and cross-modal repetition, $F(1, 14) = 16.85, p < .005$, and congruity priming, $F(1, 14) = 15.12, p < .005$. We also observed that repetition priming was significantly larger for cross-modal trials compared to within-notation trials, $F(1, 14) = 5.10, p < .05$. For masked trials there were significant effects for both repetition priming, $F(1, 14) = 7.028, p < .05$, and congruity priming, $F(1, 14) = 26.15, p < .0005$. When we further restricted our comparisons to each format change, we found for within-notation trials an effect of congruity priming, $F(1, 14) = 20.47, p < .0005$, but surprisingly no significant effect for repetition priming. This was also true for cross-notation trials for which we only observed congruity priming, $F(1, 14) = 10.30, p < .01$. By contrast, cross-modal trials led to both congruity priming, $F(1, 14) = 12.49, p < .005$, and repetition priming, $F(1, 14) = 8.10, p < .05$. Furthermore, we observed that the repetition priming advantage for cross-modal trials compared to both within-notation and cross-notation trials felt short of significance in both cases ($ps < .10$).

We then turned to congruity priming for unpracticed primes (see Figure 5). For those trials, we observed a global congruity priming effect, $F(1, 14) = 23.95, p < .0005$, which interacted with masking type, $F(1, 14) = 7.67, p < .05$. This interaction resulted from the fact that priming was significant for unmasked trials, $F(1, 14) = 24.27, p < .0005$, but not for masked trials ($p = .14$). Further restrictions revealed priming for unmasked within-notation, $F(1, 14) = 8.76, p < .05$, and cross-notation trials, $F(1, 14) = 11.16, p < .005$, and a marginally significant effect for cross-modal trials, $F(1, 14) = 4.23, p = .06$. For masked trials, no effect of congruity priming reached significance when restricting our comparisons to any of the format change.

Prime Visibility

Participants' performances on the visibility measure were 74.0% for unmasked trials and 58.7% for masked trials.

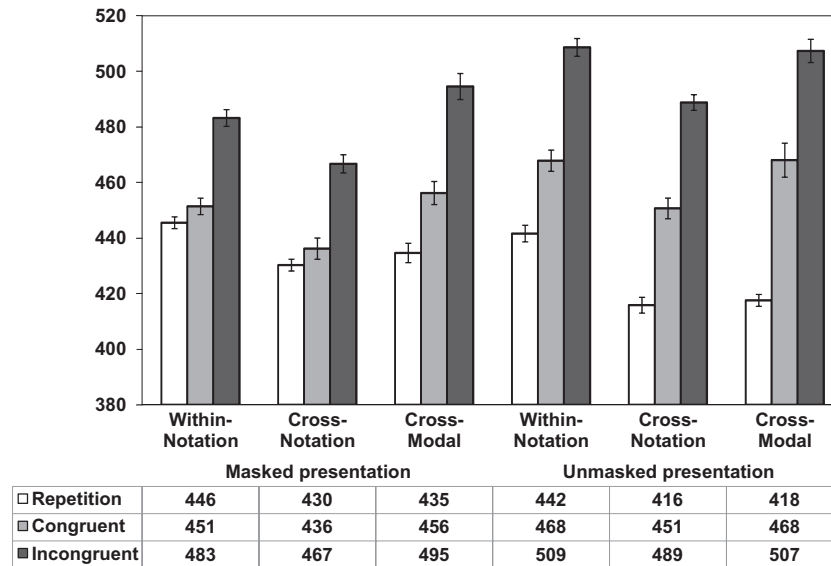


Figure 4. Average RTs for practiced trials (i.e., trials with a practiced prime) in Experiment 2.

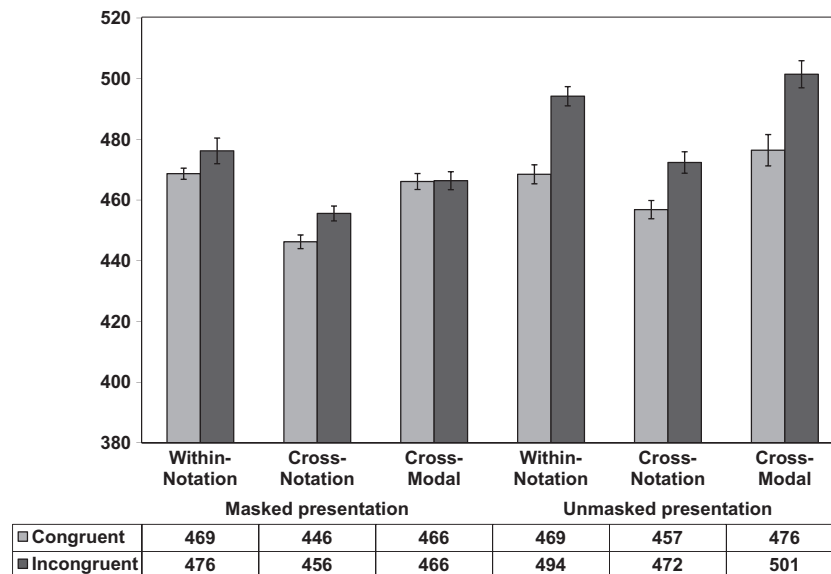


Figure 5. Average RTs for unpracticed trials (i.e., trials with a novel prime) in Experiment 2.

601 Values of d' were computed by treating primes on the right
 602 side as signal and primes on the left side as noise. For masked
 603 trials, the mean d' values across format change were 0.27,
 604 0.51, and 0.69 for within-notation, cross-notation, and
 605 cross-modal trials, respectively, and for unmasked trials,
 606 1.30, 1.16, and 2.30, respectively. The mean d' values
 607 were significantly different from zero both for unmasked trials,
 608 $d' = 1.59$; $t(14) = 7.15$, $p < .0001$, and for masked trials,
 609 $d' = 0.49$; $t(14) = 5.56$, $p < .0001$. In addition, there was
 610 a main effect of masking type, $F(1, 14) = 29.75$, $p < .0001$,
 611 a main effect of format change, $F(1, 14) = 11.11$,
 612 $p < .0005$, and an interaction between these two factors,
 613 $F(1, 14) = 3.66$, $p < .05$. Planned comparisons across for-

mat changes revealed that, for masked trials, d' values were
 higher for cross-modal compared to within-notation trials,
 $F(1, 14) = 4.62$, $p < .05$. For unmasked trials, d' values for
 cross-modal trials were higher compared to both within-
 notation, $F(1, 14) = 12.49$, $p < .005$, and cross-notation
 trials, $F(1, 14) = 11.13$, $p < .005$.

Because here, as in the previous experiment, d' values
 for masked trials were still significantly different from zero,
 we relied on the regression method to investigate the amount
 of priming when performance on the prime visibility
 measure is extrapolated to zero. Crucially, as can be seen
 in Figure 6, the intercept of the regression was significant
 (32 ms; $t(14) = 3.992$, $p < .005$) when collapsing across

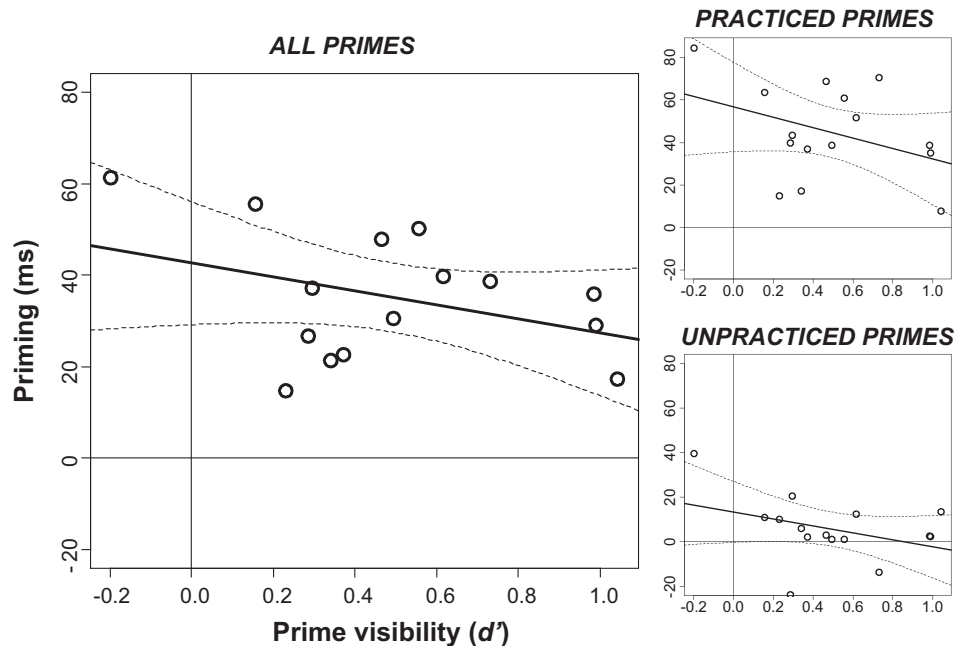


Figure 6. Regressions of masked priming on prime visibility in Experiment 2, for all trials (large left panel) or as a function of practice with the prime stimuli (two small right panels).

627 trials with practiced and unpracticed primes. However, when
 628 separating the two types of trials, this was clearly true for
 629 practiced trials (57 ms; $t(14) = 5.813$, $p < .0001$) while it
 630 only approached significance for unpracticed trials (13 ms;
 631 $t(14) = 2.12$, $p = .054$).

632 In sum, this second experiment replicated the masked congru-
 633 ity priming effect for practiced items found in Experiment
 634 1. Yet, masked repetition priming vanished almost entirely
 635 in this second experiment. Interestingly the global masked prim-
 636 ing effect for practiced items (i.e., comparing incongruent vs.
 637 repetition trials) had a similar magnitude in Experiments 1 and
 638 2 (49 and 45 ms, respectively). Therefore, one possibility is
 639 that congruity and repetition effect in masked priming are
 640 modulated depending on individual task strategies (for
 641 instance, by now relying less on perceptual identification than
 642 categorical classification of motor or semantic attributes).
 643 This is consistent with indications from the masked priming
 644 literature that repetition priming vanishes in the presence of
 645 strong congruity effects (Dell'Acqua & Grainger, 1999;
 646 Fabre, Lemaire, & Grainger, 2007), although this issue
 647 clearly needs to be further addressed and specified.

648 By contrast to practiced items, masked congruity prim-
 649 ing for unpracticed items was weak and nonsignificant. This
 650 result is at odds with the study by Naccache and Dehaene
 651 (2001a) showing in two experiments that although congruity
 652 priming for novel primes is smaller than for practiced
 653 primes, it was still highly reliable. Yet, one main difference
 654 in the present study was that participants received both sub-
 655 liminal and supraliminal trials. Thus, although the novel
 656 primes were not practiced in the sense that they received a
 657 response, here they were nevertheless seen during the course
 658 of the experiment. One possibility is that participants inhib-
 659 ited these novel primes when perceiving them consciously,

660 mainly because they are distracting and task-irrelevant.
 661 For this reason, we decided to exclude unmasked trials in
 662 the next experiment.

Experiment 3

Method

Participants

663 Fifteen students were recruited from Paris universities to
 664 take part in this experiment. None of them participated in
 665 any of the previous experiments.

Stimuli, Procedure, and Design

666 The stimuli, procedure, and design were exactly the same as
 667 for Experiment 2 when excluding the two blocks with
 668 unmasked trials. Thus, this experiment was about half the
 669 duration of the previous one because it contained only
 670 two blocks of stimuli with masked trials.

Results and Discussion

Priming

671 A first global ANOVA (collapsing across practiced
 672 and unpracticed primes) revealed a main effect of
 673 relation, $F(2, 28) = 37.15$, $p < .0001$, and format change,
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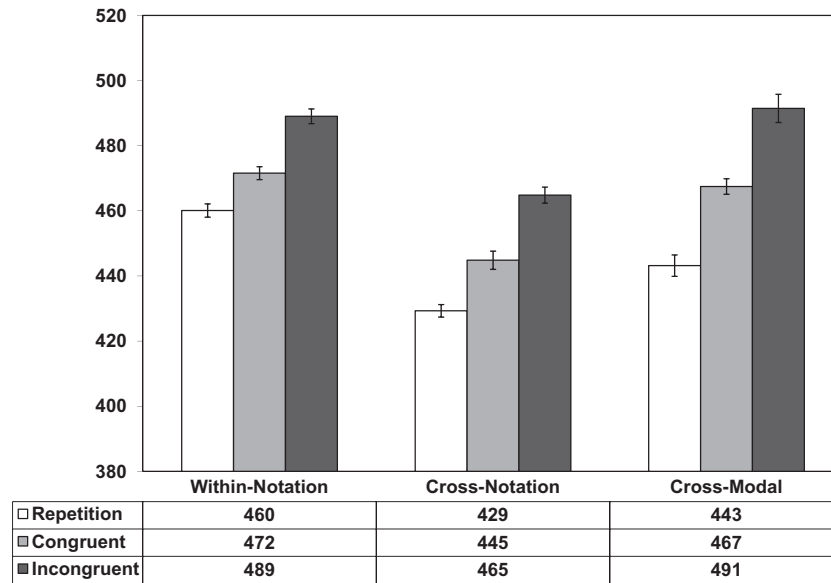


Figure 7. Average RTs for practiced trials (i.e., trials with a practiced prime) in Experiment 3.

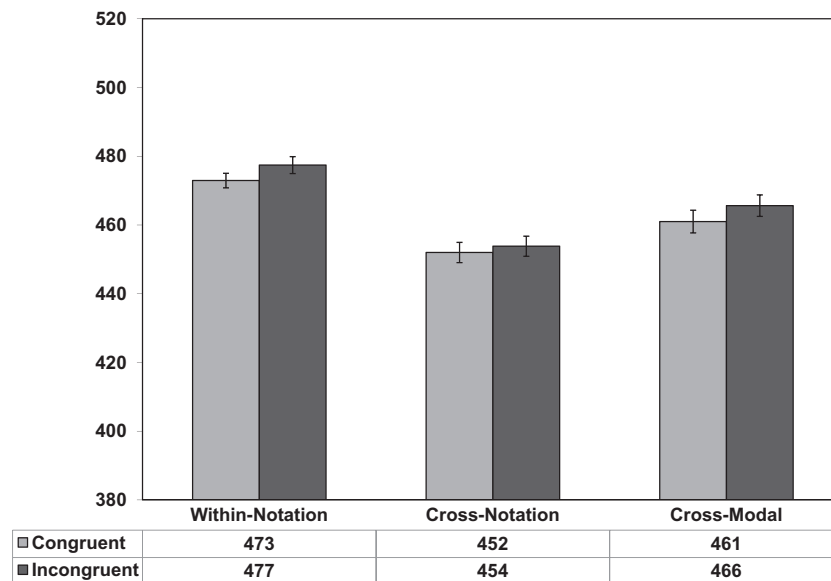


Figure 8. Average RTs for unpracticed trials (i.e., trials with a novel prime) in Experiment 3.

680 $F(2, 28) = 10.95, p < .0005$. For practiced trials (see
 681 Figure 7), we found a significant repetition priming,
 682 $F(1, 14) = 23.15, p < .0005$, that marginally interacted with
 683 format change, $F(2, 28) = 3.163, p = .06$, as well as a congruency
 684 priming effect, $F(1, 14) = 28.015, p < .0005$. Repetition
 685 priming was significant for within-notation, $F(1, 14) =$
 686 $4.65, p < .05$, cross-notation, $F(1, 14) = 14.50, p < .005$,
 687 and cross-modal trials, $F(1, 14) = 29.24, p < .0001$. It was
 688 marginally greater for cross-modal compared to within-notation
 689 trials, $F(1, 14) = 4.42, p = .05$. Congruency priming was
 690 also significant for within-notation, $F(1, 14) = 10.96,$
 691 $p < .01$, cross-notation, $F(1, 14) = 11.54, p < .005$, and
 692 cross-modal trials, $F(1, 14) = 6.83, p < .05$. For unpracticed

693 trials (Figure 8), there was no congruency priming either when
 694 collapsing all the trials across format changes or when consid-
 695 ering the different format changes separately (all $ps > .25$).

Prime Visibility

696
 697 Performance on the visibility test was 58.85% and resulted in a
 698 mean d' value of 0.56. This value was significantly different
 699 from zero, $t(14) = 4.61, p < .0005$. When dissociating it as
 700 a function of format change, we obtained d' values of 0.43 for
 701 within-notation trials, 0.47 for cross-notation trials, and 0.77
 702 for cross-modal trials. Yet, the main effect of format change

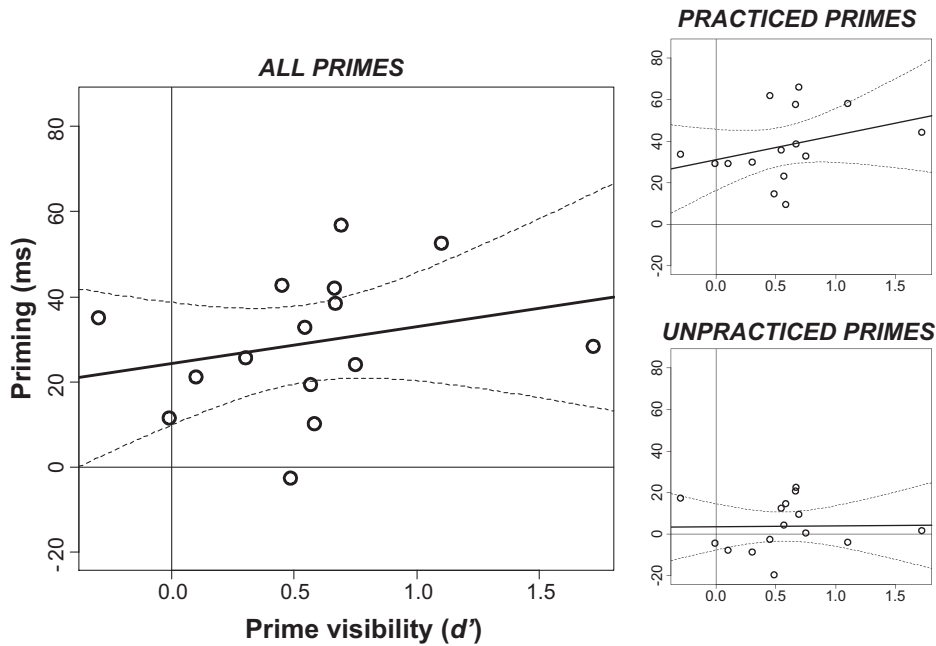


Figure 9. Regressions of masked priming on prime visibility in Experiment 3.

was not significant, nor were pairwise comparisons across these values (all $ps > .30$). As in the previous experiment, the Greenwald's regression method (Figure 9) revealed that priming extrapolated to null performance was significant as a whole (24 ms; $t(14) = 3.632$, $p < .005$) and when restricted to practiced trials (31 ms; $t(14) = 4.542$, $p < .001$) but not when restricted to unpracticed trials (3 ms; $t < 1$).

In sum, the exclusion of unmasked trials in this third experiment did not improve the observation of congruity effects for novel primes. These were still absent, contrary to congruity effect for practiced primes. Note also that, as in Experiment 1 and contrary to Experiment 2, the contribution of repetition trials to masked priming was now highly significant. Interestingly, and in accordance with the explanation proposed in the discussion of Experiment 2, the occurrence of repetition priming was accompanied with a weaker congruity priming effect (for practiced primes) in Experiment 3 compared to Experiment 2. The repeated absence of congruity effects for novel primes, even if we could sometimes observe small nonsignificant trends, is problematic for semantic interpretations of subliminal number priming. Yet, the recurrent observation of a masked cross-modal effect suggests that subliminal processing extends beyond the perceptual level. Before discussing further the implication of these findings, we wanted to ensure that the cross-modal effect did not result from residual prime awareness. Indeed, masked cross-modal priming was consistently higher than within-notation and cross-notation priming in the previous experiment. However, d' values were also consistently higher for cross-modal trials, although this difference was significant only in Experiment 2. Therefore, we decided to run a fourth experiment with a different backward masking procedure for cross-modal trials. Piloting

work revealed that replacing the series of hash marks (e.g., #####) serving as a final backward mask (see Figure 1) with another random letter string (e.g., FTfVkJ) considerably reduced prime visibility.

Experiment 4

Method

Participants

Twenty-three students were recruited from Paris universities to take part in this experiment. None of them participated in any of the previous experiments. One participant was excluded because of extreme error rate and RTs.

Stimuli, Procedure, and Design

The stimuli, procedure, and design were exactly the same as for Experiment 3 with one exception: the third mask during cross-modal trials, rather than being a series of hash marks (e.g., #####), was now another combination of 6 upper- and lower-case consonant letters (e.g., FTfVkJ) constructed along the same principle as for the other masks.

Results and Discussion

Priming

The global ANOVA revealed a main effect of relation, $F(2, 42) = 37.51$, $p < .0001$. For practiced primes

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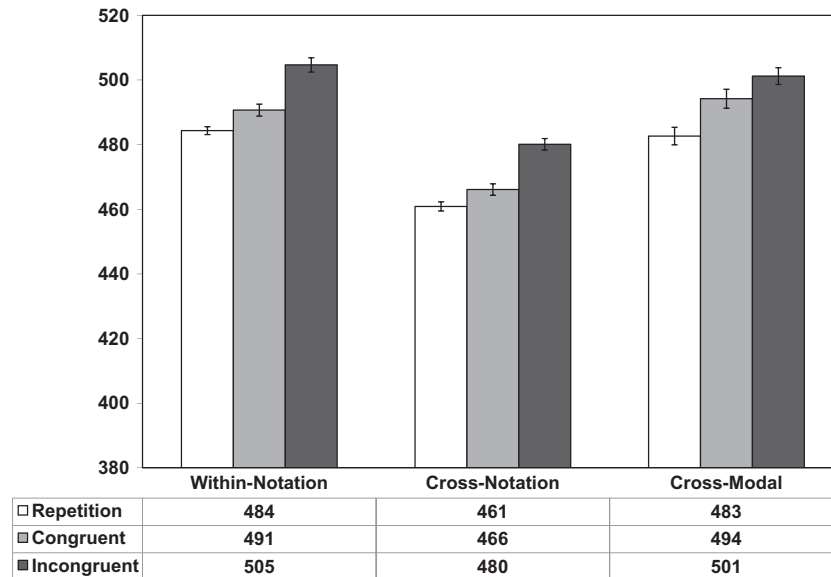


Figure 10. Average RTs for practiced trials (i.e., trials with a practiced prime) in Experiment 4.

(Figure 10), we observed both a significant effect of repetition priming, $F(1, 21) = 7.22$, $p < .05$, and a significant effect of congruity priming, $F(1, 21) = 8.69$, $p < .01$. Although none of the two forms of priming interacted with format change, we performed restricted comparisons and found that the six priming contrasts were significant or marginally significant (all $ps < .10$), except for the cross-modal congruity effect ($F = 1.02$). For unpracticed primes (Figure 11), priming felt short of significance only when considered as a whole regardless of format change, $F(1, 21) = 3.05$, $p < .10$.

Prime Visibility

Performance on the visibility test was 55.5% resulting in a mean d' value of 0.25 that was significantly different from zero ($t(21) = 2.88$, $p < .01$). Considered as a function of format change, these values were 0.38 for within-notation trials, 0.42 for cross-notation trials, and 0.09 for cross-modal trials. Greenwald's regression method (Figure 12) showed that priming extrapolated to null performance reached significance as a whole (13 ms; $t(21) = 4.56$, $p < .0005$) and also for practiced (17 ms; $t(21) = 3.68$, $p < .005$) but not for unpracticed trials although there was a trend of 7 ms ($p = .11$).

General Discussion

The present study was aimed at examining two markers of the depth of subliminal number priming. One was the possibility of a subliminal transfer from the visual to the auditory modality, suggesting that subliminal number priming extends

beyond the perceptual level. The other was the robustness of generalization to novel numbers, suggesting further that subliminal number priming necessarily involves semantic attributes. We performed four experiments in order to test these two hypotheses. In regard to transfer across modalities, we consistently found masked cross-modal priming across the four experiments. In addition, the improved masking method used in Experiment 4 showed that it genuinely reflected a subliminal effect that cannot be explained as resulting from a residual form of stimulus awareness. By contrast, the results we found for novel primes were much weaker and lead to an ambiguous interpretation. Indeed, on the one side, none of the three experiments containing novel primes (Experiments 2–4) showed a robust and significant effect of generalization. On the other side, we consistently observed small trends in the expected direction. As such, it remains difficult to interpret these effects, and more fundamentally their participation in subliminal priming.

In order to deal with this ambiguous outcome, we further performed two global analyses, one collapsing all the masked trials with practiced primes from the four experiments reported in this study, and the other one collapsing all the masked trials with unpracticed primes (thus excluding Experiment 1). For practiced trials, we observed a main effect of repetition priming of 13 ms, $F(1, 62) = 49.31$, $p < .0001$, and a main effect of congruity priming of 22 ms, $F(1, 62) = 71.10$, $p < .0001$. When considering these two effects as a function of the three conditions of format change, we observed highly significant effects in the six resulting contrasts (all $ps < .001$). For unpracticed primes, we observed a much smaller main effect of congruity of 6 ms which, nevertheless, was statistically significant, $F(1, 51) = 6.91$, $p < .02$. When considering this main effect as a function of notation change, it reached significance for within-notation trials, $F(1, 51) = 4.41$, $p < .05$, but not for cross-notation trials, $F(1, 51) = 0.53$, $p = .46$, or

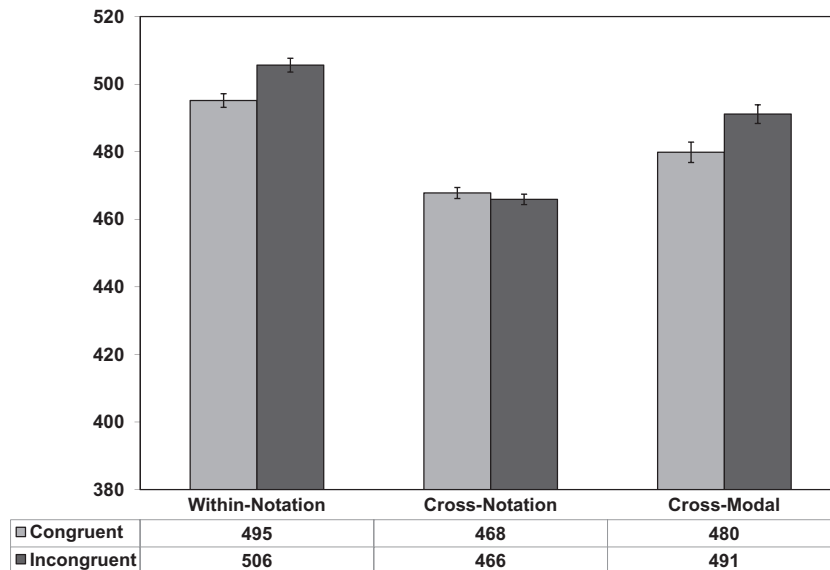


Figure 11. Average RTs for unpracticed trials (i.e., trials with a novel prime) in Experiment 4.

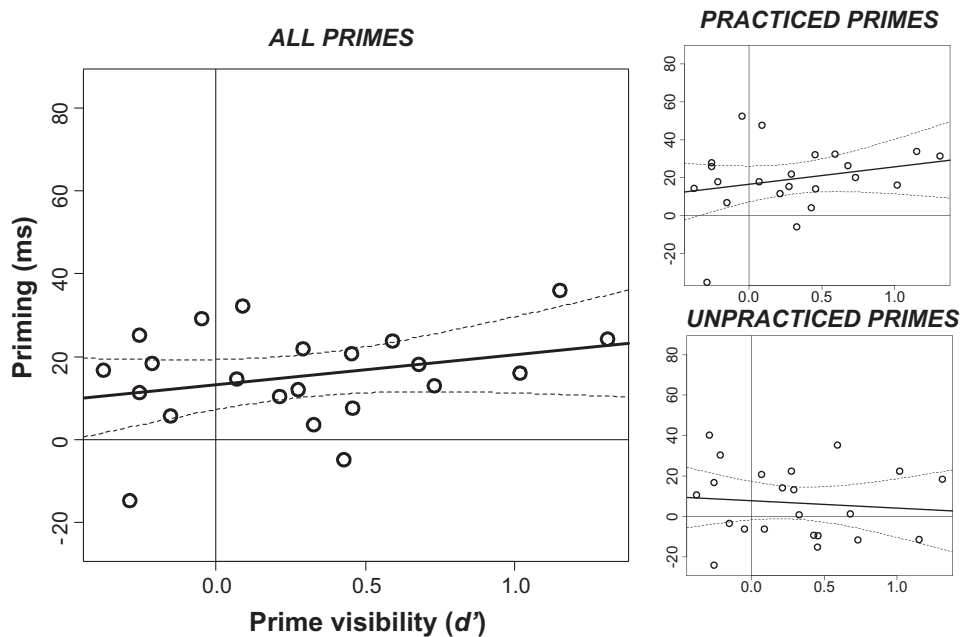


Figure 12. Regressions of masked priming on prime visibility in Experiment 4.

823 cross-modal trials, $F(1, 51) = 2.10$ $p = .15$. Importantly, as
 824 depicted in Figure 13, regressions of the priming effect with
 825 d' measures of prime awareness revealed that the regression
 826 intercept was significantly different from zero not only for
 827 practiced primes (31 ms; $t(62) = 8.10$, $p < .0001$), but also
 828 for unpracticed primes (8 ms; $t(51) = 2.60$, $p < .02$), indi-
 829 cating that the effect could not be attributed to prime visibil-
 830 ity. We therefore conclude, together with Naccache and
 831 Dehaene (2001a), that subliminal number priming can

extend to unpracticed stimuli, although this effect turns
 out to be relatively small. These results provide evidence
 that semantic information contributes to subliminal number
 priming (Dehaene et al., 1998; Naccache & Dehaene,
 2001a), although they also show that priming can, in a large
 part, be affected by the deployment of stimulus-response
 associations (Abrams & Greenwald, 2000; Damian, 2001).

Why are semantic effects so weak and unstable in
 masked number priming? From a theoretical perspective,

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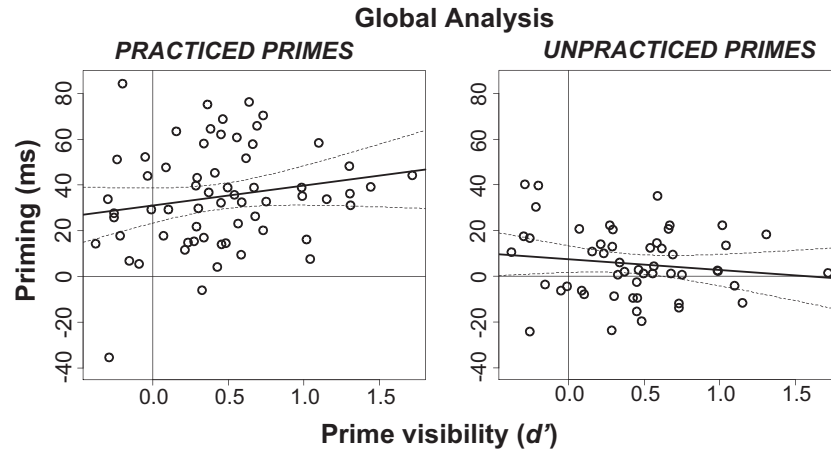


Figure 13. Regressions of masked priming on prime visibility across all experiments, as a function of practice with the prime stimuli.

841 in fact, masked priming is predicted to be weaker than for
 842 supraliminal primes, since masked primes are assumed to
 843 convey neural information only in a bottom-up fashion, pre-
 844 venting any contribution from reinforcing feedback loops
 845 (Lamme, 2003). In addition, brain imaging and neurophysi-
 846 ological data have shown that masking also prevents the
 847 efficient propagation of bottom-up stimulus activation in
 848 successive perceptual areas, leaving only a short pulse of
 849 activity whose amplitude decreases at each synaptic step
 850 (Dehaene et al., 2001; Del Cul, Baillet, & Dehaene, 2007;
 851 Kovacs, Vogels, & Orban, 1995; Lamme, 2003; Thompson
 852 & Schall, 1999). As such, we have recently proposed that
 853 although behavioral priming effects can be detected when
 854 they involve neural processing at a certain distance from
 855 sensory systems, they are expected to decrease with synaptic
 856 distance and become very small and sometimes undetectable
 857 in distant semantic areas (Kouider & Dehaene, 2007). Addi-
 858 tionally, as predicted by the neuronal workspace theory
 859 (Dehaene & Naccache, 2001), the occurrence of subliminal
 860 semantic priming should also be modulated by the reinforce-
 861 ment of semantically mediated pathways during task execu-
 862 tion. That is, the more the task involves the extraction of
 863 semantic information in a routine manner, the more this
 864 semantic stream of processing will be automatized and oper-
 865 ate in an unconscious fashion (see Nakamura, Dehaene,
 866 Jobert, Le Bihan, & Kouider, 2007, for brain imaging evi-
 867 dence of how task context influences the neural circuitry
 868 at the origin of subliminal priming). As such, it is possible
 869 that the involvement of subliminal semantic processing in
 870 the numerical comparison task depends on intrinsic
 871 experimental features and participants' response strategies,
 872 stressing either numerical magnitude estimation or rather
 873 sensorimotor mappings. Along the same lines, the fragility
 874 of semantic effects when using behavioral priming methods
 875 might also result from a problem of sensitivity. Indeed, the
 876 single data point obtained in RTs only reflects a partial
 877 read-out of the processing stream triggered by the stimulus.
 878 As such it is possible that semantic processing occurs

879 without being detected, because the decisional components
 880 leading to priming are mainly driven by other (i.e., sensorimotor)
 881 stages of processing. Consistent with this idea,
 882 it appears that neuroimaging methods, that can in principle
 883 cover the locus of any processing stage, have been more
 884 consistent than behavioral methods in observing subliminal
 885 semantic influences (see Kouider & Dehaene, 2007).

886 Finally, another possible interpretation for the weakness
 887 of subliminal number priming for novel stimuli can be found
 888 in the theory of "action-triggers" (Elsner et al., 2008;
 889 Kiesel, Kunde, & Hoffmann, 2007; Kunde, Kiesel, &
 890 Hoffmann, 2003). Kunde, Kiesel, and colleagues suggested
 891 that apparently inconsistent data patterns in subliminal priming
 892 might be explained by considering that subjects prepare
 893 action triggers in order to quickly associate each possible
 894 experimental stimulus with its appropriate response in minimal
 895 time. The setting of action triggers happens during the
 896 instructions or practice phase and depends on the stimulus
 897 set size, as it is efficient only for narrow categories (e.g.,
 898 Arabic numbers from 1 to 9). According to this account,
 899 even novel primes (e.g., 2 and 3) may prime the appropriate
 900 response not because the meaning of these primes has been
 901 extracted, but rather because the adequate response to these
 902 stimuli was consciously prepared in advance. Consequently,
 903 according to this interpretation, the absence or presence of
 904 priming for novel stimuli will depend on participants' exact
 905 interpretation of the instructions and on their expectations
 906 that these novel stimuli will be presented during the exper-
 907 iment. In our study, it is possible that participants did not
 908 prepare action triggers efficiently because they were faced
 909 with three formats (Arabic, written words, and auditory
 910 word), leading to a rather large set of possible triggers.
 911 We suspect that these factors do contribute to the weaker
 912 and somewhat variable priming effects observed with novel
 913 primes, compared to the strong effects consistently observed
 914 with practiced primes, although recent research clearly indi-
 915 cates that it cannot be the whole story as priming *can* be
 916 observed with unpracticed primes even when very large

stimulus categories are used (e.g., Klauer et al., 2007; Van den Bussche & Reynvoet, 2007). An alternative explanation is that subliminal category congruity effects are obtained with novel primes as a function semantic overlap, that is as long as the prime and target share many semantic features (e.g., Quinn & Kinoshita, 2008). Clearly, more research is needed to characterize more precisely how priming does generalize to novel primes.

The most convincing finding for unconscious processing beyond the perceptual level is the subliminal cross-modal priming effect we observed. Indeed, our study revealed that subliminal priming involves amodal representations during number processing, as we found both repetition and congruity priming for masked visual-to-auditory trials. This finding is consistent with the triple-code theory (Dehaene, 1992) which postulates that numerical cognition involves a lower step of modality-specific analysis of number stimuli, and then a higher processing stage where these representations reach an abstract “number sense”, that is an amodal numerical estimation module computing magnitude estimation and contributing to mathematic performances. Neuropsychological investigations have found that these numerical representations imply cerebral activity in the intraparietal sulcus (Dehaene & Cohen, 1995). This region is identically activated when processing target numbers in the visual and in the auditory modality (e.g., Eger, Sterzer, Russ, Giraud, & Kleinschmidt, 2003). Importantly, using fMRI, Naccache and Dehaene (2001b) have observed masked numerical repetition suppression in this area across the Arabic and written notations, although this study remained in the visual domain. The extension of repetition priming to cross-modal conditions in the present study is highly suggestive of the involvement of this intraparietal magnitude representation, and thus of some form of semantic processing, in subliminal number priming.

It is important to note, finally, that the weakness of cross-modal congruity effects for novel stimuli does not allow for an unequivocal semantic interpretation of subliminal cross-modal priming. It could therefore still be argued that subliminal cross-modal priming for repeated primes resulted from the construction of amodal representations “on the fly”, as a function of the specific experimental context in use. Under this interpretation, participants would, through practice during the experiment, build episodic representations that are shared between visual and auditory numbers, and use these representations both for categorizing the incoming stimuli (primes as well as targets) and for activating the relevant motor codes. This episodic interpretation of priming might explain why cross-modal subliminal priming itself is not consistently observed in the literature.²

In sum, our experiments revealed that number priming transfers across modalities, suggesting that it involves higher-level representations beyond the perceptual stage

and possibly semantic attributes related to magnitude estimation. Nevertheless, the weakness of priming effects in generalizing to novel stimuli suggests that semantic-level activation is heavily reduced by masking and that priming effects are often, though not always, dominated by lower-level perceptual effects.

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² Indeed, in the first study using this method, Kouider and Dupoux (2001) used masked words and found that only within-modal repetition priming was reliable under subliminal conditions, cross-modal priming showing a perfect correlation with prime awareness. However, more recent studies by Grainger, Diependaele, Spinelli, Ferrand, and Farioli (2003), and by Nakamura et al. (2006) revealed the presence of cross-modal priming under subliminal conditions. One main difference that can explain this discrepancy is that the latter studies used stimuli that were repeated and appeared both as primes and as targets during the experiment, while Kouider and Dupoux used a large set of words which appeared only once during the experiment.

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Sid Kouider 1138

Ecole Normale Supérieure 1139

29 rue d'Ulm 1140

75005 Paris 1141

France 1142

Tel. +33 1 44 32 26 22 1143

Fax +33 1 44 32 26 30 1144

E-mail sid.kouider@ens.fr 1145

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