Implicit change identification:

Cédric Laloyaux*, Arnaud Destrebecqz and Axel Cleeremans

Cognitive Science research Unit
Université Libre de Bruxelles CP 191
Av. F.-D. Roosevelt, 50
1050 Bruxelles
BELGIUM
Tél: +322 650 42 31
Fax: +322 650 22 09
Email: claloyau@ulb.ac.be

* = corresponding author

Keywords: change detection, change blindness, implicit change detection, awareness, attention
Abstract

Using a simple change detection task involving vertical and horizontal stimuli, Thornton and Fernandez-Duque (2000) showed that the implicit detection of a change in the orientation of an item influences performance in a subsequent orientation judgment task. However, Mitroff, Simons and Franconeri (2002) were not able to replicate this finding after correcting for confounds, and thus attributed Thornton et al.’s results to methodological artifacts. Because Mitroff et al.’s failure to replicate might in turn stem from several methodological differences between their study and those of Thornton and Fernandez-Duque (2000) and of Fernandez-Duque and Thornton (2003), we set out to conduct a further replication in which we corrected all known methodological biases identified so far. Our results suggest that implicit change detection indeed occurs: People’s conscious decisions about the orientation of an item appear to be influenced by previous undetected changes in the orientation of other items in the display. We discuss the implications of this finding in light of current theories of visual awareness.
**Introduction**

Change blindness and other related phenomena (e.g., inattentional blindness; the attentional blink) demonstrate that people’s experience of the visual world is far more limited than one would think: Large changes presented in the center of the visual field fail to be detected, incongruous moving objects escape detection, etc. (Rensink, 2002; Rensink *et al.*, 1997; Simons, 2000; Simons & Ambinder, 2005; Simons & Chabris, 1999). However, there also is a substantial (but controversial) literature suggesting that people’s behavior can be influenced by aspects of the environment that they show little or no evidence of having processed consciously (e.g. Greenwald *et al.*, 2003) or in the near absence of attention (e.g. Li *et al.*, 2002). The phenomenon of contextual cueing, for instance, in which people’s ability to locate a visual target presented amongst distractors is improved when the array of distractors is predictive of the target’s location, occurs even in cases where subjects fail to detect the contingency between the visual context set up by the distractors and the target’s location (Chun & Jiang, 1998). Likewise, sequence learning studies demonstrate that people can use the temporal context set up by previous elements to speed up their responses in a choice reaction time task even in cases where they show little or no ability to report on the sequential contingencies (Cleeremans & McClelland, 1991; Destrebecqz & Cleeremans, 2001). These phenomena suggest that behavior can be influenced, essentially through priming mechanisms, by information that subjects do not hold consciously.

Such findings raise the following issue in the context of change blindness experiments: What is the fate of changes to objects that people have failed to perceive? Are they
represented beyond the retina? Can they influence subsequent processing? If this were the case, it would constitute a clear instance of implicit perception: People claim not to have seen a stimulus, yet this stimulus demonstrably exerts subsequent causal effects and must hence be represented somehow in the cognitive system.

The possibility that stimuli of which one remains unaware of nevertheless influence subsequent processing has recently been the focus of renewed debate in the domain of visual processing, through the paradigm of change detection (Fernandez-Duque & Thornton, 2000, 2003; Mitroff et al., 2002; Thornton & Fernandez-Duque, 2000). In this paradigm, observers are exposed to simple changes such as changes in the orientation of horizontal and vertical rectangles (see Fernandez-Duque & Thornton, 2000, 2003; Mitroff et al., 2002; Thornton & Fernandez-Duque, 2000). Such simple changes stand in contrast with other situations that typically involve complex scenes, such as the well-known flicker paradigm, first introduced by Rensink et al. (1997) and subsequently widely used to explore the mechanisms underlying change detection (i.e. Hollingworth & Henderson, 2002; O'Regan et al., 1999; Rensink et al., 1997; Simons et al., 2002). Simple stimuli have also been used by Rensink in a paradigm that combines visual search with a repeated flicker paradigm (i.e. visual search for a change, Rensink, 2000b).

Thus, in this paradigm, subjects are exposed to a change detection task involving vertical and horizontal bars. Thornton and Fernandez-Duque (2000) showed that exposure to a change in the orientation of an item influenced performance in a subsequent orientation judgment task performed on the same material, even when subjects claimed not to have consciously perceived the change. However, Mitroff et al. (2002) were not able to
replicate this result after having corrected methodological biases, and thus took Thornton et al.’s findings to be artefactual. These and other studies reviewed below now form the core of a controversial literature that Simons and Rensink (2005) have recently characterized as addressing one of the most central issues raised by change blindness research.

In this context, the main goal of our study was to conduct a conceptual replication of the original studies in such a manner that all known methodological issues identified by previous authors were addressed. We begin by describing the original findings and continue by surveying subsequent attempts to replicate these results. We focus on studies that have specifically explored implicit change identification (Fernandez-Duque & Thornton, 2003, Experiments 2-4; Mitroff et al., 2002, Experiments 4A and 4B; Thornton & Fernandez-Duque, 2000). Table 2 lists the experiments we have considered, their results and important aspects of their methodology. It can serve as a roadmap to the following sections.

Thornton & Fernandez-Duque (2000)’s study

Thornton and Fernandez-Duque (2000) explored whether undetected changes can nevertheless be processed and influence subsequent processing. Observers were shown 2 successive displays containing an array of 8 black rectangles. On the first display, half of these items were oriented horizontally, while the remaining half was oriented vertically (see Figure 1 for an example of the stimuli used in the present studies. These stimuli are very similar to those used in the previous studies). This display was shown for 250 ms and was immediately followed by a blank screen, again shown for 250 ms. Next, a
second stimulus array, in which one of the rectangles might now appear rotated by 90°
relative to its orientation in the first display, was shown for 250ms. Finally, a third
display—the probe—appeared for a mere 20ms. This probe display was exactly the same
as the second one, but one of the rectangles now appeared in white. Subjects had to
perform two tasks: First, they had to make an orientation judgment on the highlighted
(white) rectangle, pressing one key when this item was oriented horizontally and another
when it was oriented vertically (“speeded orientation task”). Next, they had to indicate
whether they had perceived the change in orientation that might have taken place between
the first and second displays. This second task (“change detection task”) involved a
go/no-go response: Subjects pressed on the space bar if they had perceived a change, and
did nothing in case they had not.

This design made it possible to compare performance in four conditions defined by
crossing two factors: Congruency and validity (see Figure 2). Validity refers to the
location at which both the change and the probe may be presented. Trials in which a
change occurred between the first and second display and in which the change occurred
at the same location as the one at which the probe stimulus appeared on the third display
were labeled “valid” trials, based on the notion that if people are sensitive to the change,
their orientation response to the white bar would be facilitated (Posner, 1980). Trials in
which the change occurred at a different location than the one at which the probe item
appeared were labeled “invalid” trials. Importantly, in Thornton and Fernandez-Duque
(2000)’s experiment, the highlighted rectangle always appeared either at the location of
the change, or at the diametrically opposite location.
Congruency between the orientation of the probe item and the final orientation of the changed rectangle defined the second factor of interest in this paradigm. “Congruent” trials are trials in which the probe item is oriented in the same direction as that of the changed rectangle presented on the second display; “incongruent” trials are those in which this condition does not hold. Thus, if the orientation of the changed rectangle was vertical on the second display, the trial was congruent when the highlighted rectangle presented on the probe display was vertical, but incongruent when the highlighted rectangle was oriented horizontally.

Using this paradigm, Thornton and Fernandez-Duque (2000) found a congruency effect: Observers responded more slowly and made more errors for incongruent than for congruent trials when they were aware of the change. The authors also reported a validity effect on the error rate when the change had been consciously reported: the error rate was lower for valid than for invalid trials. More interestingly, they also found a congruency effect on the error rate when observers had reported being unaware of the change. This finding clearly suggests that the nature of a change can be registered without reaching awareness and that this information (e.g. “verticality” when a horizontal rectangle is replaced by a vertical one) can be activated sufficiently strongly to have an effect on a subsequent response. It must be noted that the congruency effect implies that it is indeed the nature of the change that is activated, and not merely its localization, since the effect holds even for invalid trials, that is, for trials in which the change actually occurred at a different location than the one at which people have to respond.
However, this interpretation of Thornton & Fernandez-Duque’s data, and thus the very existence of implicit change detection, was challenged by Mitroff, Simons and Franconeri (2002). In the next section, we describe their alternative explanation of Thornton and Fernandez-Duque’s results (2000), according to which “implicit” change detection can simply be attributed to explicit strategies.

Mitroff, Simons and Franconeri (2002)

A simple, alternative explanation of the implicit change detection effect reported by Thornton & Fernandez-Duque (2000) is that subjects are using conscious strategies to anticipate the location of the probe based on regularities contained in the stimulus material. Building on this idea, Mittroff et al. (2002) replicated Thornton and Fernandez-Duque’s experiments and also other studies purporting to demonstrate implicit change registration (Williams & Simons, 2000) or localization (Fernandez-Duque & Thornton, 2000; Smilek et al., 2000), and claimed that all the behavioral evidence for implicit change detection or registration could be explained in terms of the deployment of conscious strategies rather than on unconscious sensitivity to change.

Thus, Mitroff et al. (2002) pointed out that in the case of an orientation change in Thornton and Fernandez-Duque’s experiments (2000), the probe (i.e., the item that changed color) was either the changed item (i.e., the item that changed orientation in 50% of the trials) or the item that was located at the diametrically opposite location (in the remaining 50% of the trials). Mitroff et al. (2002) therefore suggested that participants could quickly learn this spatial relationship between the probe and the changed item and hence come to pay more attention to the item opposite to the probe. In congruent trials,
the probe and the changed item have the same orientation in the third display, while they have different orientations in incongruent trials. The congruency effect reported by Thornton and Fernandez-Duque (2000) would thus depend on the relationship between the probe and the changed item in the final display rather than on the relationship between the probe and the change that occurred between the first and the second displays.

To explore this hypothesis, Mitroff et al. (2002) first replicated Thornton and Fernandez-Duque’s experiment (2000), and were able to reproduce the congruency effect both when subjects were aware and unaware of the change (Mitroff et al., Experiment 4A). In a second replication, however, Mitroff et al. eliminated the spatial relationship between the item that changed orientation and the item that changed color (the probe) (Mitroff et al., 2002, Experiment 4B). With the exception of the valid trials (in which the probe appeared at the same location than the orientation change), the probe could then be any of the seven remaining elements. Significantly, under these better controlled conditions, Mitroff et al. (2002) were not able to replicate the congruency effect reported by Thornton & Fernandez-Duque (2000), neither for the RT nor for the error rates, and regardless of whether participants reported being aware or not of the having perceived the change. Also in line with their interpretation, Mitroff et al. (2002) reported that most observers were conscious of the spatial relationship between the probe and the changed item in experiment 4A. Mitroff et al. (2002) therefore concluded that the effects described by Thornton & Fernandez-Duque (2000) were entirely artifactual, and hence that there is no basis for the concept of implicit change detection. Theoretically, they further argued that the two visual representations—before and after the orientation change—could not be
compared in the absence of an explicit comparison process, which is assumed to require attention.

It must be noted, however, that Mitroff *et al.* (2002) failed to observe any congruency effect, even in the “aware” trials in which participants noticed the orientation change. In a reply to Mitroff *et al.* (2002), Fernandez-Duque & Thornton (2003) thus claimed that this failure to obtain a congruency effect suggests that the modified version of the paradigm used by Mitroff *et al.* (2002) was not sensitive enough. We examine this study in the next section.

**Fernandez-Duque & Thornton (2003)**

According to Thornton & Fernandez-Duque (2003), Mitroff *et al.* (2002) changed the paradigm so much that it makes it simply impossible to obtain any congruency effect. In other words, Fernandez-Duque and Thornton (2003) consider that observing a congruency effect in aware trials constitutes a prerequisite to obtain the same effect in unaware trials. In the absence of the former, the failure to replicate the latter effect should therefore not be construed as a demonstration that implicit change detection does not exist.

According to Fernandez-Duque and Thornton (2003), there are at least two reasons why the congruency effect was completely absent in Mitroff *et al.*’s Experiment 4B (2002). First, accuracy was only 60 % in that experiment, while it was about 80 % in previous experiments demonstrating a significant congruency effect (even for the first replication of Mitroff *et al.* (2002): 77%). Given that chance level is at 50%, a hit rate of 60% might
be too low to reveal any significant effect, simply because the numbers of correct trials upon which to measure the effect is too small.

Second, RTs were very slow in Mitroff et al.’s (2002) experiment, and congruency effects are known to depend on global response speed. It has indeed been reported that congruency effects can disappear when RTs are too slow (Dejong et al., 1994). In contrast to the initial experiments demonstrating an implicit congruency effect (Fernandez-Duque & Thornton, 2003; Thornton & Fernandez-Duque, 2000), Mitroff et al. (2002, Experiment 4B) did not use an auditory signal to speed up and constrain RTs in their experiment. The mean RT difference between Mitroff et al.’s (2002) non-replication and Thornton and Fernandez-Duque’s (2000) original findings was about 300 ms. In contrast, RTs were significantly faster in Mitroff et al.’s (2002) experiment 4A than in their experiment 4B, which probably allowed them to successfully produce a reliable congruency effect.

Fernandez-Duque and Thornton (2003) also replicated their initial experiment (Thornton & Fernandez-Duque, 2000), this time eliminating the potential bias uncovered by Mitroff et al. (2002), that is, the spatial relationship between the changed and the highlighted item. They also made sure that their subjects would respond more quickly than in Mitroff et al. (2002)’s experiment 4B. Under these conditions, they again found a congruency effect on the error rates, for both aware and unaware trials. Moreover, they also observed a congruency effect on the RTs, for both aware and unaware trials—a result they had failed to find in their original study. However, it should be noted that Fernandez-Duque and Thornton (2003) argued that finding a congruency effect for the accuracy and not for
the RTs in the unaware trials might suggest that there is a qualitative difference between these two measures. More generally, they argued that any kind of dissociation between aware and unaware trials would be a strong argument in favor of the interpretation that two different processes (one explicit and another one implicit) are involved in the task.

To sum up, Fernandez-Duque and Thornton (2003)’s new results were even stronger than their previous findings, and also ruled out the alternative explanation proposed by Mitroff et al.’s (2002). However, it is worth noting that Fernandez-Duque & Thornton (2003) increased the duration of the probe to 40 ms (instead of 20ms) in this new experiment so as to increase accuracy. This again introduces another modification with respect to the original task.

In another experiment aimed at further elucidating differences between the two series of experiments, Fernandez-Duque & Thornton (2003) reset the display duration to its initial value of 20 ms and eliminated the speeding beep. They still found congruency effects on error rates, in both aware and unaware trials. Further analyses showed that the effect was only present for so-called fast responders (RT < 1000ms), which again suggests that the slow RTs in Mitroff et al.’s (2002) might explain their failure to replicate.

Finally, Fernandez-Duque and Thornton (2003) again replicated their first experiment correcting for another potential confound. In every experiment reviewed so far, the first stimulus screen always displayed 4 horizontal (H) and 4 vertical (V) rectangles. The second display, in which one of the rectangles changed its orientation, was then always composed of 5 rectangles of one category and 3 of the other category. This necessarily implies that one of the rectangles has changed its orientation between the first and the
second display. The presence of such regularity in the stimulus material may lead participants to apply an explicit (or even implicit) strategy based on counting so as to identify the nature of the change. As a consequence, Fernandez-Duque and Thornton (2003) conducted another experiment in which this potential bias was controlled for by using as a first stimulus screen either a 4V/4H display, or a 3V(or H)/5H (or V) display. Under these conditions, Fernandez-Duque and Thornton (2003) reported a congruency effect on the RTs, in both aware and unaware trials. It must be noted, however, that in this last experiment, Fernandez-Duque and Thornton (2003) reintroduced another potential bias, i.e. the systematic spatial relationship between the changed item and the probe.

To summarize this rather long and intricate story, none of the experiments conducted by the two labs were successful in correcting all known potential confounds possibly influencing the occurrence of a congruency effect in unaware trials. As a result, the possibility for changes to be processed unconsciously remains an open question. As Thornton and Fernandez-Duque (2002) suggested, converging evidence is needed to establish implicit change detection. We therefore attempted to replicate their findings in a better controlled experimental setting.

In this controversial context, our main goal was thus simply to replicate the original findings in such a way that all previously identified methodological biases were controlled for. Indeed, while Fernandez-Duque and Thornton (2003) did replicate their own previous findings, they also failed to correct every potential confound in a single experiment. In particular, they either controlled for the number of vertical and horizontal
items (thus preventing an explicit counting strategy) or for the spatial relationship
between prime and target (thus preventing strategies based on the learning of this
relationship), but never for both. In addition, their studies all involved rather small
number of participants. In our replication, we controlled for both biases and used a larger
sample of 24 participants. We now turn to describing our conceptual replication.

Method

Participants

24 students from the Université Libre de Bruxelles participated in this experiment for
course credit. All had normal or corrected to normal vision and were naive as to the
hypotheses under investigation.

Material

Stimulus presentation and data acquisition were conducted using a G4 Macintosh
computer running Psyscope for OSX and connected to a 17” 100 Hz CRT monitor.

Stimuli

Each stimulus display was a circle of rectangles arranged in a clock face design such that
any particular item was equidistant from a central fixation point. Rectangles were colored
in black on a grey background. The size of a rectangle was of 10 x 30 pixels on a 17
inches screen at a 1024 x 768 pixels screen resolution, which subtended approximately
1.15° x 0.38° visual angle at a 50 cm viewing distance. The complete ring subtended 4.3
degrees of visual angle from the fixation point, a value that is similar to those used by

For each trial, the initial ring of rectangles was composed either of 4 horizontally oriented rectangles and 4 vertically oriented ones, or of 5 rectangles of one category and 3 rectangles of the other category. This initial display was presented for 250 ms and subsequently replaced by a blank screen, again displayed for 250 ms. A second array of rectangles was then presented for 250 ms. This second array could either be the same as the first one in the no-change, catch trials, or it could differ from the first array by a single rectangle, the orientation of which was rotated by 90°. This second array could thus also be composed either of 4 vertical rectangles and 4 horizontal rectangles, or of 5 rectangles of one category and 3 rectangles of the other category. Immediately after this second display, a third was presented, this time only for 40 ms. This third array was exactly identical to the second array, except that one rectangle was now colored in white instead of black. A central fixation cross was presented for 1500 ms before the onset of the first display.

**Experimental design**

One central aspect of our experimental design concerned the relationship between the changed item and the features of the highlighted item. Each of the eight possible locations was probed in 64 trials for a total of 512 trials.

Among the trials, 25% were “catch” trials in which no change occurred between the first and the second array. Amongst those catch trials, 50% included 4 horizontal and 4
vertical rectangles, 25% included 3 vertical and 5 horizontal rectangles, while the remaining 25% included 5 vertical and 3 horizontal rectangles (see Table 1 for a global overview).

Among the trials, 25% were “valid change” trials, in which an orientation change occurred between the first and the second array (e.g., a vertical rectangle rotates 90°), and in which the highlighted rectangle in the third screen was the same one as the rectangle that had changed orientation between the first and second arrays. Half of these valid change trials began with 4 horizontal (H) and 4 vertical (V) rectangles in the first display, and finished in either a 3V/5H or a 5V/3H configuration on the third display (each configuration occurred in 50% of the cases). For the other half of the valid change trials, 50% used an initial 3V/5H configuration and a final 4V/4H distribution, while the other 50% used the reverse pattern.

The remaining 50% of the trials were “invalid” trials, in which the location of the change and the location of the probe differed. Half of those invalid trials were congruent: the final orientation of the changed item and the probe’s orientation were identical. The other half of the invalid trials were incongruent: the final orientation of the changed item and the probe’s orientation differed. Finally, the different possible configurations of vertical and horizontal rectangles (3V/5H, 5V/3H, 4V/4H) were also counterbalanced for the initial and final displays of the invalid congruent and invalid incongruent trials. Another important feature of the procedure is that the total number of vertical and horizontal probes was equal over the entire experiment.
Procedure

Participants performed a total of 512 trials. In addition, they were first trained for 20 trials with the probe shown for 200 ms and then with 40 trials with the probe displayed for 40 ms in order to train them to the task progressively. Observers were asked to stare at a central fixation cross until a probe appeared. They were instructed that in some cases, a change of orientation would occur and that they had to try to detect it. They were also told that one rectangle—the probe—would change color (from black to white) in the final display and that they had to press the “S” or “L” key with the indexes of both hands to indicate the orientation of the probe. For half of the subjects, the “S” key coded for “horizontal” orientation while the “L” key coded for “vertical” orientation. The reverse mapping was used for the other subjects. They were instructed to respond as quickly and as accurately as possible, and told that a speeding beep would occur after 1200ms if they had failed to respond.

Participants were then asked whether or not they had noticed the orientation change of one rectangle between the first and second display. They had to press the space bar only if they had seen the change and not to press any key if they had not seen the change. It was then a go/no-go response. After each trial, the next one began 2 sec after the orientation response. We used a very liberal criterion for awareness: subjects were instructed to say that they had perceived the change even if they were not sure at all. We chose this procedure in order to make sure that change identification for unaware trials could safely be attributed to implicit processes rather than to conscious knowledge held with low confidence. Hence, even if subjects had just a tiny feeling that a change might
have happened, they had to say that they had seen the change. We did not give feedback concerning the accuracy of the orientation response like in Fernandez-Duque and Thornton (2003) and Mitroff et al. (2002).

Results

We removed 3 subjects from the analysis either because their False Alarms (FA) rate was higher than their hit rate, or because both rates were strictly equivalent — suggesting poor understanding of the task or uncooperative behavior. Analyses were thus conducted on the data from the remaining 21 subjects.

Before analyzing potential congruency effects, we had to ascertain that subjects could perceive the stimulus, at least in some cases, and that their accuracy differed from chance level. To do so, we inspected hit and false alarm rates. The average hit rate was 56% while the false alarm rate was 27%, yielding an average d’ of 0.86 (average of all the individual d’s). A t-test performed on d’ compared to random performance (d’=0) showed that subjects were able to discriminate between change and no-change trials [t(20)=8.70 ; p<0.001].

Before going further in the analyses, it is important to determine whether overall performance level is comparable to previous studies. Accuracy was about 89%, while average RT was 750 ms. Comparing to previous studies, both RTs and accuracy rate seem to be roughly in the same range as in Fernandez-Duque and Thornton’s (2003) second experiment. Note, however that, like in our study, Fernandez-Duque and Thornton (2003) used a 40 ms probe in their second experiment while Mitroff et al. (2002) used a 20 ms probe.
Orientation task accuracy

**Congruency effect**

Data from invalid trials were submitted to an ANOVA with two within-subject variables: *awareness of the change* (2 levels, seen vs. not seen) and *congruency* (2 levels, congruent vs. incongruent trials). We found a significant effect of *awareness* \[F(1, 20)=23.25 ; p<0.001\], accuracy being significantly higher for the unaware trials than for the aware trials, and a significant *congruency* effect \[F(1, 20)=14.32 ; p<0.001\], accuracy being higher for congruent trials than for incongruent trials. There was no significant *awareness* X *congruency* interaction \[F(1, 20)=1.80 ; p=0.194\]. We also analyzed aware and unaware trials separately and found a *congruency* effect both for unaware \[F(1, 20)=6.80 ; p<0.01\] and for aware trials \[F(1, 20)=8.50 ; p<0.008\] (see Figure 3).

**Validity effect**

Data from congruent trials were submitted to a within-subjects ANOVA with two variables: *awareness of the change* (seen vs. not seen) and *validity* (valid vs. invalid trials). The effect of *awareness* was marginally significant \[F(1, 20)=4.24 ; p=0.053\], which again suggests that the accuracy was higher for unaware than for aware trials. However, there was no main effect of *validity* \[F(1, 20)=0.05 ; p=0.826\]. We also found a significant awareness X validity interaction \[F(1, 20)=11.81 ; p<0.002\]. Analyzing aware and unaware trials separately, we found a significant “reversed” *validity* effect for the unaware trials \[F(1,20)=5.36 ; p<0.03\], and a *validity* effect for the aware trials \[F(1,23)=5.54; p<0.029\]: Mean accuracy was higher for invalid congruent trials than for
valid congruent trials when the change was not perceived consciously, while the opposite pattern of results was observed for the aware trials (see Figure 3).

**Reaction time**

**Congruency effect**

Data from invalid trials were submitted to an ANOVA with two within-subjects variables: *awareness of the change* (seen vs. not seen) and *congruency* (congruent vs. incongruent trials). We found a significant effect of *awareness* [F(1, 20)=29.93 ; p<0.001], RT being much longer for aware trials than for unaware trials, and a significant *congruency* effect [F(1, 20)=8.40 ; p<0.008], RT being longer for incongruent trials than for congruent trials. There was no significant interaction between those two variables [F(1, 20)=0.01 ; p=0.945]. However, we found a *congruency* effect for the unaware trials [F(1, 20)=14.13 ; p<0.001] but not for the aware trials [F(1, 20)=2.35 ; p=0.142] (see Figure 4).

**Validity effect**

Data from congruent trials were submitted to an ANOVA with two within-subjects variables: *awareness* of the change (seen vs. not seen) and *validity* (valid vs. invalid trials). We found a significant effect of *awareness* [F(1, 20)=14.49; p<0.001], but no *validity* effect [F(1, 20)=2.90 ; p=0.104]. Moreover, we found a reliable interaction between both variables [F(1, 20)=10.89; p<0.003]. There was a highly significant *validity* effect for the aware trials [F(1, 20)=9.11; p<0.006], but not for unaware trials [F(1, 20)=1.85; p=0.189] (see Figure 4). To summarize, we found a significant main *congruency* effect for accuracy. Post-hoc planned comparisons showed that this
congruency effect was significant both for aware and unaware trials. Concerning RTs, we found a significant main congruency effect and no interaction with awareness. Moreover, post-hoc planned comparisons showed that the effect is reliable for the unaware trials. The validity effect was found both in the accuracy and in the RT data, but only for aware trials. It therefore seems that a robust congruency effect was found for both aware and unaware trials on both accuracy and RTs, whilst a validity effect was found for aware trials only.

Far – Close analysis

An important issue is to determine whether the congruency effect we obtained is modulated by the location at which the change occurred. To find out, we followed Fernandez-Duque and Thornton (2003)’s method and split our data into “far” and “close” trials. “Close” trials are invalid trials for which the change had occurred within 45° of the location at which the probe appeared. “Far” trials group all the remaining invalid trials, that is, trials for which the change had been located at 90, 135, or 180° from the location at which the probe appeared.

Accuracy for the orientation task

Data from invalid trials were submitted to an ANOVA with three within-subjects variables: location of the probe relative to the location of the change (close vs. far change, see previous paragraph), awareness of the change (seen vs. not seen) and congruency (congruent vs. incongruent). We failed to find a significant effect of location [$F(1, 20)=1.67 ; p=0.210$], but the analysis revealed significant effects of awareness
Implicit change detection

[F(1, 20)=31.03 ; p<0.001] and of congruency [F(1, 20)=15.00; p<0.001]. None of the 2-way or 3-way interactions reached significance (see Figure 5).

To ascertain whether congruency effects were significant for “close” and “far” trials, we conducted planned comparisons. The rationale of this analysis is that if the congruency effect is truly based on the identification of the change and not merely on identification of its location, we should observe a significant congruency effect for both “far” and “close” trials. The first planned analysis compared accuracy between congruent and incongruent trials for the close and aware trials. This analysis only revealed a trend for a congruency effect [F(1, 20)=3.60 ; p=0.072]. The second planned analysis compared accuracy between congruent and incongruent trials for close but unaware trials. This analysis did not reach significance [F(1, 20)=0.66 ; p<0.425]. A third planned comparison performed on far and aware trials revealed a significant congruency effect [F(1, 20)=7.18 ; p<0.01]. Finally, a fourth planned comparison performed on far and unaware trials also revealed a significant congruency effect [F(1, 20)=7.63 ; p<0.01]. Hence, we observed congruency effects only for “far” trials, regardless of whether subjects had become aware of the change or not. Presumably, our failure to find similar effects for “close” trials stems from a lack of power.

**Reaction Time**

Data from invalid trials were submitted to a three way ANOVA with three within-subject variables: location of the probe relative to the location of the change (close vs. far change), awareness of the change (seen vs. not seen) and congruency (congruent vs. incongruent). We failed to find a significant effect of location [F(1, 20)=1.70 ; p=0.207]
but the main effect of *awareness* was significant \([F(1, 20)=38.41 ; p<0.001]\). We also found a significant *congruency* effect \([F(1, 20)=12.94 ; p<0.001]\). None of the 2-way or 3-way interactions reached significance (see Figure 6). As for the accuracy measure, we performed a series of planned comparisons to determine whether congruency effects were present for close and far positions. The first planned comparison revealed a significant *congruency* effect for the close and aware trials \([F(1, 20)=6.73 ; p<0.01]\). The second planned comparison also revealed a significant *congruency* effect for close and unaware trials \([F(1, 20)=9.70 ; p<0.005]\). A third planned comparison performed on far and aware trials was not significant \([F(1, 20)=1.35 ; p=0.258]\). Finally, a fourth planned comparison revealed a significant *congruency* effect for far and unaware trials \([F(1, 20)=6.38 ; p<0.02]\). As for our accuracy analyses, this suggests that there is a *congruency* effect for the “unaware” and “far” trials. In addition, we also found a congruency effect for the “unaware” and “close” trials. This suggests that the congruency effect does not interact with the location of the change.

**Number of stimuli 4/4 Vs 3/5**

As discussed in the introduction, in previous studies (Mitroff et al., 2002; Thornton & Fernandez-Duque, 2000; except in Fernandez-Duque and Thornton, 2003, experiment 4), the last frame of the trials always contained an unequal number of horizontal and vertical rectangles. Participants’ performance may therefore be biased when they have to decide of the orientation of the probe in the third display. For instance, they could respond faster when the probe is horizontal not because they (consciously or unconsciously) noticed the orientation change between the first and second display but merely because the number of
horizontal rectangles exceeds the number of vertical rectangles. To control for this potential confound, we introduced the initial configuration of the rectangles as an independent variable in a new series of analyses. We expect a congruency effect regardless of the configuration of H and V rectangles used.

**Accuracy for the orientation task**

Data from invalid trials were submitted to a three way ANOVA with three within-subjects variables: the initial *configuration* of the display (4V/4H vs. 3/5), *awareness of the change* (seen vs. not seen) and *congruency* (congruent vs. incongruent trials). We failed to find a significant main effect of *configuration* \[F(1, 20)=0.02 ; p=0.880\]. The main effect of *awareness*, however, was significant \[F(1, 20)=22.11 ; p<0.001\], as was the *congruency* effect \[F(1, 20)=13.16 ; p<0.001\]. None of the 2-ways interactions reached significance, but there was a trend for the 3-way *configuration* X *awareness* X *congruency* interaction \[F(1, 20)=3.62 ; p=0.07\] (see Figure 7). We performed planned comparisons to determine whether *congruency* effects were present for the 4/4 and the 3/5 initial *configurations*. The first planned comparison revealed a significant *congruency* effect for the 4/4 initial *configuration* and aware trials \[F(1, 20)=12.65 ; p<0.001\]. The second planned comparison, performed on 4/4 initial *configuration* for unaware trials, only revealed a marginally significant *congruency* effect \[F(1, 20)=3.61 ; p=0.071\]. A third comparison performed on 3/5 initial *configuration* for aware trials was not significant \[F(1, 20)=2.66 ; p=0.118\]. Finally, a fourth planned comparison performed on unaware trials with a 3/5 initial *configuration* indicated a significant *congruency* effect \[F(1, 20)=6.33 ; p<0.02\].
Implicit change detection

25

Reaction Time

Data from invalid trials were submitted to a three way ANOVA with three within-subject variables: the initial configuration of the display (4V/4H vs. 3/5), awareness of the change (seen vs. not seen) and congruency (congruent vs. incongruent trials). The main effect of the initial configuration was not significant \([F(1, 20)=1.70 ; p=0.207]\), but we found a significant effect of awareness \([F(1, 20)=32.27 ; p<0.001]\) and a significant congruency effect \([F(1, 20)=9.33 ; p<0.006]\). None of the 2-ways or the 3-ways interactions was significant (see Figure 8). We performed planned comparisons to determine whether congruency effects were present for the 4/4 and the 3/5 starting configurations. The first planned comparison contrasted congruent and incongruent trials for the 4/4 initial configuration and aware trials. It failed to reveal a congruency effect \([F(1, 20)=1.25 ; p=0.276]\). The second planned comparison evaluated the congruency effect for 4/4 initial configuration amongst unaware trials, and it also failed to be reliable \([F(1, 20)=1.25 ; p=0.276]\). A third comparison checked for congruency effects for 3/5 initial configuration in aware trials. This was significant, \([F(1, 20)=7.08 ; p<0.01]\). Finally, a fourth planned comparison concerned 3/5 starting configuration and unaware trials, and it was also significant \([F(1, 20)=22.83 ; p<0.001]\)

Comparison between Fast and “Slow” responders

In a final analysis, we compared performance for fast and slow responders. Indeed, if the congruency effect is reliably stronger for fast responders than for slow responders, it would strengthen the notion that fast RTs are necessary to obtain a congruency effect, as
previously shown by Fernandez-Duque and Thornton (2003)\(^1\). A first important point in this respect is that in our experiment, all participants should be considered as “fast” responders according to criterion used by Fernandez-Duque and Thornton (2003), as they all responded within 1000 ms. We therefore decided to set our criterion with which to separate fast from slow responders to be the median value of the mean RTs across every condition (751ms). This resulted in 11 subjects considered to be “fast” responders, and 10 considered to be “slow” responders. For accuracy, we obtained similar patterns of congruency effects for the very fast (RT<=751 ms) and slower (RT>751 ms) responders (F(1,10)=5.95 ; p<0.034 and F(1,9)=8.12 ; p<0.019 respectively). We also observed an effect of awareness in both groups F(1,10)=10.21; p<0.009 and F(1,9)=13.526; p<0.005 respectively. The congruency X awareness interaction failed to reach significance in either group of subjects (fast responders: F(1,10)=1.61 ; p=0.232 — slow responders: F(1,9)=0.20; p=0.662). For the RTs, the congruency effect was highly significant for fast responders (F(1,10)=10.08; p<0.009) but not for slow responders F=(1,9)=0.81; p=0.39). The effect of awareness was significant in both groups F(1,10)=13.93; p<0.003 and F(1,9)=14.720; p<0.003. The congruency X awareness interaction was significant neither for the fast responders [F(1,10)=0.79; p=0.395] nor for the slow responders [F(1,9)=1.83; p=0.208]. These results therefore reinforce the notion that fast responses are necessary in this paradigm, and suggest that Mitroff et al. (2002)’s failure to replicate was most likely due to the overall slower RTs of their participants.

\(^1\) We thank Ron Rensing for suggesting this analysis.
Summary of the results

The goal of this study was to explore whether implicit change detection occurs when all known potential biases previously described in the literature were controlled for. Our results repeatedly indicated a congruency effect, not only for aware but also for unaware trials, and are therefore clearly in favor of the implicit change detection hypothesis previously defended by Fernandez-Duque and Thornton (2003). More specifically, we replicated a congruency effect for the unaware trials, both in terms of accuracy as in terms of reaction time. More detailed analyses concerned interactions between the location of the change and the occurrence of a congruency effect. We could not replicate Fernandez-Duque and Thornton (2003)’s 3-way interaction here, suggesting that congruency effects might be dependent of both “closeness” and awareness of the change. However, in constrast to Fernandez-Duque and Thornton (2003), we found a congruency effect for “far” unaware trials, on the accuracy data. This rules out purely spatial accounts of the congruency effect, and suggests instead that congruency effects truly depend on the identity of the change rather than simply on its localization. We also found this effect in various other conditions, and also for the RTs, which suggests that the congruency effect we observed for unaware trials is relatively robust.

We also explored of the extent to which the initial configuration of stimuli influences performance—a factor that we had identified as a potential confound in previous studies. Here, we only found a trend for congruency effects in the accuracy data associated with 4/4 initial configurations when subjects had reported being unaware of the change, but the effect was significant for aware trials. In the case of 3/5 initial configurations,
however, we found a significant congruency effect for unaware trials, but not for aware
trials. A slightly different pattern emerged from the RT data, but altogether, we found
congruency effects over a range of conditions in which such effects were not supposed to
emerge according to Mitroff and al. (2002)’s account that subjects use explicit, conscious
strategies in this situation.
Discussion

Can changes that occur in visual displays and that fail to be registered by participants nevertheless influence subsequent decisions? What is the fate of undetected changes? Such questions are good starting points to think about the differences between conscious and unconscious information processing. A simple way to address these issues is to ascertain whether an undetected change can nevertheless prime subsequent processing. Thornton and Fernandez-Duque (2000) reported just such an effect using a paradigm in which a change in the orientation of one of several items could prime a subsequent orientation decision about another item (a congruency effect) even when subjects reported having failed to perceive the change.

Because this original finding later proved to be controversial, we set out to explore whether we could replicate Thornton and Fernandez-Duque’s “implicit change detection” effect. Our main approach has been to integrate, in a single experiment, controls for all known methodological biases identified in previous studies. Using this improved design, we found congruency effects (i.e., faster RTs and fewer errors) even for those trials for which subjects reported not having perceived the change. Our results therefore replicate Fernandez-Duque and Thornton (2003)’s findings, and are thus congruent with the notion that changes in visual displays can be processed unconsciously. In the remainder of this discussion, we would like to address three central issues. First, do our findings indeed suggest that changes in visual displays may be processed unconsciously up to fairly sophisticated levels that involve, for instance, identifying the nature of the change? Second, what are the mechanisms involved? Third, what other evidence is there to
support the notion that undetected changes can influence subsequent processing? Here, we focus on converging evidence obtained through functional brain imaging and physiological measures in addition to behavioral methods.

**Is implicit change detection implicit?**

Fernandez-Duque and Thornton’s (2003)’s interpretation of implicit change detection not only requires implicit registration, but also implicit identification of the change. Three elements seem crucial for change identification to occur: Representations of both pre- and post-change displays must exist (which seems to be the case, see Mitroff *et al.*, 2004), and a comparison process capable of indicating the nature of the change, if any, must operate on these two representations. For implicit change identification, all of this also needs to occur outside conscious awareness. Fernandez-Duque and Thornton’s interpretation therefore postulates relatively sophisticated implicit processes.

This being said, other evidence suggests that complex visual processes might operate outside conscious or attentional processes. As discussed in the introduction, the context created by an array of unattended distractors can cue the location of the target in a visual search task (Chun & Jiang, 1998). Moore and Egeth (1997) also showed that observers continued to be sensitive to illusory arrays of dots even while failing to experience the illusion because their attention was engaged elsewhere.

In this context, the replication of the implicit perception of the *nature* of a change is an important result, not only because it suggests that change perception can occur implicitly, but also because this form of implicit perception can involve sophisticated processes and
representations reflecting the *identity* of a change and not its mere *localization*. Our data are particularly convincing in this respect insofar as the congruency effect for unaware trials is more reliable when the change occurred at a “far” location from the probe. Fernandez-Duque and Thornton (2003), by contrast, only reported a congruency effect for unaware trials when the change occurred at a location close to the probe. Contrary to ours, this result might suggest that participants were (consciously) paying attention to the localization of the probe when the orientation change occurred.

One could argue, however, that change detection was at least partly supported by conscious perception left undetected by the rather insensitive awareness test that we used in our experiment. Indeed, if the awareness test lacks sensitivity, change detection might be attributed to unconscious processes not because it indeed took place outside awareness, but simply because the awareness test did not compel participants to report the actual conscious information on which their performance was based. It might be the case for instance that, given the difficulty of the task, conscious perception was associated with a very low level of confidence and was therefore not reported by the participants. It is interesting to note that the very same issues continue to elicit debate in related fields, such as implicit learning (Shanks & St. John, 1994) or subliminal perception (Draine & Greenwald, 1998; Hannula et al., 2005; Holender, 1986; Merikle & Reingold, 1998; Snodgrass & Shevrin, in press).

In our experiment, we used, like Fernandez-Duque and Thornton (2003), a rather limited measure of awareness: We simply asked subjects if they had seen the change or not. However, we instructed them to use a liberal criterion, that is, to respond that they had
not seen the change only when they were certain that they had not seen anything. We used this particular set of instructions to ensure that “unaware” trials effectively corresponded to cases in which participants had failed to consciously perceive any aspect of the orientation change. Moreover, this set of instructions also tends to increase the level of confidence associated with responses to the unaware trials as compared to the aware trials (Mitroff et al., 2002).

This procedure might explain why we observed an awareness effect in RT and accuracy (i.e. more errors and slower RT for aware than for unaware trials). As participants only responded “unaware” when they were confident of not having seen the change, they did not have to thoughtfully consider whether or not they had perceived something. This process, which requires both time and resources, must take place for the “aware” trials. Another interpretation would be that aware trials are slower because it takes some time for change perception to reach conscious awareness. This latter interpretation, however, would lead one to consider “aware” trials as cases in which the orientation change was effectively perceived consciously whereas, given the difficulty of the task, it is in fact more plausible that participants were uncertain of their perception for most of the “aware” trials. This interpretation is also supported by the important rate of false alarms (27%) observed for the aware trials.

We are confident that our use of a liberal criterion compensates for the relative insensitivity of the awareness test in our experiment, and that the unaware trials indeed corresponds to cases in which there was no or, at most, extremely low levels of conscious perception. We also believe, however, that the sensitivity of the awareness test should be
improved in order to provide a more precise assessment of implicit change detection. An interesting way to extend awareness measurement in this task would be to add a subjective confidence rating to the change detection report. Such a procedure has been previously used in the domains of subliminal perception (Cheesman & Merikle, 1984) and implicit learning (Destrebecqz & Cleeremans, 2003; Dienes et al., 1995; Dienes & Berry, 1997; Shanks & St. John, 1994) and consists in asking participants to rate on a graded scale, from “guess” to “certain”, how confident they were in their responses.

According to Cheesman and Merikle (1984), perception is unconscious when it is under the subjective threshold, that is, when participants are able to identify the target at above chance performance while stating that they were guessing and did not perceive it consciously. Reingold and Merikle (1990), however, have insisted that subjective measurement of unconscious perception must be interpreted with caution because it depends on the participant’s interpretation of the task instructions. For instance, participants might give a more liberal interpretation to the term “guess” than the experimenter does, or assign a high level of expectancy to the experimenter and therefore tend to claim that they were guessing while they in fact basing their responding on low-confidence, fragmentary, but nevertheless conscious knowledge.

A method of particular interest in this context has been introduced by Kunimoto et al. (2001). As a possible solution to the potential bias of subjective measurement, they propose to use a two alternatives forced-choice confidence rating associated with the awareness test. They argue that this method makes it possible to use Signal Detection Theory, which can deal with biases in criterion. The idea is to compute d’ combining
detection performance and confidence ratings. In this framework, a correct response given with high confidence is considered as a hit; a correct response associated with low confidence is viewed as a miss; an incorrect response given with high confidence is considered as a false alarm and an incorrect response associated with low confidence is classified as a correct rejection. This method allowed Kunimoto et al. (2001) to find evidence for subliminal perception even for unbiased measurement of participants’ awareness. This method allows one to truly dissociate perception from awareness, in the sense that subjects can discriminate different kinds of stimuli at a better-than-chance level while their confidence rating is not predictive of their performance and hence suggests that subjects have no conscious access to the information they use. The added value of this method is thus that response biases are neutralized thanks to SDT.

This method could be nicely applied to the implicit change detection paradigm. It might provide new information regarding the confidence of the observers and allow control for the potential biases described above. It is important to note that this idea is rooted into the notion that consciousness should be considered as a gradual phenomenon. Indeed, it acknowledges the fact that in some cases, fragmentary or imperfect perception can induce phenomenal states that fall between full awareness and complete lack of awareness of some features of the material—states that may lead participants to consider themselves as partially aware of a stimulus.

In conclusion, we are quite confident that the information perceived by our participants in unaware trials was so weak and noisy that they can be described as truly unaware. Two aspects of our results specifically support this conclusion. First, the fact that accuracy is
higher for unaware than for aware performance seems hard to explain by assuming that performance on unaware trials can be accounted for by weak and fleeting conscious perception of the change, for if this were the case, one would expect performance on such trials to be lower than performance on aware trials. What we found instead is increased accuracy on unaware trials. Second, though we did not expect to observe an interaction between validity and awareness, such a qualitative difference between aware and unaware trials (i.e., a validity effect for the aware trials and a reverse validity effect for the unaware trials) suggests that conscious perception and unconscious sensitivity appeal to different mechanisms in this task (Reingold & Merikle, 1990). Though establishing the extent to which observed dissociations depend on separate, overlapping, or unitary information processing systems is notoriously difficult (Dunn & Kirsner, 1988), we nevertheless offer a few reflections on these issues in the next section.

**What are the mechanisms of implicit change detection?**

It seems difficult to explain our data by means of the dominant models defended in the change blindness or in the Visual Short Term Memory (VSTM) literatures. Indeed, in the field of change blindness research, it is often claimed that change detection requires focused attention that can only be allocated to a few items at a time (see for instance Mitroff et al., 2002; see for instance Rensink, 2000a; Rensink et al., 1997). Similarly, in the VSTM literature, the dominant view claims that the VSTM capacity limitations strongly restrict the number of items that can be encoded at once (e.g. Luck & Vogel, 1997). From this perspective, only a few items, typically 3 or 4, can be encoded from a visual display and then compared with the subsequent view.
However, it is important to note that some authors proposed that implicit and explicit processes are subtended by different mechanisms. Thus, according to Rensink (2002) and Thornton and Fernandez-Duque (2002), focused attention is required for a change to be *consciously* detected. Thornton and Fernandez-Duque (2002) further suggested that the representation of a change built by attentional processes can remain unavailable to consciousness. In their view, attention and awareness are thus not synonymous. Attention can be seen as a functional process modulating information processing while awareness is defined as an “attribute of the represented stimulus” (Thornton & Fernandez-Duque, 2002, p103). Other researchers have also claimed that attention and awareness should be distinguished from each other (i.e. Lamme, 2003).

Our results indicate that conscious access does not take place in unaware change identification trials. We cannot say, however, whether attention is involved in these cases or not.

An alternative perspective proposed by Wilken and Ma (2004), however, offers a very different conceptualization of visual perception, one that is based on the notion that neural activity is inherently noisy. Thus, Wilken and Ma (2004) claim that an observer who attempts to detect a change at *n* locations is monitoring *n* noisy channels. A change in an item will produce a change in a channel but, because the signal to each channel is noisy, there is a probability that the signal coming from a no-change channel will be above threshold and produce a false alarm (Wilken & Ma, 2004).

Thus, in this framework, the entire visual display is assumed to be encoded, but in such a way that the corresponding representations contain a certain level of noise. Note also that
Landman, Spekreijse, & Lamme (2003) likewise defend the view that the entire visual display is encoded and stored; however, they consider that overwriting of the pre-change representation by the post-change representation is what causes change blindness (see also Lakha & Wright, 2004 for similar findings and discussion). When a change occurs, the pre-change representation will be compared with the post-change representation within each channel. Importantly, these representations are graded rather than all-or-none: They vary in quality depending, among other factors, on the level of noise, which in turn depends on the time available for processing. Implicit change detection, in this framework, would thus take place whenever the changes that occur in a channel are of a sufficient strength to produce priming, yet weak enough not to reach awareness (see also Cleeremans, 2005; Cleeremans & Jiménez, 2001 for similar ideas).

The notion of noisy representations is also defended by Baldassi and Burr (2000) who proposed a “featured-based integration of orientation signals in visual search”. In this perspective, observers could access the different orientations in a display containing up to 8 grating patches displayed for 100 ms. They also found that, when adding noise to the display, the identification of the orientation of a target was at a much lower threshold than its location. Hence, the observers can discriminate the orientation of rectangles without being able to locate them. The authors suggested that performance was based on a simple integration mechanism that quickly extracts the global aspect of the display.

In the same vein, using a different paradigm, Ariely (2001) showed that observers can extract information about the average characteristics of a set of items presented for 500 ms but cannot access specific information concerning the individual items within the sets.
He concluded that when presented with sets of items, the visual system might create representations reflecting the general characteristics of the set but discarding information about the individual items. However, we cannot determine whether such a global comparison mechanism of some average characteristics between the first and the second display is operating in our experiment or whether a more focused mechanism centered on individual items is at play. Nevertheless, our results clearly suggest that this mechanism is implicit. We now turn to converging evidence from functional imaging studies of change blindness and change detection.

**Evidence from imaging and electrophysiological studies**

Our results suggest not only that non-consciously perceived changes can nevertheless be represented, but that they can also influence subsequent behavior. Numerous recent studies have addressed the relationships between subjective experience of a stimulus and its neural correlates in an attempt to elucidate the neural bases of conscious awareness.

As a case in point, a recent study by Haynes and Rees (2005) suggests that sometimes, subjects’ brains seem to know more than they can tell. Haynes and Rees used sophisticated signal processing techniques to demonstrate that it is possible to predict (albeit not perfectly) what stimulus (gratings oriented to the left to the right) a subject has been exposed to based on a single fMRI image of activity in their visual cortex—a method they dubbed “mindreading”. Strikingly, this was the case regardless of whether or not subjects had actually consciously perceived the stimulus. Indeed, whereas a first experiment, Haynes and Rees used visible gratings, in a second they used subliminal gratings rendered invisible by masking, and that subjects were also shown unable to
detect the orientation of despite prolonged exposure lasting up to 15 seconds. Interestingly, Haynes and Rees found graded differences in V1, V2 and V3 activity between the two conditions. V1’s activity, for instance, was predictive of the stimulus regardless of whether or not subjects had perceived it, but more so when they had then when they had not. V2 and V3, on the other hand, were only active when subjects had consciously perceived the stimulus. Haynes and Rees concluded by pointing out that “Whether to be represented in conscious experience information has to cross a threshold level of activity, or perhaps needs to be relayed to another region of the brain, is an interesting question for further research”. (p. 689). Thus, it seems possible that a stimulus that is not perceived consciously nevertheless produces brain activation similar to those that produced by the same stimulus but when perceived consciously. We will now turn to studies exploring the neural correlates of undetected changes in change blindness situations.

A recent fMRI change blindness experiment has shown that specific regions of the extrastriate cortex are implicated when participants are exposed to a change that they fail to detect consciously compared to a situation in which no change occurs in the display (Beck et al., 2001). Moreover, conscious detection of a change was associated with increased activation in bilateral parietal and in right dorsolateral frontal areas compared to situations in which the subjects failed to consciously report the change. The specific pattern of activation observed when the change is not consciously reported suggests that implicit change detection might occur. However, further studies should replicate this result as it was only found for a subset of the stimuli used in the study (pictures of faces) and only in some subjects (6 out of 10). Another study, using ERP methods, found a
specific pattern of brain activity associated with the presence of a change without conscious detection, which was different from the pattern associated with explicit change detection (Fernandez-Duque et al., 2003). The authors concluded that this specific activation reflects implicit change detection. Other researchers concluded, based on similar differences, that it merely reflects differences in the level of subjects’ confidence regarding their response rather than a difference in the availability to consciousness of the perceived change (participants should be more confident when they see the change than when they don’t see it) (Eimer & Mazza, 2005).

To sum up, these results using different brain imaging methods and the behavioral findings seem to indicate that implicit change detection can occur, as a change that is non consciously detected is capable of (1) influencing further behavior, (2) yielding a pattern of brain activity that is different from the pattern of activity resulting from a non-changing stimulus.

**Conclusion**

We replicated Thornton and Fernandez-Duque (2000)’s original findings under better controlled experimental conditions. This result suggests that weak and noisy spatio-temporal representations can nevertheless influence subsequent behavior. Because participants remain unaware of such representations and of their influence on their behaviour, they can be described as implicit. Our replication findings extend previous results and interestingly elaborate on subliminal perception findings, for in contrast to the latter, implicit change perception requires integration over two different displays over time.
References


Rensink, R. A., ORegan, J. K., & Clark, J. J. (1997). To see or not to see: The need for attention to perceive changes in scenes. Psychological Science, 8(5), 368-373.


Acknowledgments

CL is a scientific research worker with the national Fund for Scientific Research (Belgium). AC is a senior research associate with the same institution. This work was supported by an institutional grant from the Université Libre de Bruxelles.
Figure captions

<table>
<thead>
<tr>
<th>%</th>
<th>Change category</th>
<th>Configuration of the 1\textsuperscript{st} display</th>
<th>Configuration of the 2\textsuperscript{nd} display</th>
</tr>
</thead>
<tbody>
<tr>
<td>25%</td>
<td>Catch trials</td>
<td>64       32     32</td>
<td>64       32     32</td>
</tr>
<tr>
<td>25%</td>
<td>Valid change</td>
<td>64       32     32</td>
<td>64       32     32</td>
</tr>
<tr>
<td>25%</td>
<td>Invalid congruent change</td>
<td>64 32 32</td>
<td>64 32 32</td>
</tr>
<tr>
<td>25%</td>
<td>Invalid incongruent change</td>
<td>64 32 32</td>
<td>64 32 32</td>
</tr>
</tbody>
</table>

Table 1. Number of repetitions of a same configuration (H and V distribution) for one subject.
Table 2: Summary of the previous experiments investigating implicit change identification. The result column is a very brief reminder of the results found in the different experiments. The column “Confounds eliminated” describes the confounds eliminated compare to the previous experiments and the column “Remaining confounds” describes briefly the potential confounds still present in the experiment and that we corrected in the present study.
Figure 1: Time course of one trial used by Thornton and Fernandez-Duque (2000).
Figure 2: Example of different types of trials used in the present study. Note that these stimuli are slightly different to those used by Thornton and Fernandez-Duque (2000) because a “spacial link” has been eliminated compared to this initial study (see the main text for a description of the “spatial link”).
Figure 3: Accuracy for the orientation judgment task as a function of the conditions for aware and unaware trials. VC = valid change trials, IC = Invalid Congruent, II = Invalid Incongruent and NC = No change.
Figure 4: RTs for the orientation judgment task as a function of the conditions for aware and unaware trials. VC = valid change trials, IC = Invalid Congruent, II = Invalid Incongruent and NC = No change.
Figure 5: Accuracy for the orientation judgment task as a function of the conditions for aware and unaware trials and for close and far changes as compare to the location of the probe. CC = Close and Congruent trials, CI = Close and Incongruent trials, FC = Far and Congruent trials and FI = Far and Incongruent trials.
Figure 6: RTs for the orientation judgment task as a function of the conditions for aware and unaware trials and for close and far changes as compare to the location of the probe. CC = Close and Congruent trials, CI = Close and Incongruent trials, FC = Far and Congruent trials and FI = Far and Incongruent trials.
Figure 7: Accuracy for the orientation judgment task as a function of the conditions for aware and unaware trials and for 4 Vertical (V)/4 Horizontal or 3 V (or H) /3 H (or V). 44C = 4V/4H and Congruent trials, 44I = 4V/4H and Incongruent trials, 35C = or 3 V (or H) /3 H (or V) and Congruent trials and 35I = or 3 V (or H) /3 H (or V) and Incongruent trials.
Figure 8: RTs for the orientation judgment task as a function of the conditions for aware and unaware trials and for 4 Vertical (V)/4 Horizontal or 3 V (or H)/3 H (or V). 44C = 4V/4H and Congruent trials, 44I = 4V/4H and Incongruent trials, 35C = or 3 V (or H)/3 H (or V) and Congruent trials and 35I = or 3 V (or H)/3 H (or V) and Incongruent trials.