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On the dissociation between compound and present/absent tasks in visual search: Intertrial priming is ambiguity driven

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Visual search is speeded when the target-defining property (a feature- or dimension difference relative to the distractors) is repeated relative to when it changes. It is thought that automatic and implicit intertrial priming mechanisms underlie this effect. However, intertrial priming has been found to be less robust in compound search tasks (in which the response property is unrelated to the target-defining property) than in present/absent search tasks (in which the response is directly related to the presence of a target-defining property). This study explored the hypothesis that intertrial priming is dependent on the level of ambiguity in a task, with the present/absent task being inherently more ambiguous than the compound search task. The first three of five experiments further established the dissociation between the tasks and excluded alternative explanations. Intertrial priming was strong in present/absent and go/no-go tasks, but absent in compound and compound/absent tasks. The last two experiments supported the ambiguity hypothesis by introducing more uncertainty in the compound task, after which intertrial priming returned.

It is becoming more and more evident that the human attention system is subject to implicit adaptation mechanisms that, without the observer's awareness, continuously change the system's sensitivity to an altered environment. An example of this is the phenomenon of priming in visual search.

In a typical visual search task, the observer is looking for a target object that differs on a certain dimension (e.g., colour, orientation, shape, or a particular combination of these) from a set of spatially dispersed distractor objects. The task usually is to either determine the presence or absence of the target (''present/absent search''), or to determine the nature of an inconspicuous

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feature of the target, which in that case is always present. The latter task is referred to as a ''compound search'' (after Duncan, 1985) and in a typical example the to-be-responded-to property would be a letter or a left- or right-pointing arrowhead placed inside the target.

Maljkovic and Nakayama (1994) have shown that when the target-defining feature is uncertain from trial to trial, repeating the same target feature results in shorter reaction times (RTs). They used a compound search task in which the target could be either a red or a green diamond among distractors of the opposite colour (i.e., green when the target was red and vice versa; the target was always present). The participant's response was determined by which side of the diamond had a small piece of its corner cut off (left or right). When the same colour was repeated from one trial to the next, RTs were shorter than when the colour switched, regardless of whether or not the response was repeated. Maljkovic and Nakayama referred to this finding as ''priming of pop-out'', but comparable intertrial priming effects have been found for more difficult searches in which the target does not pop out of the display (Hillstrom, 2000; Kristjánsson, Wang, & Nakayama, 2002). Similar intertrial effects have also been observed with present/absent tasks. Müller and colleagues (Found & Müller, 1996; Müller, Heller, & Ziegler, 1995) asked participants to determine the presence of a target defined either by a unique orientation or a unique colour. In blocks in which target types were mixed, RTs were longer than in blocks containing only a single target type. It was found that intertrial effects largely underlay these differences between mixed and pure blocks, because RTs were particularly slow when the target-defining dimension changed from one trial to the next.

Evidence for the implicit nature of these intertrial effects comes from several studies. Maljkovic and Nakayama (1994; see also Hillstrom, 2000; Olivers & Humphreys, 2003) reported influences from targets presented as far back as eight trials. It is unlikely that observers consciously base their search strategy on these distant trials. Indeed, Maljkovic and Nakayama (2000) as well as Müller, Krummenacher, and Heller (2004) have shown that observers hardly remember what they saw even on the immediately preceding trial. In another experiment from Maljkovic and Nakayama (1994), observers could explicitly expect a switch trial (because they alternated regularly with same trials). This did not eliminate the costs associated with switch trials, indicating that intertrial priming effects were not the result of, and could not be overcome by, strategic attentional control. In a similar vein, Theeuwes, Reimann, and Mortier (in press) found that, in a compound search task, providing observers with a verbal cue as to which target-defining dimension to expect (colour or shape) had little effect on RTs. Instead, providing observers with an image of the actual target resulted in a significant priming effect even when the cue was 80% invalid. Theeuwes et al. concluded that observers have little conscious control over which dimension should receive priority in attentional selection.

THE PRESENT STUDY

The present study focuses on an apparent discrepancy in the literature on intertrial priming effects in search. Whereas such effects are generally robust in present/absent search tasks (Found & Müller, 1996; Kristjánsson et al., 2002; Müller et al., 1995; Wang, Kristjánsson, & Nakayama, 2005; Wolfe, Butcher, Lee, & Hyle, 2003), they have been shown to be much reduced or even absent in compound search tasks (Cohen & Magen, 1999; Kumada, 2001; Mortier, Theeuwes, & Starreveld, 2005; Theeuwes et al., in press). For instance, Kumada (2001) found intertrial priming in a search task when participants were required to determine the presence of an orientation- or colour-defined target, but not when they were required to determine the direction of a left- or right-pointing arrowhead inside the very same types of target. Since the stimuli had remained identical and only the required response had changed, Kumada concluded that intertrial priming is response specific. Similarly, Theeuwes et al. (in press) found that intertrial effects were stronger in a present/absent task than in a compound task. In the same vein, Cohen and Magen (1999) failed to observe overall costs in a compound search task in which the target dimension could change, as compared to a compound search task in which the target dimension remained constant. (It is worth noting though that Cohen and Magen's task deviated from the more typical compound task, a point to which we return in the General Discussion.)

The picture becomes more complicated when we consider that although these studies indeed have reported the absence or reduction of intertrial priming effects in compound search, other studies have actually shown rather robust effects in compound search (Goolsby & Suzuki, 2001; Hillstrom, 2000; Huang, Holcombe, & Pashler, 2004; Maljkovic & Nakayama, 1994, 2000; Olivers & Humphreys, 2003; Pinto, Olivers, & Theeuwes, in press). To explain this apparent discrepancy, we propose an *ambiguity resolution* account of intertrial priming. The account states that intertrial priming becomes functional, and therefore measurable, only under circumstances of ambiguity. Ambiguity refers to the presence of uncertainty, conflict, or competition at any level between stimulus and response. The most straightforward case is when the ambiguity resides in the stimulus, for instance when the target is not the only unique item in the display, but accompanied by a unique (''singleton'') distractor. The exact identity of the target then becomes important and its defining dimension may receive additional activation. This activation may carry over to the next trial and may be measured as a priming effect. Conversely, this carried-over activation in turn becomes more useful if the next trial is also ambiguous, further enhancing the intertrial priming effect. Elsewhere (Meeter & Olivers, in press) we have shown that increased stimulus ambiguity indeed leads to increased priming effects.

However, in a more subtle way, the ambiguity may also be induced on a higher level, namely on the level of the task definition. In this case the uncertainty or competition is expressed in the relationship between stimulus and response. An example is the present/absent search task. Intuitively, this task appears simpler than the compound search task (which requires an additional judgement of the response feature), but in our view it is nevertheless more ambiguous. This is because, by nature, the present/absent task is a signal detection task (Palmer, 1995; Tanner & Swets, 1954). The observer tries to respond as quickly as possible to the presence of a deviating signal in the visual array. The problem is that noise (either perceptual or stimulus based) makes this decision often uncertain: Sometimes spurious activity is treated as a target, and sometimes a less salient target is missed. The observer will therefore need to set a response criterion. Any activity above this criterion will lead to a ''present'' response; otherwise an ''absent'' response will be given. This also explains the reasonably substantial error rates typically found in present/absent tasks. Crucially, under these ambiguous circumstances, the response decision will benefit from target repetition because, as the target is repeated, evidence for target presence accumulates faster, as is evident from electrophysiological recordings (Bichot & Schall, 2002). Due to the accumulated perceptual evidence, the criterion for a present response is met more often and more rapidly, resulting in an intertrial benefit relative to when the target is not repeated.

Like in the present/absent task, in the standard compound search observers will look as rapidly as possible for evidence for a specific response, in this case an inconspicuous response feature presented in or on the target. However, compound search differs from present/absent search in two ways: First there is no ambiguity concerning the presence of the target: It is always there. Typically therefore, much fewer errors are made than in the present/absent task. Second, there is no ambiguity in the stimulus–response relationship: They are independent. The target identity says nothing about the required response (of course, the response feature inside the target does, but we will return to this later), and accumulation of evidence for a specific target type is therefore ineffective. Note that a lack of ambiguity does not mean that the response is known or easily retrieved. Responses in compound tasks are usually much slower than in present/absent tasks. Instead, lack of ambiguity means that the relationship between stimulus and response is clear: In case of the compound task it is clear that these two are independent.

Here, we test this explanation of the dissociation between present/absent and compound search tasks. Experiment 1 directly compared the present/absent and compound tasks within the same group of participants, and found that priming was absent in the latter task. This is already an advantage over previous studies because so far the dissociation between these tasks has only been found across different studies or across separate groups of subjects within a study. Experiment 1 also served to exclude some of the alternative explanations of this dissociation between the present/absent task and the compound task. First, studies using present/absent tasks have generally relied on different types of target changes than studies using compounds search tasks. In the intertrial priming studies using present/absent tasks the target predominantly switches between different perceptual dimensions (e.g., colour and orientation), whereas in the compound tasks it switches between features on a certain dimension (e.g., red and green). Second, targets are present on all compound search trials, whereas they are present on only half the number of trials in present/absent tasks. This may have increased the relative salience of the target in the latter task. Experiments 2 and 3 controlled for yet more alternative explanations in terms of overall RTs (which tend to be substantially lower on present/absent trials than on compound trials) and specific response requirements on absent trials. Experiments 4 and 5 then sought to directly test the ambiguity account by reintroducing ambiguity in the compound task. Task uncertainty was increased by introducing a foil on absent trials, in the shape of a shape singleton, or by making the compound task alternate with the present/absent task. As predicted, when ambiguity was increased, intertrial priming returned.

EXPERIMENT 1: DIRECTLY COMPARING THE PRESENT/ABSENT AND COMPOUND TASKS

As outlined in the introduction, some studies have reported intertrial priming effects in a compound task (Hillstrom, 2000; Maljkovic & Nakayama, 1994), whereas others failed to find such effects or found substantially smaller effects (Cohen & Magen, 1999; Kumada, 2001; Mortier et al., 2005; Theeuwes et al., in press). In contrast, intertrial priming effects have been rather large and robust in present/absent tasks (Found & Müller, 1996; Kristjánsson et al., 2002; Müller et al., 1995; Wang et al., 2005; Wolfe, et al., 2003). Experiment 1 served to establish this dissociation again, but now within a single experiment—that is, within the same group of observers. In addition, Experiment 1 sought to explore further why this dissociation occurs. We believe that the present/absent task, in its classic form, is inherently more ambiguous than the compound task, because (a) there is uncertainty about the target's presence, and (b) this uncertainty results directly in uncertainty on the level of response. In other words, the stimulus–response link is inherently ambiguous, and intertrial priming helps to resolve this ambiguity.

However, there may be other explanations for the differential intertrial effects. One possibility is that absent trials (in the present/absent task) make the presence of a target relatively more salient, which then results in stronger priming effects. It has been shown that, in the spatial domain, increasing the number of nontargets may make the target more salient (Bravo & Nakayama, 1992). The same may also apply to the temporal domain such that target trials become more salient when following nontarget (i.e., absent) trials. To control for this, we introduced the *compound/absent* condition. In the compound/absent condition, the target was present on half the number of trials, absent on the other half. When the target was present, the participant's task was to determine the direction of an arrowhead placed inside it, just as in the standard compound task. When the target was absent, the participant simply pressed the space bar, just as in the present/absent task. Under our specific task ambiguity hypothesis, the introduction of absent trials in the compound task should have little effect on intertrial priming. This is because on ''present'' trials (i.e., the trials on which priming may operate) the response is still independent from the target properties. In other words, and in contrast to the present/absent task, accumulating evidence for a certain target type (e.g., colour or orientation) does not result in accumulating evidence for a particular response. If, on the other hand, intertrial effects occur because absent trials make the target more salient, then we should see such effects reemerge in the compound/absent task.

Another explanation of the dissociation between the two tasks follows a further line of division running through the literature, namely the distinction between dimension changes and feature changes. So far the majority of studies that have found intertrial effects in compound tasks have focused on feature changes within a specific dimension. On switch trials of Maljkovic and Nakayama's (1994) study, for example, targets changed only within the colour domain (from red to green and vice versa). Studies that failed to find intertrial effects have focused on dimension changes. An example is Kumada's (2001) study, in which targets changed between colour and orientation. On the other hand, present/absent tasks have always yielded robust intertrial effects, and these involved mainly dimension-based changes (Found, 1998; Müller et al., 1995). To rule out the possibility that a lack of intertrial priming had in any way to do with the use of dimension-based manipulations rather than feature-based manipulations, the present experiment was conducted on two groups of observers. In the dimension-based group the target changed on a dimension level. That is, on ''change'' trials it switched from being colour defined to being orientation defined (or vice versa), whereas on ''same'' trials it remained defined within the same dimension (even though it changed feature within this dimension, e.g., from red to green). In the feature-based group, the target changed on a feature-based level. That is, on ''change'' trials it could only switch feature within one dimension (either colour or orientation), compared to ''same'' trials in which the target stayed identical. An example is shown in Figure 1. Based on the compound search literature we might expect intertrial effects in the feature-based conditions, but not in the dimension-based condition. Based on the present/absent search literature we might expect the reverse.

Method

Participants. Forty-four observers participated (24 men, 20 women, mean age 22), all having self-reported normal eyesight and colour vision. Twelve observers participated in the dimension-based condition; thirty-two observers participated in the feature-based condition. The latter condition contained more

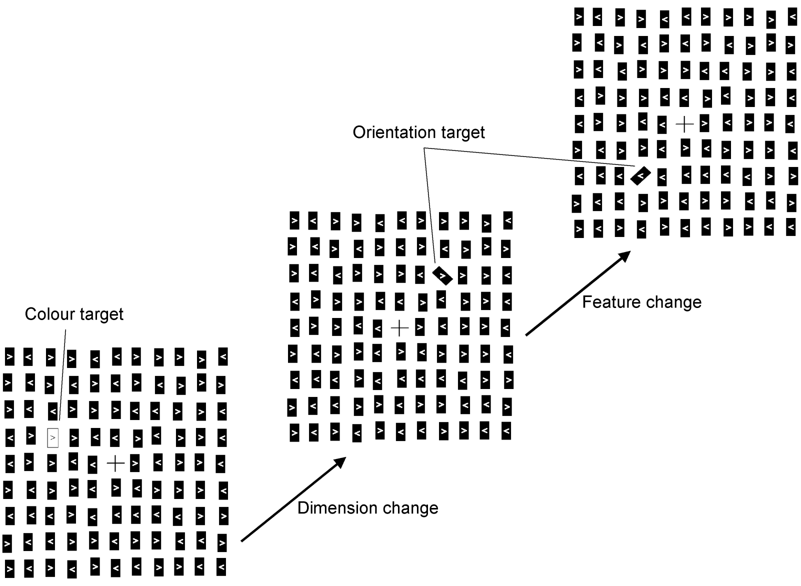

Figure 1. Examples of possible intertrial relationships in the present experiments. In reality, search items were grey on a black background and targets, when coloured, were either red or green. Here a target is present on all trials. In the present/absent task these trials were mixed with target absent trials. The ''<'' and ''>'' brackets were present in all tasks, but were relevant only in the compound task (in which they determined the response).

participants because the feature-based effects tend to be weaker than the dimension-based effects (that is, here and across the literature).

Stimuli and apparatus. All stimulus presentations and event timings were done using E-Prime (Psychological software Tools, Inc.), running on Compaq Pentium IV machines. Search displays consisted of an 11 × 9 grid filled with line elements, covering a 19.4 × 19.4 degree square of the visual field on a 19 inch monitor running in 800 × 600 SVGA mode, viewed from approximately 70 cm in a dimly lit room. The locations of the elements were slightly jittered with respect to the grid, by introducing continuously distributed noise in position with a spread of 0.5 degrees. All line elements except one were grey (12.0 cd/m^2), upright rectangles of 2.1 × 1.1 degrees. Within all line elements, an arrowhead pointing leftwards (''<'') or rightwards (''>'') was drawn. The target, when present was a singleton placed on an imaginary circle of 6.3° diameter around the fixation point. Targets were the same size as the nontarget elements, but were either coloured red or green (CIE x = 0.597, y = 0.366, luminance 9.51 cd/m^2 and CIE x = 0.254, y = 0.649, luminance 12.13 cd/m^2, respectively), or tilted left or right by 30 degrees.

Procedure. Each participant completed three tasks in counterbalanced order. In all tasks, trials began with a 500 ms presentation of a fixation cross. Then the search display was presented until the observer responded. Observers received visual feedback when they had made an error, and after each block they were informed of their mean reaction time and error rate. In the *present/absent* task, the target was present on half the trials. The ‘‘>’’ key was pressed with the right middle finger when the target was present; the spacebar was pressed with the left hand when the target was absent. In the *compound* task the target was always present, and participants responded to the arrowhead inside it by pressing the ‘‘<’’ and ‘‘>’’ keys with the right index and middle fingers. The *compound/absent* task was a combination of the present/absent and compound tasks. The target was present on half the trials, and, if so, participants responded as in the standard compound task. On absent trials, they responded as in the present/absent task (i.e., the spacebar was pressed with the left hand). Participants first received 24 practice trials of each task, after which two experimental blocks of each task were presented in counterbalanced order. Standard compound blocks contained 64 trials each, whereas the present/absent and compound/absent blocks contained 124 trials each. Four warm-up trials in each block were excluded from analyses. For one group of participants (in the dimension-based group) on half the number of target present trials the target dimension changed (between colour and orientation); on the other half the dimension remained the same (but changed feature value, e.g., from red to green). For the other group of participants (in the feature-based group), the target was always colour defined and on half the number of present trials it changed between red and green; on the other half it remained identical. Trial types within each condition were randomly mixed.

Results

Trials with response times longer than 1500 ms, less than 1% of the total, were not analysed. On average, observers made 4.6% errors in the dimension-based group and 3.0% in the feature-based group. The error pattern (see Table 1) largely followed the RTs and we did not suspect any speed–accuracy tradeoffs.

The RTs were entered in several analyses of variance (ANOVA) with task type (present/absent, compound, and compound/absent) and intertrial relationship (same dimension/feature, different dimension/feature) as within-subject factors, and transition type (dimension-based vs. feature-based) as a between-subject factor. Separate analyses were conducted for the comparison of present and absent trials, since the latter type of trial only occurred in the present/absent and compound/absent tasks.

Absent trials in the present/absent task were not significantly slower than present trials (for both transition types, either feature or dimension based), 507 vs. 502 ms, $F < 1$. In the compound/absent task, however, absent trials were

TABLE 1
Error percentages for Experiment 1

	Task type		
	Present/absent	*Compound*	*Compound/absent*
Dimension-based group			
Absent	2.8		0.4
Feature change	4.0	4.3	8.3
Dimension change	9.4	5.1	8.7
Feature-based group			
Absent	2.8		0.4
Same target	2.3	4.7	4.7
Feature change	3.6	4.0	4.4

faster than present trials, 563 vs. 725 ms, $F(1, 42) = 360.49$, $MSE = 2553.86$, $p < .001$. This is a trivial effect, because on present trials of this task, participants had to make the additional arrowhead judgement. However, absent trials in the compound/absent task were significantly slower than absent trials in the present/absent task, 563 vs. 507 ms, $F(1, 42) = 71.74$, $MSE = 1510.11$, $p < .001$. The remainder of the analyses focuses on present trials, since these are more relevant for the intertrial effects.

Figure 2 shows the costs associated with dimension changes (in the dimension-based block, relative to feature changes) or feature changes (in the feature-based block, relative to same targets) for the different tasks. There was a significant main effect of task type, $F(2, 84) = 444.67$, $MSE = 2464.93$, $p < .001$. RTs were longer in both compound tasks than in the present/absent task. The effect of intertrial relationship was also significant, $F(1, 42) = 12.73$, $MSE = 573.15$, $p = .001$, as RTs were faster when the target dimension (in the dimension-based block) or feature (in the feature-based block) was repeated. Importantly, the intertrial relationship effect interacted with task, $F(2, 84) = 4.54$, $MSE = 542.92$, $p < .02$. In the present/absent task there were strong effects of target repetition, $F(1, 42) = 20.42$, $MSE = 545.98$, $p < .001$, whereas there were no significant effects in either of the compound tasks ($F < 1$, $p > .5$ for the standard compound task; $F < 1.2$, $p > .25$ for the compound/absent task). There were no interactions with transition type, although in general the effects were less pronounced in the feature-based transition group. In any case, we performed the same analyses for each group separately and all reported results hold for both groups.

We leave the analysis of response repetition and its potential interaction with target repetition until after Experiment 5, when we have gained sufficient power across studies to assess these usually somewhat feeble effects (Hillstrom, 2000; Huang et al., 2004),

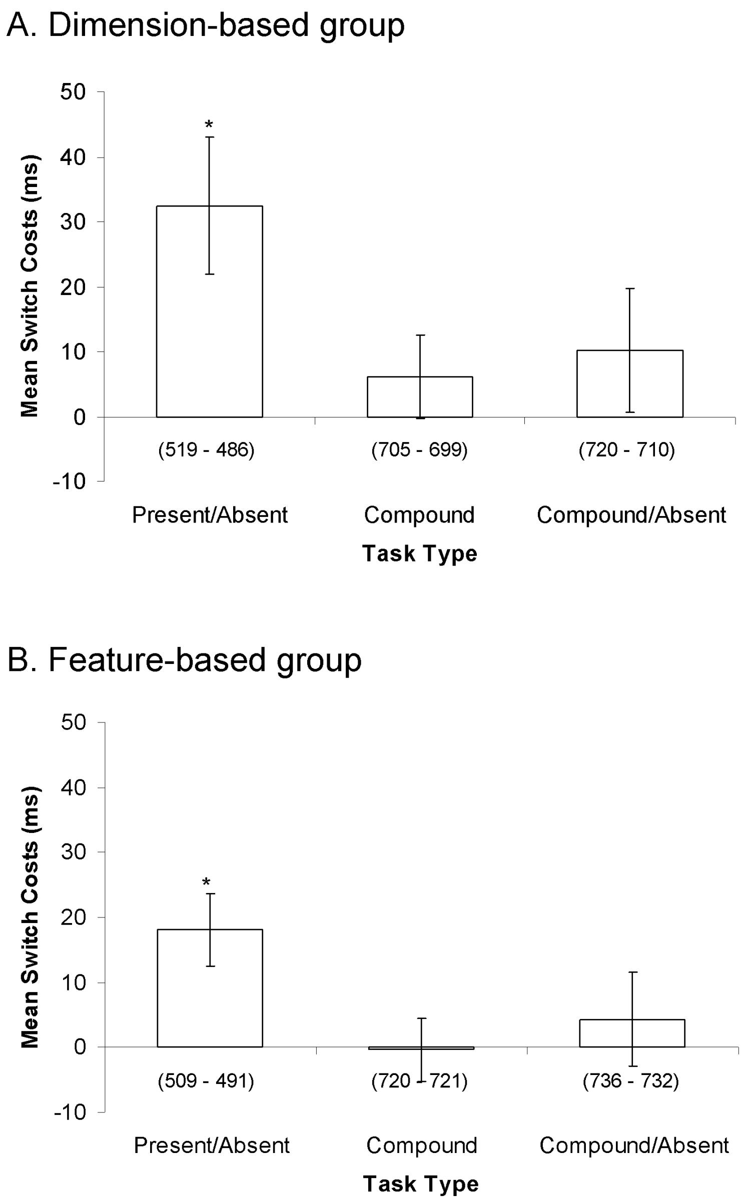

Figure 2. Mean switch costs for dimension changes relative to feature changes (dimension-based condition, Panel A), and for feature changes relative to same target trials (feature-based condition, Panel B) in Experiment 1, as a function of task type. Below each bar the absolute RTs underlying the subtraction are plotted. Error bars represent one between-subject standard error of the mean switch costs. Asterisks denote a significant difference from zero.

The findings again point towards a dissociation between the present/absent task and the compound task with regard to intertrial priming effects. The current demonstration is particularly strong because the manipulations involved exactly the same observers and exactly the same stimuli—the only thing that changed was the task. This clearly points towards a role of task demands in intertrial priming. An additional finding of importance was that this dissociation was not due to the inclusion of absent trials per se. When a compound task was combined with absent trials, intertrial effects did not reemerge. We believe this is because even though the insertion of absent trials may have contributed to increased target salience, it did not alter the response decision on present trials.

The results also do not support the idea that intertrial effects in compound search depend on whether target changes are feature or dimension based. Although overall effects were somewhat weaker in the feature-based group, the pattern of results was exactly the same as in the dimension-based group. We conclude that the feature/dimension distinction contributes little to the differential priming effects in compound search. This result is important because it goes against some existing theories that state that intertrial priming is dimension-specific (Cohen & Magen, 1999; Müller et al., 1995).

EXPERIMENT 2: INTERTRIAL PRIMING ALSO OCCURS IN A GO/NO-GO TASK

Experiment 1 established the dissociation between the present/absent and compound tasks. As suggested in the introduction, on a task level the present/absent task is more ambiguous than the compound task because the response decision is based on somewhat uncertain perceptual evidence (i.e., the possible presence of a target signal). In a compound task, by definition, the response decision is only being made when the target has already been found, in other words, when the target evidence has already solidified. It goes without saying that, in a present/absent task, ‘‘absent’’ trials do not directly contribute to the priming itself, since there is no target present and thus no feature or dimension to be primed. According to the ambiguity resolution hypothesis, ‘‘absent’’ trials in the present/absent task only contribute to the ambiguity of the task and therefore cause priming to be functional on ‘‘present’’ trials. This means that an overt ‘‘absent’’ response should not even be necessary in order to obtain priming on ‘‘present’’ trials, as long as the ambiguity is preserved. To test this prediction, in the present experiment we compared the present/absent task to a go/no-go task in which participants responded as fast as possible to the presence of a target, but withheld their response when a target was absent. Like in the present/absent task, in the go/no-go task rapid ‘‘present’’ response decisions are based on uncertain evidence and the observer is likely to set a criterion for when to ‘‘pull the trigger’’. This criterion will be met sooner when evidence accumulates across trials, as is the case when the target identity is repeated. We therefore predicted similar intertrial priming effects in the go/no-go task as in the present/absent task.

Method

Participants. Sixteen observers participated (eight men, eight women, mean age 21), all having self-reported normal eyesight and colour vision.

Stimuli, design, and procedure. The stimuli were identical to Experiment 1 (including the brackets inside the bars which now served no purpose). However, there were a number of changes to the design and procedure: Target dimension changes were directly compared to target feature changes, in other words, different dimension trials were compared to same dimension trials (but within which the feature changed). The task was a present/absent task equal to the one used in Experiment 1 in one condition, and a go/no-go task in another, which was equal to the present/absent task except that the response requirements changed. Participants pressed the ‘‘>’’ key with the right hand in response to present trials in both tasks, and the spacebar with the left hand in response to absent trials only in the present/absent task. On no-go trials, the trial ended after 2000 ms.

Results

Trials longer than 1500 ms, less than 1% of the total, were not analysed. On average, observers made 5.2% errors, which were excluded from our RT analyses. On the present trials of the go/no-go task, no errors were made after dimension changes, 0.81% errors were made after feature changes. No errors were made on absent trials. On the present trials of the present/absent task, 6.4% errors were made after dimension changes, 4.5% after feature changes. On absent trials, on average 9.5% errors were made. This pattern largely followed the RTs and was not analysed further.

In the present/absent task, responses to absent trials were slower than to present trials, 535 ms vs. 510 ms, $F(1, 15) = 6.65$, $MSE = 1526.73$, $p < .05$. Because dimension changes only apply when the target is present, and moreover no response was given on absent trials in the go/no-go task, analyses of intertrial effects were only done on present trials following present trials. Figure 3 shows the RT costs associated with target dimension changes relative to target feature changes. An ANOVA with task (present/absent, go/no-go) and intertrial relationship (dimension change, dimension repeat) revealed significant main effects of task, $F(1, 15) = 5.04$, $MSE = 1648.87$, $p < .05$, and intertrial relationship, $F(1, 15) = 31.33$, $MSE = 5681.39$, $p < .001$, with no interaction, $F < 1$. RTs in the go/no-go task were faster than in the present/absent task (487 ms vs. 510 ms), and RTs were faster when the target dimension repeated than when it changed (a benefit of 19 ms in each of the tasks).

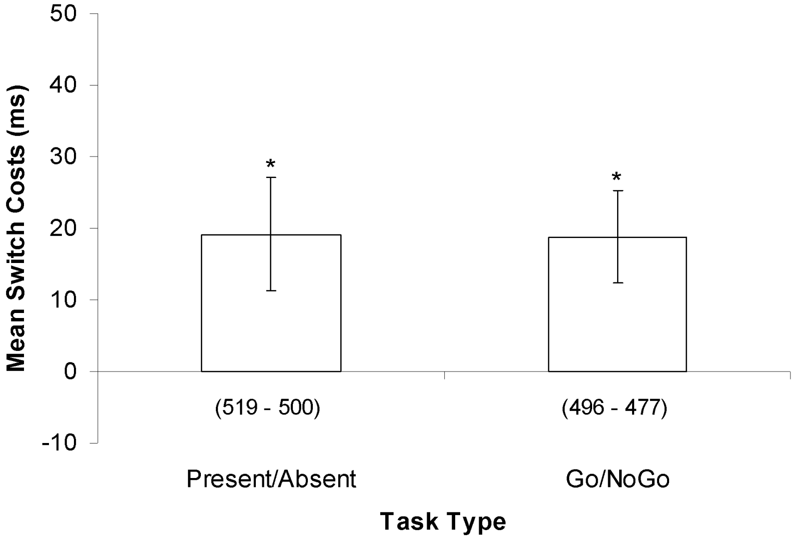

Figure 3. Mean switch costs for dimension changes relative to feature changes in Experiment 2, as a function of task type. Below each bar the absolute RTs underlying the subtraction are plotted. Error bars represent one between-subject standard error of the mean switch costs. Asterisks denote a significant difference from zero.

We found intertrial priming effects in a present/absent task as well as a go/no-go task. The fact that priming in the go/no-go task is virtually as strong as in the present/absent task indicates that an actual overt ''absent'' response is not necessary. Merely the presence of target absent displays, plus having to respond ''present'' when the target is there, is sufficient to yield intertrial priming. We believe this is because in the go/no-go task the perceptual evidence is just as ambiguously related to the response as in the present/absent task.

EXPERIMENT 3: CONTROLLING FOR SPEED DIFFERENCES

Another difference between the compound search task and the present/absent task is that given the same stimuli, the latter task usually generates much faster responses simply because it does not involve the additional task of having to determine the response feature (the same goes for the go/no-go task). This raises the possibility that intertrial priming effects are actually latent in compound search, but that they drown in the long RTs caused by the additional task. To investigate this possibility we changed the nature of the compound task. Again the target was always present, but instead of determining the direction of an arrowhead placed inside it, observers now determined its location by indicating the display quadrant in which it appeared. Note that although it is not a typical example, the localization task is still a compound task. Observers need to detect the target on the basis of one feature (in our case colour or orientation), but respond on the basis of another (namely its location). However, because location information is more readily available (Cave & Pashler, 1995; Nissen, 1985; Theeuwes, 1989; Treisman & Gelade, 1980; Tsal & Lavie, 1988; von Wright, 1968), and because it allows for more natural stimulus–response mappings than an arbitrary symbol, responses will be faster. If intertrial effects depend on such fast responses we may now see those effects emerge. As in Experiment 1, we also varied the type of target change: It could either be feature based or dimension based.

Method

Participants. Twelve new observers participated (nine men, three women, mean age 24), all having self-reported normal eyesight and colour vision.

Stimulus, design, and procedure. Methods were identical to the dimension-based compound condition of Experiment 1 except for the following changes. Trials were organized in twelve blocks of 84 trials, preceded by 32 practice trials. The first four trials of each block were warm-up trials and were excluded from analyses. In three *feature-based* blocks, only colour targets were presented, and in another three, only orientation targets. Within these blocks, half the targets were of one feature value (e.g., red in a colour block, or left-tilted in an orientation block), the other half of the other feature value (green or right-tilted respectively), randomly mixed. In the remaining six *dimension-based* blocks, colour and orientation trials were randomly intermixed, with the constraint that a quarter of intertrial relations were feature repetitions, a quarter were feature switches, and half were dimension switches (i.e., from a colour target to an orientation target and vice versa). Block order was randomized with the constraint that each feature-based block type occurred at least once, and dimension-based blocks at least twice every four blocks. Instead of determining the direction of the arrowhead inside the target, the participants now determined the quadrant in which the target appeared (top left, top right, bottom left, or bottom right), by pressing either one of the ''A'', ''Z'', ''L'', and ''<'' keys on a standard keyboard. These keys were chosen such that they map easily onto the different quadrants. The participant used the index and middle fingers of each hand.

Results and discussion

Trials longer than 1000 ms, less than 1% of the total, were not analysed. On average, observers made 3.7% errors, which were excluded from our RT analyses. Error rates did not vary much between different conditions (see Table

TABLE 2
Error percentages for Experiment 3

Intertrial relationship	*Feature-based condition*	*Dimension-based condition*
Same	3.7	3.9
Feature change	4.0	3.8
Dimension change		3.0

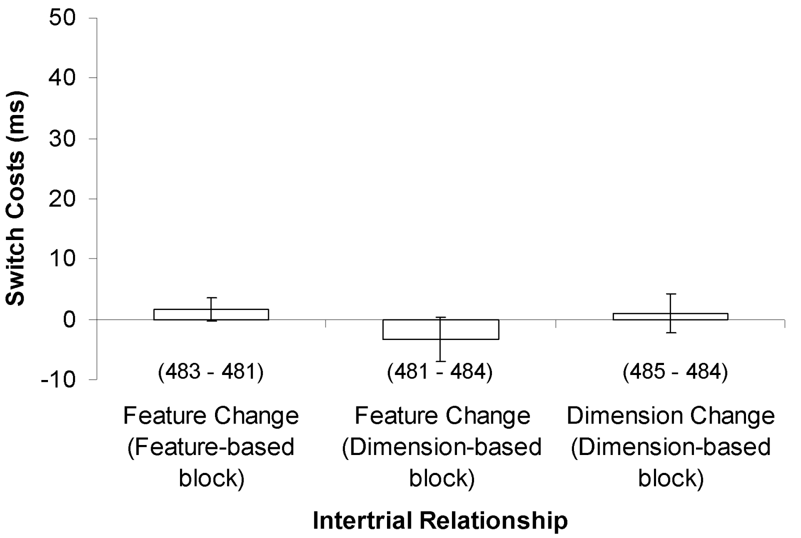

Figure 4. Mean switch costs for different intertrial changes in the feature-based and dimension-based conditions of Experiment 3, relative to when the target remained the same. Below each bar the absolute RTs underlying the subtraction are plotted. Error bars represent one between-subject standard error of the mean switch costs.

2), and were not analysed further. Figure 4 shows the mean RT costs for the different intertrial changes relative to same target trials. As can be seen, the intertrial relationships had no effect on RTs, as all averages remained within 4 ms of each other, $F < 1$, n.s.

These results do not support the idea that intertrial priming effects drown in the longer RTs of the typical compound task. In the present localization task (which we regard as a special case of the compound task) RTs were reduced by a third relative to the standard compound task of Experiment 1, and brought down to the level of the present/absent tasks of Experiments 1 and 2. Yet, inter-trial effects were absent. This excludes overall RT as an explanation of the dissociation found between these tasks.

EXPERIMENT 4: PRIMING RETURNS WHEN THE AMBIGUITY IS INCREASED IN THE COMPOUND TASK

Experiments 1–3 have indicated when intertrial effects do not occur in compound tasks. The purpose of Experiments 4 and 5 was to test conditions under which intertrial effects are expected to emerge. The ambiguity resolution hypothesis states that intertrial priming should occur when the situation becomes more ambiguous. In Experiment 4 we increased the ambiguity by combining compound trials with trials in which a ''foil'' appeared. We compared this *compound/foil* condition to the compound/absent condition we introduced in the previous experiment. On foil trials there was no target present, and the observer had to response ''absent''. However, on these trials one of the nontargets was replaced with a shape singleton. We assumed that the foil would often capture attention, because it was a singleton just like the colour- and orientation-defined targets. Because moreover the foil contained an (irrelevant) arrowhead, it would likely contribute to task confusion as to which response to press. That is, in contrast to normal absent trials, the presence of foil trials are more likely to interfere with the response decisions on present trials, because for the correct decision to be made on present trials it is paramount that the unique item is a target and not a foil. To resolve this ambiguity, the specific target dimension (e.g., colour or orientation) now becomes instrumental, as it serves to distinguish the target from the foil. In other words, to avoid confusion it is helpful to emphasize the specific defining target property. This then leads to priming of the same property on the next trial. We therefore predicted intertrial priming to be stronger in the compound/foil task than in the compound/absent task.

Method

Participants. Twenty-four observers participated (18 men, 6 women, mean age 22), all having self-reported normal eyesight and colour vision.

Stimuli, design, and procedure. The method was the same as in previous experiments except for the following changes. There were two task types. The compound/absent task was identical to that of the dimension-based group of Experiment 1. The compound/foil task was the same as the compound/absent task, except that now on absent trials one of the grey background nontargets was replaced with a foil. This foil was a grey circular shape singleton with a radius of 1.1 degrees visual angle. The required response to these foil displays was nevertheless ''absent''. Note also that it did not share any defining features with any of the targets. Each task was presented twice in counterbalanced order. Each block contained 120 trials of which half were target absent trials. The other half consisted of equal proportions of colour- and orientation-defined targets. All transitions were dimension based, that is, to assess intertrial effects dimension changes were compared to feature changes.

Results

Trials longer than 1500 ms, less than 1% of the total, were not analysed. On average, observers made 4.2% errors. On present trials of the compound/absent task, participants made 7.2% errors after a feature change, 5.2% after a dimension change. On absent trials 0.5% errors were made. On present trials of the compound/foil task, participants made 8.7% errors after a feature change, 8.3% after a dimension change. On foil trials, 1.4% errors were made. This pattern does not go against that of the RTs and it was not analysed further.

Trivially, absent trials were again faster than present trials, 583 vs. 734 ms, $F(1, 23) = 131.12$, $MSE = 8430.38$, $p < .001$, because the latter involved the additional requirement of determining the direction of the arrowhead. More relevant is that absent trials in the compound/foil condition were slower than in the compound/absent condition, 603 vs. 562 ms, $F(1, 23) = 10.46$, $MSE = 3903.35$, $p < .01$. This indicates that the foil indeed captured attention on a substantial number of trials.

The RT costs for dimension changes (relative to feature changes) on present trials are shown in Figure 5. An ANOVA with task type (compound/

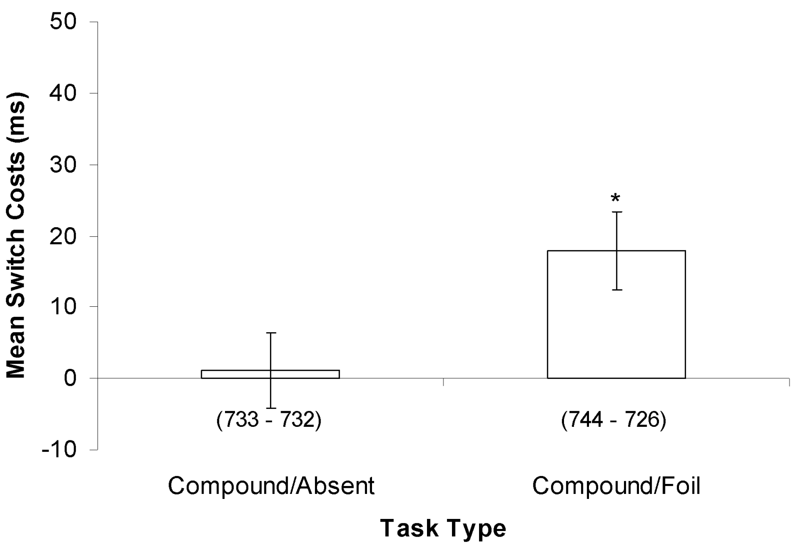

Figure 5. Mean switch costs for dimension changes relative to feature changes in Experiment 4, as a function of task type. Below each bar the absolute RTs underlying the subtraction are plotted. Error bars represent one between-subject standard error of the mean switch costs. Asterisks denote a significant difference from zero.

absent, compound/foil) and intertrial relationship (same dimension, different dimension) as factors revealed a significant main effect of intertrial relationship, $F(1, 23) = 6.51$, $MSE = 332.80$, $p < .02$. Importantly, this effect interacted with task, $F(1, 23) = 4.77$, $MSE = 359.63$, $p < .05$. As can be seen in Figure 5, there was no priming effect in the compound/absent task, 1 ms benefit for dimension repetitions, $t < 1$, n.s., whereas there was a priming effect for the compound/foil task, 18 ms benefit for dimension repetitions, $t(23) = 3.31$, $p < .01$.

In support of the ambiguity resolution hypothesis, the results show that when task ambiguity is increased, intertrial priming effects are increased too. Note that in the compound/foil condition, the target present trials (i.e., the trials that allow for priming) were exactly the same as in the compound/absent condition. The only thing that differed was the stimulus on absent trials. We propose that the presence of the foil on absent trials changed the nature of the task on present trials, as the particular target dimension now became crucial for the response decision process.

EXPERIMENT 5: AMBIGUITY THROUGH TASK ALTERNATION ALSO RESULTS IN INCREASED PRIMING

One objection to Experiment 4 may be that although we sought to alter the ambiguity of the task, we actually did this by altering the stimulus—be it only on absent trials. Experiment 5 was designed to manipulate ambiguity without manipulating the stimulus. In this experiment the search displays were again like those used in the experiments presented so far (without foils). The crucial manipulation was a continuous alternation of the task performed on those stimuli: Two consecutive present/absent trials were always followed by two consecutive compound trials and vice versa. We assumed that the requirement of a task switch every two trials would lead to increased ambiguity as to which task to perform, and we hypothesized that target repetition would now become beneficial in resolving this ambiguity. For instance, imagine that on a particular trial the participant is required to switch from the present/absent task to the compound task. A target is present, but he or she is likely to be somewhat uncertain as to which task to perform, but eventually performs the correct one. Now, if the next target is then the same, the decision becomes easier: Same stimulus, same task. If the next target is different, the confusion may return: Are we dealing with a different task here or just with a different stimulus? The prediction is then that we should find intertrial effects within the individual tasks (present/absent and compound), but not between them. On the contrary, target repetition may only add to the confusion when simultaneously the task has switched.

Method

Participants. The data of 26 observers (12 men, 14 women, mean age 22) were included in the present experiment. Six more participants were run, but they made too many errors as a result of the task switching involved. Since this led to too few useful trials in at least one of the data cells (i.e., fewer than 10), these participants were excluded.

Stimuli, design, and procedure. The method was the same as in previous experiments except for the following changes. There were two task types, a present/absent task and a standard compound task, both identical to the dimension-based condition of Experiment 1. The major change was that these two tasks were not blocked but alternated every two trials. Thus two present/absent trials were followed by two compound trials, and so on. Observers responded with the ‘‘z’’ and ‘‘x’’ keys to ‘‘absent’’ and ‘‘present’’ trials respectively, and with the ‘‘<’’ and ‘‘>’’ keys to the direction of the arrowhead on compound trials. These tasks were first practiced separately for 34 trials each, and then practiced in alternation for another 34 trials. The real experiment contained four alternation blocks of 124 trials each (including 4 warm-up trials). Half the trials were present/absent trials, the other half compound trials. On half the present trials the target dimension changed between colour and orientation, on the other half it remained the same (but the feature changed).

Results

Because overall task difficulty had increased (due to the task switching requirements), the RT cutoff limit was increased to 3000 ms. Less than 2% of the total number of trials were not analysed. On average, observers made 6.6% errors. The pattern of errors (see Table 3) reveals that more errors were made

TABLE 3
Error percentages for Experiment 5

	Current task			
	Present/absent		*Compound*	
Intertrial relationship	*Present/absent previous task*	*Compound previous task*	*Present/absent previous task*	*Compound previous task*
Absent	0.85	1.1		
Feature change	3.9	14.2	3.2	2.7
Dimension change	7.1	14.1	4.3	3.0

after a task switch, especially when the present/absent task followed the compound task. Overall the errors follow the RTs and were not analysed further.

In the present/absent task, absent trials resulted in faster RTs than present trials (770 ms vs. 906 ms), $F(1, 25) = 14.87$, $MSE = 64{,}761.70$, $p = .001$. This is probably because on absent trials, the task requirements are less ambiguous, since only the present/absent task contained absent trials. Absent trials after a present/absent task were faster than absent trials after a compound task, 747 vs. 793 ms, $F(1, 25) = 6.36$, $MSE = 8626.97$, $p < .02$, the latter of which involved a task switch.

The RT costs associated with dimension changes on present trials are shown in Figure 6, as a function of the task on the current trial as well as on the previous trial. An ANOVA was performed on these trials with current task type (present/absent, compound), intertrial task relationship (same task, different task), and intertrial dimension relationship (same dimension target, different dimension target) as factors. Overall, RTs did not differ significantly between the present/absent and compound tasks, $F < 2$, n.s. Task changes, however,

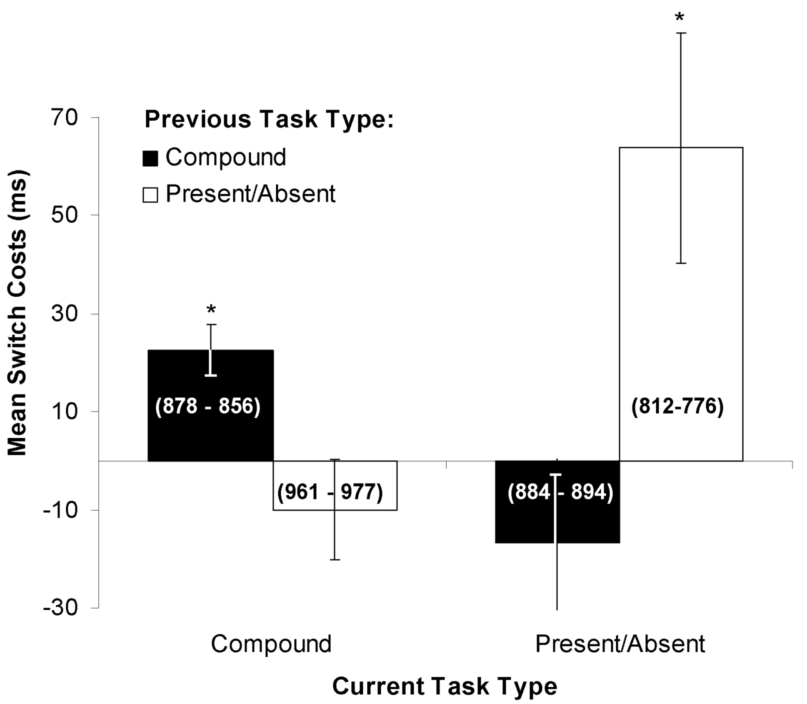

Figure 6. Mean switch costs for dimension changes relative to feature changes in Experiment 5, as a function of the task on the current trial as well as the type of task on the previous trial. Inside each bar the absolute RTs underlying the subtraction are plotted. Error bars represent one between-subject standard error of the mean switch costs. Asterisks denote a significant difference from zero.

resulted in increased RTs relative to task repetitions, $F(1, 25) = 27.51$, MSE = 10,282.21, $p < .001$. Task switches had a greater effect on present/absent trials than on compound trials, as indicated by a Current task × Intertrial task relationship interaction, $F(1, 25) = 18.99$, $MSE = 7214.22$, $p < .001$. The only other significant effect was an interaction between intertrial task relationship and intertrial dimension relationship, $F(1, 25) = 6.66$, $MSE = 6182.86$, $p < .02$. As can be seen from Figure 6, dimension repetitions resulted in benefits only between trials of the same task. For the compound task this benefit was 22 ms, $t(25) = 4.12$, $p < .001$. For the present/absent task it was 64 ms, $t(25) = 2.72$, $p <$ .02. In contrast, when the task changed, if anything, dimension repetitions resulted in costs (of −10 and −17 ms for the compound and present/absent tasks respectively; however, these costs were not significant; both $t < 1$).

The results show that increasing ambiguity on a task level (while keeping the stimuli exactly the same) results in increased intertrial priming effects. We suggest that target repetitions help the observer in coping with the ambiguity imposed by the task switch, through priming of the relevant task set (provided that the target repetition occurs within the same task).

A point of interest is the relative difficulty observers had with a switch from the compound task to the present/absent task compared to the reverse scenario. A likely explanation in the present context is one in terms of congruency effects. A closer look at the errors suggested that participants, when switching from the compound to the present/absent task, often responded with the finger corresponding to the direction of the bracket inside the target, either with the correct or incorrect hand (46% and 37% of all errors, respectively). The RTs followed a similar pattern. Thus it seems that participants had particular trouble in ignoring the bracket even when it was irrelevant to the task. Note that this did not just reflect an overall failure to comply with the instruction to switch tasks, because switching from present/absent to compound tasks affected performance much less and on a majority of error trials the hand was correctly switched.

Response analysis across experiments. So far the experiments have shown that intertrial priming of target-defining features is typically absent or diminished in compound search tasks, but that it may be invoked by creating task ambiguity. A question that we have left open so far is whether any direct priming occurs between *responses* within the compound task, as has been found by Huang et al. (2004). Are RTs perhaps faster when the response feature (in our case the direction of the arrow) rather than the target-defining feature is repeated? And, does response repetition perhaps interact with repetition of the target-defining feature? Such an interaction would be important as it would provide support for the episodic retrieval account of intertrial priming, which states that repetition effects result from the retrieval of a previous stimulus–response trace. The episodic retrieval account also predicts that when the target-defining feature repeats, but the response switches, RTs should be especially

Figure 7. Interaction between response and target identity relationships between trials, across all experiments featuring a compound task (except Experiment 3; $N = 94$).

slow, because then the wrong response is being retrieved. To gain maximum power for such an analysis, we pooled the data from all compound search conditions of the present experiments ($N = 94$), except the localization task of Experiment 3. Trials were classified according to whether the target feature was the same or different across trials (regardless of whether target changes were feature or dimension based), and according to whether the response feature was the same or different. The results are shown in Figure 7. Overall, responses were faster when the target feature repeated, $F(1, 90) = 8.69$, $MSE = 582.29$, $p < .01$. There was also a trend towards faster responses when the response feature repeated, $F(1, 90) = 2.82$, $MSE = 1653.16$, $p = .096$. However, there was no Target feature $\times$ Response feature interaction, $F(1, 90) = 1.13$, $p = .29$. None of these effects interacted with the specific type of experiment. The same analyses were also applied to Experiment 3 (the localization task), but there were no differences whatsoever, all Fs < 1. Thus, it appears that in the compound tasks of our experiments, observers treated the target identity and the response identity as largely independent.

GENERAL DISCUSSION

The present study further explored the dissociation between present/absent and compound search tasks with regard to the phenomenon of intertrial priming. The experiments have shown that this dissociation occurs regardless of whether target changes are feature or dimension based (Experiments 1 and 3) and regardless of whether responses are fast or slow (Experiments 1, 2, and 3). Furthermore, an overt ‘‘absent’’ response is not necessary to obtain intertrial effects (Experiment 3), nor is the insertion of ‘‘absent’’ trials in a compound task sufficient to generate such effects (Experiment 1). Intertrial effects did emerge in a compound task when task ambiguity was increased. In Experiment 4 the insertion of foils (a singleton that could be mistaken for a target) in target absent displays resulted in priming effects for target present displays. In Experiment 5, the stimuli remained identical but task conflict was introduced by alternating the present/absent and compound task every two trials. This again resulted in intertrial effects in the compound task (as well as in the present/absent task).

The ambiguity resolution hypothesis

The results are consistent with the ambiguity resolution hypothesis. This hypothesis states that intertrial priming becomes functional (and therefore measurable) only in situations of ambiguity, uncertainty, or conflict. When it is unclear what the target is (i.e., stimulus ambiguity), or what the required response is on a selected target (or set of responses; task ambiguity), repetition of the target becomes beneficial, as evidence for a certain response (or a set of responses, i.e., the task set) accumulates. We hypothesized that the present/absent task is more ambiguous than the compound task, because the response decision is based on uncertain perceptual evidence. In contrast, the compound task can only be fulfilled when the perceptual evidence has been disambiguated, with no direct relationship between the response decision and target presence. By making this relationship more ambiguous, we succeeded in making intertrial effects reemerge in a compound task.

The ambiguity hypothesis explains why some studies found intertrial effects in compound search whereas others did not. What characterizes the studies that did report intertrial effects is a certain degree of ambiguity in the stimulus. For instance, in Olivers and Humphreys' (2003) study, the target was not always the only item in the display carrying a unique feature: On a considerable proportion of trials there was a so-called singleton distractor present that interfered with search as indicated by longer RTs. Similarly, Pinto et al. (in press) found strong intertrial priming effects when the search displays contained a singleton distractor, whereas priming was much reduced or even absent when there was no such distractor. Neither was the target identity straightforward in Hillstrom's (2000) crucial experiments. In one experiment she used conjunction search displays in which the target could only be distinguished as a combination of features shared with different distractor sets. In another experiment the target was accompanied by a singleton distractor. Also Maljkovic and Nakayama's (1994) original displays were relatively ambiguous. The target was unique, but relative to only two distractors: It was either a red diamond between two green diamonds or, vice versa, a green diamond between two red diamonds. To be certain whether the selected item is a target, the observer therefore needs to compare its features to those of the distractors. This too is likely to have induced a degree of ambiguity as to which of the three items was the target, especially on trials when the target and distractor colours switched (cf. Bravo & Nakayama, 1992). In contrast, studies that failed to find intertrial effects in compound search have typically used relatively unambiguous, homogenous, large set size displays (Kumada, 2001; see also present experiments). Recently we have systematically investigated how stimulus ambiguity contributes to intertrial priming (Meeter & Olivers, in press). In a compound search task identical to that used by Maljkovic and Nakayama (1994), intertrial priming decreased when the number of nontargets was increased from three to twelve, but it increased when a singleton distractor was added to the display. The present study extends these findings by showing that ambiguity on the task level can contribute even when the stimulus remains identical.

An exception appears to be Experiment 3 of Müller et al. (2004), which also directly compared the present/absent task to a compound task, but found equally strong intertrial effects for both. As in our experiments, the target was either colour or orientation defined. However, a crucial difference was that in our experiments the response was based on an independent feature presented inside the target (i.e., the arrowhead), whereas in Müller et al.'s task the response was based on the target type itself. That is, participants pressed one button when it was an orientation target, another button when it was a colour target. Thus, the exact target identity was instrumental to the task, allowing for functional priming relationships.

Further support for the ambiguity resolution account comes from an fMRI study by Pollmann, Weidner, Müller, and von Cramon (2000). They found that dimension changes (between colour and orientation) lead to increased activity in a frontoposterior network (involving the fusiform gyrus, lateral occipital gyrus, superior temporal sulcus, middle temporal gyrus, superior parietal lobule, and precuneus). This network has typically been associated with attention tasks, suggesting that dimension changes require additional attention. In addition, Pollmann et al. found strong activation in the left frontopolar cortex. Interestingly, the same frontopolar area has been associated with the presence of task ambiguity such as after a covert rule change in the Wisconsin Card Sorting Test (e.g., Nagahama et al., 2001), and Pollman (2004) has therefore suggested that this area may be involved in implicitly inhibiting the previous dimension after a target switch, when there are no explicit rules available indicating to the observer when to change. Thus, the frontal pole would serve to resolve the ambiguity caused by dimension uncertainty. This corresponds directly to the ambiguity resolution account proposed here.

Relation to other accounts

Several other accounts of intertrial priming effects in visual search exist. These differ mostly in how they deal with two issues, namely the level of processing at which priming is beneficial, and the kind of representations that underlie priming.

With respect to the first issue, several answers have been suggested. What we refer to as *visual selection* views suggest that intertrial priming speeds up attentional selection. Maljkovic and Nakayama (2000, p. 592) propose an ''implicit yet primitive system [that] can facilitate the return to recently attended targets and locations'', and ''ensure that objects of recent interest would be repeatedly sampled''. According to Müller and colleagues (Found & Müller, 1996; Müller et al., 1995, 2004) target detection is based on the sum of dimension-specific activation integrated in a single perceptual saliency map. This dimension-specific activation is weighted on the basis of previous target information, resulting in a necessary but time-consuming weight-shift when the target dimension is switched. A similar account has been proposed by Wolfe, Butcher, Lee, and Hyle (2003).

Response selection views, on the other hand, place benefits at the stage at which a response is selected to match the stimulus. An example is the response selection model of Cohen and Shoup (1997), according to which each visual dimension carries its own response selection modules. Repetition of a dimension therefore leads to priming of the response system within that dimension. Consistent with this, Cohen and Magen (1999) failed to observe overall costs in a cross-dimensional search task as compared to a within-dimensional search task when using a variation on the compound search task. Although Cohen and Magen did not report on intertrial effects, they did suggest that overall costs or benefits, when present, are the result of priming (p. 305). However, one methodological aspect of Cohen and Magen's study makes direct comparison with other compound tasks difficult: Their participants were required to report the target's *dimension* in the cross-dimension task, but its *feature* in the within-dimension task. The more finely required discrimination in the latter task may have slowed responses, thus potentially compensating for any priming-related benefits (Kumada, 2001; Müller et al., 2004). Another example is the account of Kumada (2001), who argued that in a present/absent task, the presence of certain activity within a dimension is exclusively coupled to a single response (namely ''present''), allowing for intertrial priming of that same response when the dimension is repeated. In a compound task, on the other hand, a repeated target does not mean that the response is necessarily repeated. A target can thus not consistently prime the correct response, which explains why no priming effects were evident. A third view on the issue is that intertrial priming influences an explicit decision stage in which it is checked whether the selected stimulus is really the target (Huang et al., 2004).

Our account does not directly contradict either of these alternatives, as we have argued that ambiguity may occur at different stages. If the visual display is ambiguous as to what the target is, intertrial priming will occur in the visual selection stage (Meeter & Olivers, in press). Ambiguity may also occur in the stimulus–response mapping, in which case intertrial priming will be observed in the response selection stage. We have argued that in a present/absent task the presence of a target provides direct evidence for a particular response. But, crucially, this evidence is somewhat uncertain, making it useful to enhance or accumulate evidence across trials. In compound tasks the target itself does not provide any evidence about the response. Moreover, by the time the response is generated the uncertainty about the stimulus has dissipated, since the compound task can only be done when the target is known. However, when the uncertainty is brought back in, as was the case in Experiments 4 and 5, the specific target dimension (or feature) information becomes useful again.

With respect to the issue of what representations priming operates on, it has traditionally been thought that it is the repeated activation of the target-defining feature or dimension which drives intertrial priming (Cohen & Magen, 1999; Maljkovic & Nakayama, 1994; Müller et al., 1995). Recently, however, several authors have argued that it is not a single feature or dimension that matters, but repetition of the target stimulus in its entirety (Hillstrom, 2000; Huang et al., 2004; Logan, 1990; Neill, 1997). For example, Huang et al. (2004) found that repeating a target feature could either harm or speed up performance, depending on whether features on other target dimensions were also repeated. The latter view is known as the *episodic retrieval view* because such whole-stimulus representations are said to be retrieved from episodic memory during task performance. According to some variants of the episodic retrieval view (Logan, 1990; Neill, 1997), combinations of stimulus and response are stored as an episodic memory for each trial. When on the subsequent trial the target is the same as on the previous trial, performance may benefit from the rapid, implicit, and automatic retrieval of the previous trial and its coupled response. Waszak, Hommel, and Allport (2003) found evidence that stimuli may even invoke complete task settings. According to other variants, retrieval of a stimulus leads to the retrieval not of a specific response, but of either other perceptual properties (Huang et al., 2004) or complete previous attentional settings, which then can speed up selection of matching targets (Hillstrom, 2000; Tipper, 2001). The former variants of the episodic retrieval account would be equivalent to a ''response selection'' view, the latter to a ''visual selection'' view. What these have in common is that intertrial priming attaches to representations of the whole stimulus, as opposed to only one or some of its features. Our results do not speak to this issue. What we propose here is that the level of representation at which the priming bears out will depend on the level at which the system experiences a conflict.

In sum, the ambiguity resolution hypothesis has elements in common with all the above explanations, but provides a comprehensive framework for when intertrial priming does and when it does not occur.

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