The time-course of endogenous temporal attention – Super fast voluntary allocation of attention

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ABSTRACT

It is widely accepted that voluntary spatial attention is slow – it can only affect performance with medium and long cue-target intervals. Here, we examined whether this also holds for voluntary temporal attention. We performed a rigorous examination of the time-course of attention allocation to a point in time using two common paradigms for studying endogenous temporal attention: 'constant foreperiod' and 'temporal orienting'. With both paradigms, the task required non-speeded identification of a letter, whose presentation was preceded by a warning cue. This cue was either auditory or visual, and it was either informative or uninformative. Critically, to avoid exogenous attention, the cues did not involve an intensity change. We found significantly higher identification accuracy when the cue was informative than uninformative, suggesting that temporal attention improved perceptual processing. Importantly, reliable effects of temporal attention on perceptual processing were found with as little as 150 ms from cue onset and up to 2400 ms. Hence, measuring endogenous attention in the temporal domain revealed a twofold faster mechanism than what was believed based on measurements in the spatial domain. These findings challenge the common assumption that voluntary processes are inherently slow. Instead, they portray voluntary mechanisms as considerably more flexible and dynamic, and they further underscore the importance of incorporating the temporal domain into the study of human perception.

1. Introduction

It is widely accepted that some of our cognitive processes are automatic and fast while others are voluntary and slow (see, Kahneman, 2011 for an extensive review). A similar distinction is also frequently applied to attention. Typically, this was examined using cues that indicate the target's location prior to its onset and systematic manipulation of the interval between the cue and the target. Indeed, this endeavor suggested that the voluntary allocation of spatial attention to a location – endogenous spatial attention – is relatively slow; it requires about 300 ms from cue onset to reach maximal effects. In contrast, exogenous spatial attention – the involuntary stimulus-driven attention allocation – requires only 100 ms for maximal effects (e.g., Cheal & Lyon, 1991). But are voluntary processes necessarily slow? Does the coupling of 'voluntary' and 'slow' also hold for endogenous temporal attention (i.e., when attending a point in time)? Additionally, unlike exogenous spatial attention, maximal effects of endogenous spatial attention were observed even when the intervals between the attentional cue and the task-relevant target were longer than 1000 ms (e.g.,

Posner & Cohen, 1984). What about endogenous temporal attention? Can it also affect performance after such long cue-target intervals?

Previous studies suggest that, like spatial attention, temporal attention could be allocated volitionally according to our goals but it could also be stimulus-driven (e.g., Alegria, 1975; Correa, Lupiáñez, & Tudela, 2006b; Coull & Nobre, 2008; Rohenkohl, Coull, & Nobre, 2011; Steinborn, Rolke, Bratzke, & Ulrich, 2008). Questions that are related to the 'time-course' of endogenous temporal attention were explored in the past, but no conclusive answers were given. For instance, some studies examined endogenous temporal attention using the constant-foreperiod paradigm. With this paradigm, a warning cue precedes target presentation. The interval between the cue and the target – the foreperiod – is constant within a block but varies between blocks. The typical finding is that performance is better with shorter than longer foreperiods (e.g., Müller-Gethmann, Ulrich, & Rinkenauer, 2003; Rolke & Hofmann, 2007). Presumably, the constant foreperiod allows the participants to estimate the time of target onset and volitionally allocate attention to this moment. However, predicting target onset is less accurate with long foreperiods and performance deteriorates (e.g., Bausenhart, Seibold,

Rolke, & Ulrich, 2010; Niemi & Näätänen, 1981). A couple of studies which employed this paradigm to study the time-course of temporal attention found maximal decrease in reaction time (RT) around 400 ms but then RT got longer with foreperiods longer than 800 ms (Lawrence & Klein, 2013; Müller-Gethmann et al., 2003). Another paradigm is the temporal-orienting paradigm, with which the foreperiod varies from trial to trial and a symbolic cue indicates the most likely foreperiod (e.g., Correa, Lupiáñez, Milliken, & Tudela, 2004; Coull & Nobre, 1998; Kingstone, 1992). The cue is either valid (e.g., the word "early" in a trial with a short foreperiod), or invalid (e.g., the word "late" in a trial with a short foreperiod). Performance is typically better in the valid than invalid trials, presumably because the informative cue allows the participants to allocate attention to the time of target onset. Importantly, Rohenkohl et al. (2011) provided unequivocal evidence for the voluntary nature of attention allocation in this paradigm by demonstrating that symbolic cues affect performance only when the participants are instructed to pay attention to them. Regarding the time-course of temporal attention, when Griffin, Miniussi, and Nobre (2001) used a symbolic cue that indicated a foreperiod of 300 ms, maximal effects were observed around 300 ms. Interestingly, the validity effect is considerably reduced with long foreperiods. Thus, both paradigms seem to suggest that endogenous temporal attention, like its spatial counterpart, is relatively slow (i.e., maximal effects require ~300 ms), but unlike its spatial counterpart, it may not exert its effects over long intervals.

It would be premature, however, to reach any conclusion regarding the time-course of temporal attention based on previous findings for the following (five) reasons: Starting with the proposition that endogenous temporal attention is slow: (1) Most previous studies did not employ cues that indicate shorter foreperiods than 300 ms. (2) The tasks in these studies were speeded and accordingly their main measurement was RT. It is hard to tell, therefore, whether they demonstrate attentional effects on perceptual processing or merely effects on motor preparation. This includes the studies which considered shorter foreperiods (Bertelson, 1967; Klemmer, 1956; Lawrence & Klein, 2013; Müller-Gethmann et al., 2003). (3) The cue employed by most previous studies involved an intensity change. Such intensity change might have also attracted exogenous temporal attention, which is particularly worrisome with short foreperiods, making it impossible to tell whether their effects truly reflect *voluntary* allocation of temporal attention (Lawrence & Klein, 2013). Regarding the proposition that temporal attention cannot exert its effect with long foreperiods: (4) Only a few constant-foreperiod studies included a baseline condition, in which attention is not allocated to a specific point in time (e.g., Bertelson, 1967; Klemmer, 1956; Lawrence & Klein, 2013; Müller-Gethmann et al., 2003), often this baseline was a condition in which no cue was presented. Thus, with many constant foreperiod studies the performance disadvantage observed with long foreperiods may merely reflect a smaller attentional effect rather than a genuine inability to allocate temporal attention with long foreperiods. Indeed, Müller-Gethmann et al., 2003 found faster RTs with their long foreperiods than a no-cue baseline. (5) The lack of attentional effects with long foreperiods of the temporal-orienting paradigm is not informatory due to the possibility of reorienting – when the target does not appear after the short foreperiod the participants can reorient attention in accordance with the longer duration (e. g., Coull & Nobre, 1998).

Hence, the current study was designed to gain a comprehensive view of the time-course of endogenous temporal attention (i.e., to map the effects of temporal attention as a function of foreperiod, from the minimal to the maximal foreperiod for which effects of temporal attention may be observed), while avoiding the above-mentioned obstacles. To that end, we conducted 4 experiments employing both the temporal-orienting (Experiments 1 & 3) and the constant-foreperiod (Experiments 2 & 4) paradigms, with a wide range of foreperiods (25-2400 ms). All experiments included the same non-speeded letter identification task, ensuring that any observed effects reflect modified perceptual processes rather than motor preparation. Additionally, we included a

baseline condition, and the cue did not involve an intensity change, avoiding the triggering of involuntary attention.

It is important to note that in the temporal attention literature, the typical interval of interest is the 'foreperiod' as defined above: the interval from cue offset to target onset, and we therefore also mainly refer to this interval. However, when comparing temporal and spatial attention we also refer to the interval that includes the duration of the cue – the time from cue onset to target onset (i.e., the stimulus-onset-asynchrony, SOA), because this is the main interval of interest in the spatial attention literature.

Finally, whenever relevant (see the Method section below), we employed a non-aging distribution of foreperiods (Nickerson & Burnham, 1969) to minimize the foreperiod effect – the typical performance improvement with longer foreperiods (e.g., de la Rosa, Sanabria, Capizzi, & Correa, 2012; Näätänen, 1971). The foreperiod effect was studied extensively in the past and several different accounts were offered to explain its nature (e.g., does it reflect a more intentional setting of temporal preparations or is it driven by memory traces of earlier timing experiences; see Los, Kruijne, & Meeter, 2014 for a review), and its locus within the stream of information processing (e.g., does it reflect modification of early stages of processing or only modification of late motor processes; see Bausenhart, Rolke, Hackley, & Ulrich, 2006 for a review). However, these different accounts were all formulated to explain differences in RT, while in this study the task is always non-speeded. Moreover, we employed a non-aging distribution and catch trials to avoid the foreperiod effect. Thus, our study cannot contribute to the debate regarding the mechanism underlying the RT shortening observed with longer foreperiods. Still, with our non-speeded task, any observed effect of attention will support the conclusion that temporal attention affects early perceptual processes.

2. Method

2.1. General

2.1.1. Observers

The observers were students from the University of Haifa, with normal or corrected-to-normal vision. All were naive to the purpose of the study. Participants number was 15, 12, 17 and 13 in Experiments 1–4, respectively. Sample sizes were based on previous successful employment of similar paradigms (e.g., temporal-orienting paradigm: Correa, Cappucci, Nobre, & Lupiáñez, 2010; Griffin et al., 2001; constant-foreperiod paradigm: Bausenhart, Rolke, & Ulrich, 2008; Lawrence & Klein, 2013). Additionally, to ensure sufficient power, we conducted power analysis with G*Power (Faul, Erdfelder, Lang, & Buchner, 2007), using alpha = 0.05, power = 0.95, and the effect sizes of Davranche, Nazarian, Vidal, and Coull (2011) and Lawrence and Klein (2013) for the temporal-orienting and constant-foreperiod paradigms, respectively, whose methodology resembled ours. We found that the minimum sample size required is 13 and 8, respectively. The experiments adhered to the Declaration of Helsinki and were approved by the ethics committee of the University of Haifa. All observers signed a consent form.
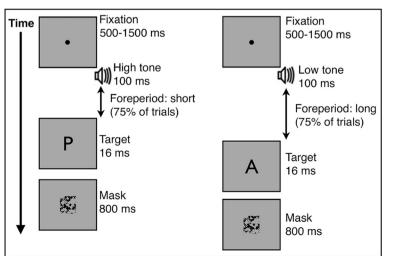
2.1.2. Stimuli, apparatus and procedure

The stimuli were presented using MATLAB and Psychophysics Toolbox (Brainard, 1997; Kleiner, Brainard, & Pelli, 2007), a 21″ monitor (resolution 1024 × 768; refresh rate 120 Hz), and a Mac-Pro computer. Visual stimuli were presented at the screen's center on a mid-gray background. The fixation mark was a black dot (0.2°). The target was a dark-gray (2.25 cd/m^2) letter (~1°) chosen randomly out of 10 possible letters: A, L, S, U, C, P, T, E, H, or M. The mask was a square (1°x1°) of dynamic random-dot noise. Auditory stimuli were presented via headphones. The target was presented for 16 ms and was followed by the mask (800 ms). The observers had to identify the target with no speed pressure.

2.2. Experiment 1 - Method

In this experiment endogenous temporal attention was invoked using symbolic auditory cues (intensity 66 dB) that preceded target presentation (Fig. 1). Three types of cues were employed: (1) A high composite tone (composed of 1500, 1600, 1700, 1800, 1900 Hz) indicated that the foreperiod will most likely be short. (2) A low composite tone (composed of 100, 200, 300, 400, 500 Hz) indicated that the foreperiod will most likely be long. The exact foreperiods indicated by these cues depended on the experimental session. Different sessions included different foreperiod distributions (Table 1). This allowed us to test a wider than usual range of foreperiods within a single experiment with a within-subject design. Additionally, dividing the experiment into several sessions allowed us to reduce the foreperiod effect by applying a non-aging foreperiod distribution in the invalid condition. Importantly, in all sessions, these cues indicated the correct foreperiod on 75% of the trials – the valid trials. On invalid trials (22%), the foreperiod that followed the cue was either shorter or longer than that indicated by the cue. The foreperiods included in a given session were chosen in an attempt to satisfy several, somewhat conflictive, constraints such as avoiding a too long experimental session, maintaining high probability for the valid foreperiods, employing a non-aging foreperiod distribution in the invalid condition, including invalid foreperiods that are shorter and longer than each valid foreperiod, as well as invalid foreperiods that match the valid foreperiods to allow a comparison between valid and invalid trials of the same foreperiod. (3) A mid composite tone (composed of 600, 700, 800, 900, 1000 Hz) was employed in a separate neutral session, and it did not bear any temporal contingencies with the target. Indeed, in this session the foreperiod duration varied randomly and was negative on some trials (i.e., the target preceded the cue). The range of foreperiods in this session was -830 ms – 2700 ms. This ensured that the neutral cue did not provide any information regarding target onset (not even that it will follow the cue) and therefore ensuring it did not afford attention allocation to a specific point in time. Critically, all 3 cue types did not include intensity change to avoid the involvement of exogenous orienting. Inspired by Lawrence and Klein (2013), this was achieved by presenting the tones amidst continuously present diotic white noise of equal intensity (66 dB).

The experiment included 4 sessions (1024 trials each) conducted on different days with counterbalanced order. Three sessions (Short, Medium, Long) included informative cues – the high/low tone cues. A trial in these sessions started with simultaneous presentation of the fixation-dot and the white noise. After a randomly varying interval (500 - 1500 ms), the noise changed briefly (100 ms) into the cue and then remained until the end of the trial. The cue was followed by a foreperiod, as detailed in Table 1. The 4th session was the neutral session in which the mid-tone cue bore no temporal contingencies with the target, but on other aspects this session was similar to the other 3 sessions. To further minimize reorienting, about 3% of the total trials were catch trials in which only the mask followed the foreperiod, and the participants had to press the space bar. The 1st session was preceded by 148 practice trials. Other sessions were preceded by 64 practice trials.

3. Experiment 1 – Results and discussion

To analyze the data, we first conducted a 2-way (session x validity) repeated-measures analysis of variance (ANOVA) on accuracy (an omnibus ANOVA that also includes foreperiod was not possible because different sessions included different foreperiods). This analysis revealed a significant main effect of validity ($F(1,14) = 11.12$, $p = 0.005$, $\eta_p^2 = 0.44$), but there was no significant main effect of session and no significant interaction ($F(2,28) = 0.57$, $p = 0.572$, $\eta_p^2 = 0.04$; $F(2,28) = 0.47$, $p = 0.630$, $\eta_p^2 = 0.03$, respectively). We therefore collapsed accuracy across sessions, and it is presented in Fig. 2 as a function of foreperiod and cue validity (valid vs. invalid).

Next, we tested the effect of foreperiod. Because the valid and invalid conditions involved different foreperiods, we had to conduct a separate 1-way (foreperiod) repeated-measures ANOVA for each of these two conditions. The two analyses revealed no significant effect of foreperiod ($F(3,42) = 0.62$, $p = 0.604$, $\eta_p^2 = 0.04$; $F(3,42) = 1.45$, $p = 0.193$, $\eta_p^2 = 0.09$, respectively). The lack of a foreperiod effect is expected, and it suggests that the employment of a non-aging distribution in the invalid trials and the inclusion of catch trials was successful in reducing this effect, as was demonstrated in the past (e.g., Correa, Lupiáñez, & Tudela, 2006b; Näätänen, 1971).

The most critical analysis, given the goal of this study, involves the validity variable (valid vs. invalid). As was already revealed by the session x validity ANOVA, we found an overall significant effect of validity ($F(1,14) = 11.12$, $p = 0.005$, $\eta_p^2 = 0.44$); as expected, accuracy was higher in the valid than invalid trials. The effect of validity was significant even when tested separately for each session (Short session: $F(1,14) = 9.84$, $p = 0.007$, $\eta_p^2 = 0.41$; Medium session: $F(1,14) = 4.65$, $p = 0.049$, $\eta_p^2 = 0.25$; Long session: $F(1,14) = 8.25$, $p = 0.012$, $\eta_p^2 = 0.37$). To further explore this validity effect, and particularly to rule out the involvement of exogenous stimulus-driven attentional processes (including increased arousal), we performed additional analyses (one-tailed paired t-tests) in which we compared directly valid and invalid trials of the same foreperiod. These comparisons are important to exclude the involvement of involuntary attentional processes because these trials were practically identical (e.g., with the 150 ms foreperiod,

Fig. 1. The sequence of events in a valid trial of Experiment 1. The attentional cues were auditory tones. A high/low tone indicated that a short/long foreperiod will follow with 75% validity. The tones were presented amidst white noise of equal intensity. The target was 1 out of 10 letters, and the task was a non-speeded letter identification. Across experimental sessions (details in Table 1), valid foreperiods were 150, 300, 600 or 1200 ms. On invalid trials the foreperiod was either shorter or longer than that indicated by the cue. Across experimental sessions, invalid foreperiods were 50, 150, 300, 600, 900, 1200, 1500 or 1800 ms.

Table 1
Foreperiod distribution in each of the informative sessions of Experiments 1 and 3. The number of trials for each foreperiod is presented in parenthesis.

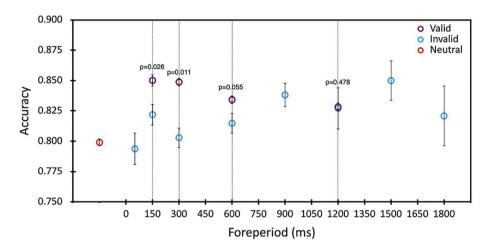|		Cueing					
Exp1	Sessions	High tone Valid 75%	High tone Invalid 21.9%	High tone Catch trials 3.1%	Low tone Valid 75%	Low tone Invalid 21.9%	Low tone Catch trials 3.1%
	Short	150 ms (384)	50 ms, 300 ms, 600 ms (64), (32), (16)	(16)	300 ms (384)	150 ms, 600 ms, 900 ms (64), (32), (16)	(16)
	Medium	300 ms (384)	150 ms, 600 ms, 900 ms (64), (32), (16)	(16)	600 ms (384)	300 ms, 900 ms, 1200 ms (64), (32), (16)	(16)
	Long	600 ms (384)	300 ms, 900 ms, 1200 ms (64), (32), (16)	(16)	1200 ms (384)	600 ms, 1500 ms, 1800 ms (64), (32), (16)	(16)
Exp3		Vertical Valid 75%	Vertical Invalid 21.9%	Vertical Catch trials 3.1%	Horizontal Valid 75%	Horizontal Invalid 21.9%	Horizontal Catch trials 3.1%
		100 ms (384)	50 ms, 300 ms, 600 ms (64), (32), (16)	(16)	300 ms (384)	100 ms, 600 ms, 900 ms (64), (32), (16)	(16)

Fig. 2. Letter identification accuracy in Experiment 1 as a function of cue-target foreperiod and cue validity. For comparison, accuracy in the neutral session is also presented. Error bars correspond to one standard error of the means (SEM) across observers. The vertical dashed lines indicate foreperiods that were common for both valid and invalid conditions. The *p*-value for each one-tailed paired *t*-test comparing valid and invalid trials of a given foreperiod is presented above that foreperiod.

regardless of validity, the target appeared 150 ms after cue offset). The only difference was whether or not the participants expected this foreperiod. If cue onset triggered exogenous attention, even with our no-intensity-change cue, and this led to improved processing of targets appearing in the temporal vicinity of the cue, then such involvement of exogenous attention should be present in both validity conditions and there should be no difference between them. Thus, if a significant valid-invalid performance difference emerges even when trials with the same foreperiod are compared, it can only be due to voluntary attention allocation to the correct point in time. Note that because the valid and invalid trials included in these analyses were procedurally identical, a reliable difference between these two types of trials will also rule out the employment of any general strategy as the source of this difference, because a general strategy should benefit both types of trials. These comparisons revealed a significant valid-invalid difference with the foreperiods of 150 ms ($t(14) = 2.134$, $p = 0.026$, $d_z = 0.55$) and 300 ms ($t(14) = 2.58$, $p = 0.011$, $d_z = 0.67$), and marginally significant difference with 600 ms ($t(14) = 1.712$, $p = 0.055$, $d_z = 0.44$). These comparisons were significant even after using the Benjamini-Hochberg procedure to control for multiple comparisons. The fact that accuracy was higher in the valid condition already with a foreperiod of 150 ms (i.e., an SOA of 250 ms) suggests that the allocation of voluntary temporal attention is at least as fast as that of voluntary spatial attention. The lack of effect with the long 1200 ms foreperiod is likely due to reorienting (i.e., when the target does not appear after the expected short foreperiod the participants reorient to attend a longer foreperiod thereby reducing

differences in attention allocation between valid and invalid trials), as is often found with the temporal orienting paradigm (e.g., Coull & Nobre, 1998; Kingstone, 1992; Miniussi, Wilding, Coull, & Nobre, 1999). The issue of reorienting is avoided in Experiment 2, in which the constant foreperiod paradigm is employed.

Finally, because in the neutral condition the foreperiod was chosen randomly (including negative foreperiods) we cannot conduct a similar analysis to compare it with the valid condition. However, given the lack of foreperiod effects in the analysis above, we collapsed accuracy across all valid trials and used a one-tailed paired *t*-test to compare the two conditions. As expected, accuracy in the valid condition was significantly higher than accuracy in the neutral condition ($t(14) = 1.976$, $p = 0.035$, $d_z = 0.51$).

4. Experiment 2

This experiment employed the constant-foreperiod paradigm to examine the same questions tested in Experiment 1. Like Experiment 1, it employed a non-speeded letter identification task, and a no-intensity-change auditory cue. However, because within a given block of this experiment the cue-target foreperiod was constant, there was only one cue type that predicted target onset with 100% validity. This afforded testing whether the findings of Experiment 1 can be replicated with a different paradigm. Moreover, because with the paradigm employed here temporal reorienting is not a concern, we could test a wider range of foreperiods (75-2400 ms), as there is no need to employ a non-aging

foreperiod distribution or catch trials. Finally, we also included a baseline condition in which the cue did not bear any temporal contingencies with the target.

4.1. Experiment 2 – Method

4.1.1. Stimuli, apparatus and procedure

These aspects were similar to Experiment 1 except for the following: The cue and the continuously present background were composed of white noise (intensity 66 dB), but unlike the background diotic noise, the cue involved two independent noise signals each presented to one ear, resulting in 0 inter-aural correlation (dichotic noise; Lawrence & Klein, 2013). The experiment included 2 sessions separated by at least 2 h rest. One session included 480 trials that were similar to the neutral trials in Experiment 1, with a break every 80 trials. The range of foreperiods in this session was -2800 ms – 2800 ms. The other session included 6 blocks of 80 trials. In each block the foreperiod was constant. The possible foreperiods were: 75, 150, 300, 600, 1200, and 2400 ms. Mask duration was 300 ms. Sessions order and blocks order were counterbalanced across participants. 148 practice trials preceded the 1st session/block, and 20 practice trials preceded each following session/block. There were no catch trials.

4.2. Experiment 2 – Results and discussion

Fig. 3 depicts accuracy as a function of the different constant-foreperiod blocks and the baseline block. As can be seen in this figure, performance was higher in all the constant-foreperiod blocks than the baseline block. To analyze this data, we first conducted a 1-way (foreperiod) repeated-measures ANOVA on accuracy in the constant-foreperiod blocks, which revealed no significant effect of foreperiod (F$(5,55) = 0.88$, $p = 0.502$, $\eta_p^2 = 0.07$). Thus, to compare performance in these blocks, in which the participants could allocate attention to the relevant point in time, to that in the baseline block in which attention could not be allocated to a specific point in time, we collapsed accuracy across the different foreperiod blocks, and used a one-tailed paired *t*-test to compare the two conditions. Indeed, accuracy was significantly higher in the constant-foreperiod than the baseline condition (t(11) = 2.734, $p = 0.010$, d$_z = 0.79$).

Two aspects of these findings are particularly notable. First, a difference between the constant-foreperiod blocks and the baseline is present even with the two longest foreperiods: 1200 ms and 2400 ms. In Experiment 1, there was no attentional effect with the 1200 ms foreperiod, which likely reflects reorienting. The constant-foreperiod paradigm avoids the issue of reorienting in time, and by including a baseline condition, we could demonstrate that endogenous temporal attention can be allocated to a specific point in time even following rather long cue-target intervals.

The second notable finding is that an attentional effect emerged already with our shortest foreperiod. This replicates and extends the finding of Experiment 1 to an even shorter foreperiod of 75 ms (i.e., an SOA of 175 ms), which is particularly remarkable given that the effect of attention is already maximal with this short foreperiod (i.e., it is not just starting to build up). As noted above, with such short foreperiods the potential involvement of exogenous attention is a concern (Ulrich & Mattes, 1996), although our no-intensity-change cue considerably diminishes this concern (Lawrence & Klein, 2013). In Experiment 1 we ruled out exogenous involvement by comparing valid-invalid trials of the same foreperiod. The constant foreperiod paradigm does not readily afford a comparison between trials that differed only in the observers' expectations, as this paradigm does not include invalid trials. Still, we could perform a somewhat similar comparison: On some baseline trials the foreperiod happened to be close to 75 ms or 150 ms, and we compared those trials with the corresponding constant-foreperiod blocks. Specifically, for the baseline condition we included under the labels "75" and "150" trials in which the foreperiod was 75 ± 25 ms and 150 ± 25 ms, respectively. We then compared performance in these two groups to that of the constant-foreperiod blocks of 75 ms and 150 ms, respectively. As can be seen in Fig. 4 and confirmed by preplanned comparisons, accuracy was significantly higher in the constant-foreperiod than baseline blocks for both foreperiod conditions (75: t (11) = 2.855, $p = 0.008$, d$_z = 0.82$; 150: t(11) = 2.337, $p = 0.020$, d$_z = 0.68$). These comparisons were significant even after using the Benjamini-Hochberg procedure to control for multiple comparisons. Thus, this analysis provides further support for the conclusion that these attentional effects do not reflect exogenous attention, rather they reflect particularly fast endogenous allocation of temporal attention. These fast volitional effects were further explored in Experiment 3.
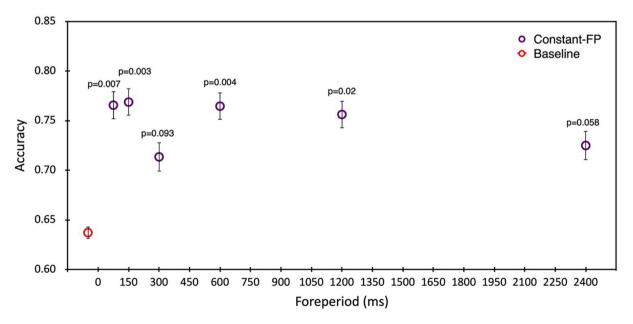

Fig. 3. Accuracy in the constant-foreperiod blocks (constant-FP) of Experiment 2 as a function of cue-target foreperiod. For comparison, the accuracy in the baseline session is also presented. Error bars correspond to one SEM across observers. The *p*-value for each one-tailed paired t-test comparing the baseline block and a constant-FP block of a given foreperiod is presented above that foreperiod.
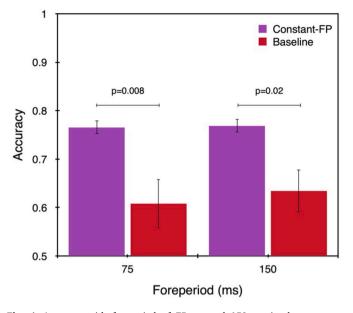
Fig. 4. Accuracy with foreperiod of 75 ms and 150 ms in the constant-foreperiod (constant-FP) and baseline blocks of Experiment 2. Error bars correspond to one SEM across observers. The *p*-value for each one-tailed paired *t*-test comparing the baseline block and a constant-FP block of a given foreperiod is presented above that foreperiod.

5. Experiment 3

The aims of this experiment were twofold. First, it was suggested that visual cues are less efficient warning signals than auditory cues (e.g., Posner, Nissen, & Klein, 1976). Thus, we employed visual cues to test whether the surprisingly fast voluntary allocation of temporal attention occurs also when involving the visual system. Second, we wondered whether an even faster attention allocation can be found if a shorter cue+foreperiod (SOA) duration is employed. To that aim, only 2 foreperiods were employed: 100 ms and 300 ms (mixed within the same session), and cue duration was shortened to 50 ms (i.e., possible SOAs were 150 ms and 350 ms). The target, task, and basic design including cue validity were similar to Experiment 1, only with visual cues. To ensure that the cues do not involve an intensity change they were based on isoluminance color changes (Fig. 5). A baseline condition in which the cue did not bear any temporal contingencies with the target was conducted on a separate session.

5.1. Experiment 3 – Method

5.1.1. Stimuli, apparatus and procedure

These aspects were similar to Experiment 1 except for the following: To ensure that the cues do not involve an intensity change, the continuous white noise was replaced with a red square (2°x2°), which surrounded the fixation throughout the trial, and was isoluminant to the gray background (21 cd/m^2; Fig. 5). When a cue was presented, 2 sides of the square changed their color into an isoluminant green for 50 ms. On valid trials (75%), a color change in the cue's vertical sides was followed by a foreperiod of 100 ms, and a change in the cue's horizontal sides was followed by a foreperiod of 300 ms. On invalid trials (22%), the foreperiod was either shorter or longer than that indicated by the cue (Table 1). Across all invalid trials, the foreperiod ranged from 50 ms to 900 ms.

Additionally, in a separate neutral session, a neutral cue involved a color change in one vertical side and one horizontal side (chosen randomly). The neutral cue and the target bore no temporal contingencies. The range of foreperiods in this neutral session was -830 ms –
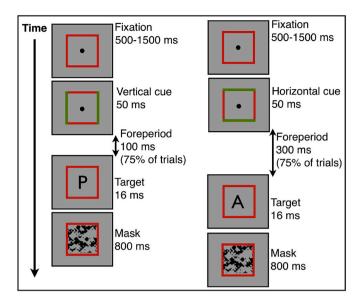

Fig. 5. The sequence of events in the valid trials of Experiment 3. The luminance of the red square and gray background was identical. The attentional cues involved a brief color change into an isoluminant green in 2 sides of the central square. A change in the vertical/horizontal sides indicated that a foreperiod of 100/300 ms will follow with 75% validity. On invalid trials the foreperiod was either shorter or longer than that indicated by the cue. Across all invalid trials, the foreperiod ranged from 50 ms to 900 ms. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

2700 ms. The mask filled the square. Overall, there were 2 sessions (informative and neutral), ran on different days (order counterbalanced), each including 1024 trials.

5.2. Experiment 3 – Results and discussion

Accuracy as a function of foreperiod and cue validity is presented in Fig. 6. As in Experiment 1, because the valid and invalid conditions involved different foreperiods, we conducted a separate 1-way (foreperiod) repeated-measures ANOVA for each of these conditions. These two analyses revealed a significant effect of foreperiod for the invalid condition (F(4,64) = 2.696, p = 0.039, η_p^2 = 0.144) but not for the valid condition (F(1,16) = 1.53, p = 0.234, η_p^2 = 0.087).

The foreperiod effect that emerged for the invalid condition – higher accuracy for longer foreperiods – suggests that in this experiment the employment of a non-aging distribution and catch trials was not enough to eliminate this effect. Perhaps this is due to the fact that the range of foreperiods is considerably smaller in this experiment in comparison to Experiment 1 (the longest foreperiod in Experiment 1 is twice as long as that in this experiment), which made it easier to estimate the point in time to which attention should be reoriented. Importantly, the foreperiod effect observed in the invalid condition rules out a possible alternative explanation. According to this alternative explanation, the validity effect merely reflects the fact that the participants implicitly 'matched' the frequency of the different foreperiods. If this was the case, accuracy should be higher with more prevalent foreperiods, regardless of their validity. Yet, the opposite pattern of results emerged: Due to the non-aging distribution (Table 1), invalid short foreperiods were more frequent than invalid long foreperiods, but their accuracy was lower (Fig. 6).

Next, we test the overall effect of validity. A 1-way (validity) repeated-measures ANOVA revealed a significant main effect of validity (F(1,16) = 12.601, p = 0.003, η_p^2 = 0.44); accuracy was higher in the valid than invalid conditions. As in Experiment 1, to further explore this
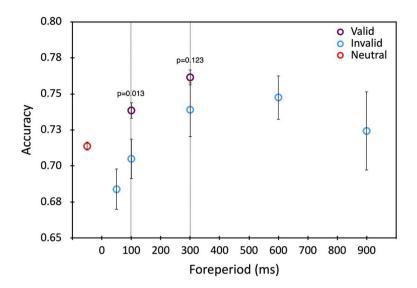
Fig. 6. Accuracy in Experiment 3 as a function of cue-target foreperiod and cue validity. For comparison the accuracy in the neutral session is also presented. Error bars correspond to one SEM across observers. The vertical dashed lines indicate foreperiods that were common for both valid and invalid conditions. The *p*-value for each one-tailed paired *t*-test comparing valid and invalid trials of a given foreperiod is presented above that foreperiod.

validity effect, and particularly to rule out the involvement of exogenous attention allocation, we performed additional analyses (one-tailed paired t-test) in which we compared directly valid and invalid trials of the same foreperiod (i.e., trials that were procedurally identical). These comparisons revealed a significant difference with the foreperiod of 100 ms ($t(16) = 2.447$, $p = 0.013$, $d_z = 0.59$) but not with 300 ms ($t(16) = 1.204$, $p = 0.123$, $d_z = 0.29$). The significant validity effect that emerged already within 150 ms from cue onset is significant even after using the Benjamini-Hochberg procedure to control for multiple comparisons, and it suggests that the allocation of voluntary temporal attention is twice as fast as that of voluntary spatial attention. Importantly, because in this experiment all valid/invalid trials were mixed within a single session, the emergence of a significant validity effect under the current design rules out the possibility that the validity effect of Experiment 1 was due to the collapsing of data across sessions. As detailed above, the lack of validity effect with the foreperiod of 300 ms is likely due to reorienting of attention in time.

Lastly, also as in Experiment 1, we compared accuracy in the neutral session to that in the valid trials, with accuracy collapsed across foreperiod. This analysis revealed no significant difference ($t(16) = 1.119$, $p = 0.140$, $d_z = 0.27$). Thus, unlike Experiment 1, the valid vs. neutral comparison does not match the main analysis (valid vs. invalid). This might be due to the fact that the main analysis compares trials that are mixed within a single session, while the valid vs. neutral analysis compares trials presented in different sessions. Although the order of sessions was counterbalanced across participants, this analysis seems more vulnerable to the influence of order (e.g., strategy carry-over). Indeed, a 2-way mixed design ANOVA indicated that validity (valid vs. neutral) interacted with sessions order ($F(1,15) = 7.199$, $p = 0.017$, $\eta_p^2 = 0.324$): accuracy was lower in the first than second session, but this difference was only significant in the neutral condition (neutral: $p = 0.004$; valid: $p = 0.586$; LSD Post Hoc tests). Hence, the effect of order moderated the effect of temporal expectation. Critically, as explained above, the valid-invalid comparison is not contaminated by order, and order was not a concern also in Experiment 1 because in that experiment there were 4 different experimental sessions, and therefore there was much more room for order randomization, with many of the orders assigned only to a single participant.

To sum, this experiment demonstrates that visual cues can also lead to fast voluntary allocation of temporal attention and that this endogenous attention can be allocated to a point in time within 150 ms from cue

onset (i.e., an SOA of 150 ms).

6. Experiment 4

In this experiment we tested whether we can 'push' this fast allocation of endogenous temporal attention down to an SOA of 75 ms. To that end, we employed the same constant foreperiod paradigm employed in Experiment 2, but here there were only two blocks: One constant-foreperiod block with a foreperiod of 25 ms and a 50 ms auditory cue, and one baseline block in which there were no temporal contingencies between the target and the cue. All other aspects of this experiment were similar to Experiment 2.

6.1. Experiment 4 – Method

6.1.1. Stimuli, apparatus & procedure

These aspects were similar to Experiment 2 except for the following: There were only 2 blocks, ran on the same day in a counterbalanced order. In the constant foreperiod block (80 trials), a foreperiod of 25 ms followed a 50 ms cue. In the baseline block (320 trials) the cue bore no temporal contingencies with the target. The range of foreperiods in this baseline block was -1000 ms – 1000 ms. The 1st block was preceded by 148 practice trials, and the 2nd was preceded by 64 practice trials.

6.2. Experiment 4 - Results and discussion

In this experiment there was no accuracy difference between the two blocks ($F(1,12) = 0.58$, $p = 0.461$, $\eta_p^2 = 0.05$; Fig. 7), suggesting that when the target appeared 75 ms after cue onset voluntary attention allocation was no longer possible.

Importantly, the fact that no benefit emerged here for the constant-foreperiod block rules out an alternative explanation for the results of Experiment 2. According to this alternative explanation, the improved performance found in Experiment 2 with short foreperiods does not reflect attention allocation to a specific point in time but rather a general increase in effort assumed to compensate for the relatively short preparation time afforded by the short foreperiods. However, if this was the case, a similar general increase in effort should have also been adopted here, and a benefit should have emerged for the constant-foreperiod block of this experiment. Thus, the lack of such benefit rules out this alternative explanation.
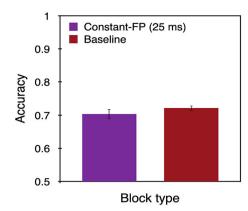
Fig. 7. Accuracy in the constant-foreperiod (Constant-FP) and baseline blocks of Experiment 4. Error bars correspond to one SEM across observers.

7. General discussion

This study examined the time-course of voluntary temporal attention using both the temporal-orienting and constant-foreperiod paradigms. Similar results were obtained with both paradigms. We found that endogenous temporal attention can be allocated rather fast; an interval of 150 ms from cue onset suffices to elicit maximal effects. We also found that attention can be voluntarily allocated to a point in time even following rather long cue-target intervals, at least 2400 ms. Importantly, the effects of attention were measured with a non-speeded letter identification task and therefore they do not merely reflect attentional effects on motor preparation. Hence, although we did not examine spatial attention in this study, comparing our findings with those obtained in studies that examined the time-course of spatial attention (e.g., Cheal & Lyon, 1991) suggests that temporal attention, like spatial attention, affects perceptual processing and these effects can be observed following long cue-target intervals, yet its allocation is twice as fast.

The finding that endogenous temporal attention is similar to spatial attention in its ability to affect perceptual processing is consistent with several previous studies (e.g., Correa, Lupiáñez, & Tudela, 2005; Correa et al., 2006c; Rolke, 2008; Müller-Gethmann et al., 2003; Rolke & Hofmann, 2007; Vangkilde, Coull, & Bundesen, 2012; see Rolke & Ulrich, 2010 for a review). For instance, it was demonstrated that endogenous temporal attention can improve visual acuity (Rolke & Hofmann, 2007), and temporal order judgements (Correa et al., 2006c). Our findings are also consistent with several electrophysiological studies showing that temporal attention can enhance the amplitude of early event-related potentials (ERPs) and reduce their latency in both the visual and auditory modalities (e.g., Correa et al., 2006a; Lange, Krämer, & Röder, 2006; Lange, Rösler, & Röder, 2003; Seibold, Fiedler, & Rolke, 2011; Seibold & Rolke, 2014a). Another similarity between temporal and spatial attention is the emergence of cross-modal attentional effects. That is, the emergence of attentional effects when the cues triggering attention allocation belong to one modality while the aim of this attention allocation – the target – belongs to another modality (as in Experiments 1 & 2). Such cross-modal effects of temporal attention were demonstrated before (e.g., Bausenhart et al., 2010; Chauvin, Gillebert, Rohenkohl, Humphreys, & Nobre, 2016; Lange & Röder, 2006; Lawrence & Klein, 2013; Müller-Gethmann et al., 2003; Seibold & Rolke, 2014b). For instance, Müller-Gethmann et al. (2003) employed the constant foreperiod paradigm with a visual warning cue and an auditory target and found attentional facilitation of RT. Lawrence and Klein (2013) also employed the constant foreperiod paradigm, but like our study, they flipped the modalities – the warning cue was auditory and the target was visual. They too found cross-modal attentional effects on RT and accuracy, but with a speeded task. Hence, we extend these cross-

modal effects to measurements of accuracy in a non-speeded task. Similar cross-modal effects with auditory symbolic cues and a visual target were also demonstrated recently with the temporal orienting paradigm (Chauvin et al., 2016).

Interestingly, we found a fast allocation of endogenous temporal attention regardless of whether the cue was auditory or visual. Several studies have suggested that the auditory system is better at temporal processing than the visual system (e.g., Grahn, 2012; Grondin, Meilleur-Wells, Ouellette, & Macar, 1998; Repp & Penel, 2002). For example, discriminating the duration of brief stimuli (e.g., indicating which of two consecutive stimuli was presented for a longer duration) was better when the stimuli were auditory than visual (e.g., Stauffer, Haldemann, Troche, & Rammsayer, 2012). Similarly, when visual and auditory rhythms that either had a regular temporal structure or not, had to be discriminated, accuracy was higher for the auditory rhythms regardless of rhythm type, though both modalities benefited from the presence of temporal structure (Grahn, 2012). Related to the claim that the auditory system is superior when considering temporal processing, and more relevant for the current study is the conjecture that an auditory warning signal is more effective than a visual signal (e.g., Bertelson & Tisseyre, 1969; Harvey, 1980). This conjecture is based on findings such as faster RTs for a visual stimulus when it is preceded by an auditory than visual warning signal (Harvey, 1980). In contrast, Turatto, Benso, Galfano, and Umilta (2002) found that a warning signal was more effective in reducing RT when it was in the same modality as the target than when they belonged to different modalities, and suggested that the latter is less effective because there is a need to shift from one modality to the other (see also Rodway, 2005). Although our study was not design to directly compare warning signals of different modalities, the fact that fast endogenous allocation was found with cues of both modalities suggests that both modalities are effective. That said, it is important to note that many of these earlier studies referred to automatic attraction of attention rather than the voluntary allocation of attention employed here.

Unlike these similarities, spatial attention and temporal attention clearly differ in one aspect: endogenous temporal attention is considerably faster than its spatial equivalent. The relatively sluggish nature of endogenous spatial attention is typically attributed to two factors: (1) At least part of the sluggishness of endogenous spatial attention was attributed to the need to process the central symbolic cue and figure out the spatial location it indicates before attention can be directed to this location (e.g., Cheal & Lyon, 1991; Cheal, Lyon, & Gottlob, 1994; Müller & Rabbitt, 1989). (2) It was suggested that regardless of the need to decode the symbolic cue, the orienting of endogenous attention to the location indicated by the cue involves a voluntary/controlled mechanism which is slower than its reflexive counterpart (e.g., Cheal et al., 1994; Cheal & Lyon, 1991; Müller & Findlay, 1988; Müller & Rabbitt, 1989; Nakayama & Mackeben, 1989). However, both factors are also involved in the allocation of endogenous temporal attention – symbolic cues were employed in Experiments 1 & 3 and all experiments involved voluntary orienting, yet we observed a considerably faster allocation of endogenous attention in time than in space. Thus, the common assumption that voluntary processes are inherently slow should be reconsidered.

What might be the reason for the different time-course of voluntary allocation of attention in time and space? At this stage we can only speculate. Perhaps there is no temporal analog for the active attentional shifting in space, and this reduces the time required for the voluntary allocation of temporal attention. That is, Posner (1980) suggested that the allocation of spatial attention involves three components: disengaging form the previously-attended location, shifting to the to-be-attended location, and engaging the to-be-attended location. Perhaps the time spent on shifting attention in space is gained with no extra 'cost' with temporal attention due to the natural passage of time, and this allows maximal effects of temporal attention within a shorter time interval. Additionally, spatial attention can dwell on a specific location until there is another location to attend. Perhaps with temporal

attention disengagement also involves minimal cost, because the continuous passage of time does not afford attentional dwelling, resulting in overall faster allocation of endogenous resources to a new point in time. Anyhow, our results concur with the findings that there is some overlap between spatial attention and temporal attention in terms of their neural substrates, but they also differ. Specifically, it was found that temporal cueing preferentially engaged the left posterior parietal and inferior premotor cortex, while spatial cueing was associated with the right posterior parietal cortex (Coull & Nobre, 1998). Hence, it seems viable that the effects of spatial and temporal attention reflect different neural mechanisms, each with its own unique time-course, though evidence regarding the neural correlates of temporal attention is only starting to accumulate (for reviews of recent advances, see Nobre & Rohenkohl, 2014; Nobre & van Ede, 2018).

Finally, this study also has several methodological implications. First, as discussed above, it indicates that auditory and visual cues are similarly effective, at least when considering volitional temporal attention. Second, we replicated Lawrence and Klein's (2013) demonstration that their uncorrelated dichotic noise is an effective auditory warning cue for the *exclusive* manipulation of endogenous temporal attention (i.e., when one wishes to avoid the involvement of involuntary attentional processes), and we further extended its usefulness to non-speeded tasks. Moreover, we demonstrated that isoluminant stimuli are similarly effective for the exclusive manipulation of endogenous temporal attention when focusing on the visual modality. With these two types of cues, future studies can further explore the characteristic of endogenous temporal attention in either modality as well as the different combinations of modalities. Third, we show that the constant-foreperiod and the temporal-orienting paradigms are comparable, although each has its pros and cons. The temporal-orienting paradigm is more suitable for the study of temporal attention when relatively short foreperiods are involved and one wishes to avoid mere repetition as an alternative explanation. That is, unlike the constant-foreperiod paradigm, with the temporal-orienting paradigm foreperiods of different duration are mixed randomly within a block, ruling out the possibility that the observed benefit is due to sequential repetition of foreperiod duration (e.g., Los & Van Den Heuvel, 2001). The constant-foreperiod paradigm is more suitable for the study of temporal attention with longer foreperiods because it does not suffer from the problem of reorienting in time. However, this advantage is applicable only when a baseline condition, in which the cue and target bear no temporal contingencies, is included, as indeed was done in our study. This is because the baseline condition allows us to compare attended performance with long foreperiods with unattended performance, instead of the more common comparison between attended performance with long foreperiods and attended performance with short foreperiods. Lastly, because performance improved over a wide range of foreperiods, future studies can be rather flexible with their choices of foreperiods, optimizing them to their specific goals and tasks.

To conclude, this study is the first to carefully examine the time-course of endogenous temporal attention with a non-speeded task and an attentional cue that does not include intensity change. Thus, unlike previous studies, we could focus exclusively on the effects of temporal attention on perceptual processes and avoid the involvement of involuntary attentional processes. This rigorous examination revealed that endogenous temporal attention can affect perception over a wide range of foreperiods – from 75 ms up to 2400 ms. Notably, when the duration of the cue was factored in, maximal effects of temporal attention emerged already within 150 ms from cue onset, which is twice as fast as the voluntary attention allocation in space. Such fast volitional effects challenge the commonly assumed speed limitation of voluntary processing.

Supplementary material

The data that support the findings of this study are available in the Open Science Framework repository, https://osf.io/ez4gw/?view_only=2c85bc3a11d64c91a2802326c83468d3.

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