Attentional refreshing of information in working memory: Increased immediate accessibility of just-refreshed representations

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Abstract
Working Memory (WM) keeps information temporarily accessible for ongoing cognition. Refreshing is a proposed mechanism to keep information active in WM, by bringing memory items into the focus of attention. We report five experiments in which we examined the local effects of refreshing. Participants were either instructed to refresh (to think of) the different memory items at an imposed pace after list presentation, so that we had experimental control over which item was being reactivated in the focus of attention at different points in time during retention, or were free to spontaneously use refreshing (or not). We present evidence for (1) the presumed local effect of refreshing that is heightened accessibility of the just-refreshed item, (2) the use of speeded responses to WM probes as a direct, independent index of the occurrence of refreshing, and (3) spontaneous occurrence of refreshing of to-be-remembered information during slow list presentation and during an empty delay following list presentation.

Introduction
Working memory (WM) is a limited-capacity system that keeps information temporarily accessible for ongoing thought and action and is, as such, typically considered a keystone of human cognition. Despite broad consensus on the importance of WM for goal-directed cognitive activities such as learning, reasoning, problem solving, language comprehension and mental arithmetic (e.g., Barrouillet, 1996; Daneman & Merikle, 1996; DeStefano & LeFevre, 2004; Engle, Cantor, & Carullo, 1992; Halford, Wilson, & Phillips, 1998; Harrison, Shipstead, & Engle, 2015; Kyllonen & Christal, 1990; Süß, Oberauer, Wittmann, Wilhelm, & Schulze, 2002), there is currently little agreement on how WM works.

One central issue that remains heavily debated is the cause of forgetting and the mechanisms that can counteract the loss of information from WM. According to one view on WM, to-be-remembered information is lost from WM because it decays over time (the temporal decay account; see Ricker, Vergauwe, & Cowan, 2016, for a recent review) and this time-based forgetting is crucial towards pitting decay accounts of WM against other accounts of WM, such as accounts in terms of interference (see Oberauer, Farrell, Jarrold, Pasiecznik, & Greaves, 2012, for a recent review), temporal distinctiveness (Brown, Neath, & Chater, 2007) or displacement from WM (Waugh & Norman, 1965; see also Atkinson & Shiffrin, 1968; James, 1890). But also, and more generally, a good understanding of refreshing is important towards a better understanding of how WM works. Studies that specifically aim at isolating, measuring and detailing the process of refreshing are scarce. With this in mind, the present study proposes a detailed examination of refreshing. Specifically, the presumed local effects of refreshing on WM representations are examined, and a new way of assessing whether refreshing has occurred or not is proposed.

Attentional refreshing
Refreshing refers to an attention-based maintenance process in WM. It is assumed to be similar in many respects to verbal rehearsal but there are some key differences. Whereas refreshing is assumed to rely on attentional reactivation of memory traces,
verbal rehearsal is assumed to rely on subvocal articulation of verbal information. Thus, whereas refreshing is assumed to be an attentional maintenance mechanism that can be used to maintain verbal, but also visuo-spatial, information, rehearsal is assumed to be a speech-related maintenance mechanism that can only be used to maintain verbal information. In contrast to refreshing which is by definition an attention-based process, verbal rehearsal is typically assumed to not, or only minimally, rely on attention (e.g., Baddeley & Logie, 1999; Camos & Barrouillet, 2014; Chen & Cowan, 2009; Naveh-Benjamin & Jonides, 1984). Accordingly, behavioral, developmental and neuroimaging studies strongly suggest that refreshing and verbal rehearsal are two independent maintenance processes (e.g., Camos, Lagner, & Barrouillet, 2009; Cowan et al., 1998; Hudjetz & Oberauer, 2007; Loaiza & McCabe, 2012; Offtinger & Camos, 2016; Raye, Johnson, Mitchell, Greene, & Johnson, 2007; Vergauwe, Camos, & Barrouillet, 2014).

Though considerable research has been devoted to the process of refreshing over recent years, little is currently known about how refreshing operates to support the maintenance of a set of elements in WM. There seems to be general agreement that the process operates by bringing WM representations into the focus of attention (Barrouillet & Camos, 2012; Cowan, 1995; Higgins & Johnson, 2009; Vergauwe & Cowan, 2014, 2015) and that the act of refreshing, or “thinking of”, results in information becoming highly accessible again in WM. This, in turn, is proposed to protect the information from being forgotten. Consistent with this idea, it has been shown that (1) decreasing the time available for refreshing, by manipulating the attentional demands of a secondary task to be performed during a retention interval, results in poorer memory performance (e.g., Barrouillet, Portrat, & Camos, 2011; Barrouillet et al., 2004, 2007; Camos & Portrat, 2015; Hudjetz & Oberauer, 2007; Ricker & Cowan, 2010; Vergauwe, Barrouillet, & Camos, 2009; Vergauwe, Barrouillet, & Camos, 2010; but see Oberauer & Lewandowsky, 2013; Oberauer, Lewandowsky, Farrell, Jarrold, & Greaves, 2012), and (2) increasing the number of times a memory item has been refreshed, by presenting cues prompting participants to think of specific WM items during a retention interval, results in better memory performance for that item (Souza, Rerko, & Oberauer, 2015). In these studies, researchers have focused on the effects of refreshing on memory performance at the end of the trial or at the end of the experimental session. Here, we propose an alternative approach to advance our understanding of refreshing. Rather than examining the effects of refreshing on the final outcome that is memory performance, we aim at examining the presumed effects of refreshing more locally.

The local study of refreshing

One of the first studies to examine the effect of refreshing more locally was done by Vergauwe et al. (2014). In that study, we examined the effect of refreshing on response times (RTs) in a secondary task performed during retention. A short series of to-be-recalled items was followed by a fixed retention delay during which a secondary task was to be performed. The number of to-be-remembered items was varied and we observed that RTs in the secondary task increased as a direct function of the number of items to be remembered (i.e., set size). This pattern indicated that participants spontaneously engaged in refreshing during the retention interval and that refreshing postponed responses in the secondary task, with each additional to-be-remembered item resulting in an additional postponement. Importantly, this was observed for verbal material under articulatory suppression, thereby minimizing the use of verbal rehearsal, and for visuo-spatial material. While we interpreted this pattern as clear evidence for spontaneous refreshing, it was argued that our interpretation of the RT-set size functions was heavily dependent on the theoretical framework within which the study was designed (i.e., the Time-Based Resource-Sharing model; Barrouillet et al., 2004, 2007). In particular, an interpretation of the RT-set size functions in terms of postponement of concurrent processing because of spontaneous refreshing only holds under the assumption that processing and refreshing rely on a common resource that has to be shared in a time-based, sequential way. As such, it has been argued that the Vergauwe et al. (2014) study does not provide independent, direct evidence for refreshing in WM (Lewandowsky & Oberauer, 2015).

In the current study, we aim at testing the refreshing hypothesis in a more direct and independent way. Therefore, we propose to focus on the local effect of refreshing on the WM representations of the to-be-remembered information during retention, rather than on secondary task performance during retention. As we will explain below, this enabled us to use a simpler design and to rely on assumptions that are not specific to refreshing-based accounts of WM.

The local effect of refreshing on WM representations

To maintain a list of items, refreshing is assumed to operate serially, with the focus of attention cycling from one item to the next (e.g., Barrouillet & Camos, 2012; Cowan, 2011; McCabe, 2008; Nee & Jonides, 2013; Vergauwe et al., 2014). The item that is represented in the focus of attention is assumed to be in a privileged state of heightened accessibility (e.g., Basak & Verhaeghen, 2011; Cowan, 1995; McElree, 2006; Nee & Jonides, 2008; Oberauer & Hein, 2012). The presumed local effect of refreshing on WM representations is thus the heightened accessibility of the just-refreshed WM representation. This state of heightened accessibility of the information brought back into the focus of attention has not yet been empirically demonstrated. That is, while there is evidence for a special status of the item in the focus of attention (Garavan, 1998; Oberauer, 2002; see Oberauer & Hein, 2012, for a recent review), there is little or no evidence that this special status applies in the situation when information is refreshed. Providing such evidence was one of our main aims in the current study.

Consistent with the idea of heightened accessibility of the information in the focus of attention, it has been shown that the status in WM of the final item of a memory list is qualitatively different from that of the other to-be-remembered items. For example, in an item-recognition task in which a list of items is followed by a probe to be judged present in or absent from the list of items, it is typically observed that RTs to the last-presented item are faster than to any other item of the list (e.g., Burrows & Okada, 1971; McElree & Dosher, 1989; Nee & Jonides, 2008; Öztekin, Davachi, & McElree, 2010). The idea is that, at the end of the memory list, the final item is maintained in the focus of attention, resulting in speeded responses to probes that match that item. However, a last-presented RT benefit is not always observed in item-recognition (e.g., Clifton & Birenbaum, 1970; Donkin & Nosofsky, 2012; Sternberg, 1966) and recent research suggests that responses to the last-presented item and access to the focus of attention might be dissociable (e.g., Morrison, Conway, & Chein, 2014).

In a recent study, we assumed that speeded responses do not need to be invariably tied to the last-presented item, and tested whether the last-presented benefit in RT can be leveraged and used as an independent, more direct index to assess if refreshing had occurred (Vergauwe et al., 2016). Specifically, we reasoned that, when refreshing occurs, the last-presented item is replaced by another list item in the focus of attention, and thus, speeded responses should no longer be observed for the last-presented item. In four experiments, short series of red letters were
presented for subsequent recall and black probe letters were presented between these memory items, with each probe to be judged present in or absent from the list presented so far (i.e., the probe span task). We manipulated the delay between each memory item and the subsequent probe. The rationale was that, if the delay before the probe is very short, then we expected that refreshing has not yet occurred and the last item remains in the focus of attention. In this case, responses to the last-presented item should be speeded, resulting in a last-presented benefit. If the delay is long, however, then we expected that refreshing should have occurred. As such, the just-refreshed item is assumed to be in the focus of attention but its serial position should vary from trial to trial. As a result, a last-presented benefit should no longer be observed.

In sharp contrast to what we expected, we observed that participants were the fastest to respond to the last-presented memory item at all probe delays, in all four experiments. This invariant pattern over time suggested either (1) that the last-presented item was still in the focus of attention when we probed WM and thus, that no serial refreshing had occurred in our study even though we aimed at creating optimal conditions to observe refreshing, or (2) that speeded responses, and the last-presented benefit in particular, do not reflect the presence of an item in the focus of attention and thus, cannot be used to infer the content of the focus of attention. The present study follows up on this study and explicitly examines whether speeded responses can reflect the presence of an item in the focus of attention in such a way that the last-presented benefit can be used as an independent, direct index to assess if refreshing has occurred.

The current study

The aim of the current study was twofold. First, we aimed at testing the presumed local effect of refreshing, i.e., the heightened accessibility of the just-refreshed WM representation. Therefore, we used an instructed-refreshing paradigm in which participants were presented with a short memory list, followed by a retention interval during which we instructed participants to think of the list items. Importantly, we used cues to have experimental control over which list item was to be refreshed at which point in time. This allowed us to control experimentally which list item was in the focus of attention at different points in time, and thus, which item is assumed to be in a state of heightened accessibility at different points in time. We reasoned that, if the just-refreshed item benefits from a privileged state of increased accessibility, then speeded responses should be observed for this item. Presenting memory probes at different points in time during the retention interval enabled us to test whether responses were indeed particularly fast for probes matching the just-refreshed list item, compared to probes matching any of the other list items (i.e., a just-refreshed benefit).

Our second aim was to examine whether speeded responses can reflect the presence of an item in the focus of attention in such a way that the last-presented benefit can be used as an independent, direct index to assess if refreshing has occurred. In these conditions, no cues were presented, and we manipulated the time available for spontaneous refreshing, either by varying the presentation rate of the memory list or by varying the presence of a retention delay. In these spontaneous-refreshing conditions, we assessed the evidence for or against a last-presented benefit. The rationale was that the presence of a last-presented benefit would reflect the final item still being in the focus of attention, indicating that no refreshing had occurred. The disappearance of the last-presented benefit would reflect that refreshing had taken place so that the last-presented item had been replaced in the focus of attention and no longer elicits speeded responses.

Our approach is novel in several ways: (1) we examine the effects of refreshing on WM representations locally and directly, rather than examining the effects of refreshing on the final memory outcome or on local secondary task performance, (2) we propose an independent, direct index to assess whether refreshing has occurred (or not) that relies on a specific RT pattern, rather than on an increase or decrease of memory accuracy, thereby avoiding the situation whereby one can only rely on memory accuracy to infer whether or not refreshing has taken place while memory accuracy is the to-be-explained behavior, and (3) we use a very simple paradigm, that does not require the presence of a secondary task, thereby avoiding accounts in terms of dual-tasking and speed-accuracy trade-offs. Moreover, using a very basic design allows us to build specific predictions that are less dependent on theory-specific assumptions or, at least less dependent on assumptions that are specific to decay-and-refreshing theories.

Overview of the experiments

Five experiments are reported in which short series of letters were to be remembered. An overview of the five experiments and methodological differences between them can be found in Fig. 1. In Experiment 1, we examined speeded responses in instructed and spontaneous conditions while using a fairly standard rate of presentation of the memory items (1 item per 1000 ms). To anticipate, we observed strong evidence for a just-refreshed benefit in the instructed-refreshing condition. Moreover, in line with the idea that the just-refreshed memory item replaced the last-presented memory item in the instructed-refreshing condition, we found evidence against a last-presented benefit. Next, we assessed the evidence for or against a last-presented benefit in the spontaneous conditions. In the spontaneous-refreshing condition that had an empty retention delay following list presentation, we found strong evidence against speeded responses for the last-presented item, suggesting that the last memory item was no longer in the focus of attention after an empty delay. The same was observed for the spontaneous-refreshing condition in which the probe immediately followed list presentation. This suggested that the last memory item had been replaced in the focus of attention, even when there was no retention delay during which refreshing could occur, indicating that refreshing might have taken place during list presentation.

This idea was tested in Experiment 2 using a faster presentation rate (1 item per 500 ms), thereby reducing the time available for spontaneous refreshing during list presentation. Moreover, we wanted to examine to what extent refreshing in the empty delay condition in Experiment 1 occurred spontaneously, as opposed to being encouraged by our methods. Therefore, in Experiment 2, environmental support facilitating refreshing (see Lilienthal, Hale, & Myerson, 2015) was no longer present during the empty delay and the different trials (spontaneous and instructed) were presented in blocks rather than randomly intermixed. The pattern of results of Experiment 1 was replicated in Experiment 2, except that there now was evidence for a last-presented benefit in the
condition without an empty retention interval. This indicated that the faster presentation rate indeed minimized the spontaneous use of refreshing during list presentation, such that the last-presented item was still in the focus of attention when probing WM right after fast list presentation.

Experiments 3 and 4 were close replications of the first two experiments. We made a few modifications to strengthen our interpretation of the observed pattern in terms of refreshing: (1) Because the evidence in Experiment 2 for a last-item benefit in the No delay condition was not overwhelming, we used an even faster presentation rate of 1 item per 350 ms in Experiment 4. The rationale was that this should minimize the use of refreshing during list presentation even more than the rate used in Experiment 2 (i.e., 1 item per 500 ms). As a result, we expected to find very strong evidence for a last-presented benefit in the No delay condition in Experiment 4. (2) We wanted to test whether evidence for spontaneous refreshing during list presentation would still be observed when refreshing is no longer facilitated by environmental support during list presentation and thus removed any environmental support during empty delay, presentation of trials of different Delay conditions, and the presence of articulatory suppression during encoding and retention.

Finally, in Experiment 5, we aimed at ruling out rehearsal-based accounts of the just-refreshed benefit in the instructed-refreshing conditions and of the disappearance of the last-presented benefit in the spontaneous conditions, findings which we interpreted as evidence for refreshing. Therefore, in Experiment 5, we replicated the main findings of Experiments 1 through 4 under articulatory suppression, thereby demonstrating that verbal rehearsal played no, or only a very small, role in the patterns observed in the first four experiments.

**Experiments 1–5**

**Method**

An overview of the five experiments and methodological differences between them can be found in Fig. 1. The data for all experiments can be accessed through the Open Science Framework ([https://osf.io/36dwq/](https://osf.io/36dwq/)).

**Participants**

Subjects were undergraduate students at the University of Geneva and received partial course credit or were paid for their participation. All had normal or corrected-to-normal vision. In Experiments 1 through 4, there were 30 participants. In Experiment 5, we started with a sample size of 30 participants and added another 15 participants to have a similar amount of data in this experiment as in Experiments 1 through 4 after performance-based exclusions (discussed below). Given Bayesian analyses, there is little danger from collecting additional data (Rouder, 2014).

**Materials and procedure: General**

In this section we outline aspects of the methods that were common across the five experiments. The task (illustrated in Fig. 1) was administered using E-prime software (Psychology Software Tools). Participants were asked to watch carefully and memorize series of four letters presented sequentially, chosen randomly.

![Table of experimental factors](https://osf.io/36dwq/).
Materials and procedure: Experiment 1

Each to-be-remembered letter was presented for 750 ms, and each letter was followed by a screen showing the fixation cross and the four empty boxes for 250 ms. In the Empty delay condition, the four empty boxes with the fixation cross were presented on screen during the entire delay, regardless of its duration. In the Instructed-refreshing condition, the four empty boxes were highlighted at a rate of one box per second, in the order of presentation, after a brief delay of 1000 ms during which the four empty boxes were shown (see Souza et al., 2015, for a similar procedure). The trials of the different experimental conditions were presented in random order and participants performed 6 practice trials before the 180 experimental trials.

Materials and procedure: Experiment 2

Each to-be-remembered letter remained on screen for 250 ms (compared to 750 ms, in Experiment 1), and each letter was followed by a screen showing the fixation cross and the four empty boxes for 250 ms. In the Empty delay condition, the screen remained empty during the delay, except for the fixation cross that remained on screen. This is different from Experiment 1 in which the four empty boxes were presented on screen during the empty delay. Like in Experiment 1, in the Instructed-refreshing condition, the four empty boxes were highlighted at a rate of one box per second, in the order of presentation, after a brief delay of 1000 ms during which the four empty boxes were shown. Whereas the different experimental conditions were intermixed in Experiment 1, the 180 experimental trials of Experiment 2 were presented in three separate blocks (one block per experimental condition), with the order of the blocks counterbalanced across participants. Two practice trials were presented before each block.

Materials and procedure: Experiment 3

Each to-be-remembered letter was presented for 750 ms, and each letter was followed by a screen showing the fixation cross for 250 ms. Thus, in contrast to Experiments 1 and 2, Experiment 3 no longer showed the four empty boxes during the brief delays following letter presentation. Like in Experiment 2, only the fixation cross remained on screen during the delay in the Empty delay condition. In the Instructed-refreshing condition, the four empty boxes were highlighted at a rate of one box per second, in the order of presentation, and highlighting of the first box immediately followed presentation of the last memory item in the Instructed-refreshing condition (i.e., the four boxes were no longer shown simultaneously for 1000 ms before the first box was highlighted). Like in Experiment 2, the 180 experimental trials were presented in three separate blocks, with the order of the blocks counterbalanced across participants, and with 2 practice trials before each block.

Materials and procedure: Experiment 4

Each to-be-remembered letter was presented for 250 ms, and each letter was followed by a screen showing the fixation cross for 100 ms. Like Experiment 3, Experiment 4 no longer showed the four empty boxes during the brief delays following letter
presentation. Like in Experiments 2 and 3, only the fixation cross remained on screen during the delay in the Empty delay condition. Like in Experiment 3, highlighting of the first box immediately followed presentation of the last memory item in the Instructed-refreshing condition, and the 180 experimental trials were presented in three separate blocks, with the order of the blocks counterbalanced across participants, and with 2 practice trials before each block.

**Materials and procedure: Experiment 5**

The materials and procedure were the same as Experiment 3, except that participants required to repeat the syllables “babibou” (articulatory suppression) during encoding and retention, to minimize the use of verbal rehearsal.

**Performance-based exclusions**

We applied the same performance-based exclusions as in the Vergauwe et al. (2016) study, to keep things consistent between studies. First, we discarded the data of participants whose average accuracy of their response to the probes fell below 55% (1, 0, 0, 0, and 4 participants in Experiments 1 through 5, respectively). Next, we verified participants’ precise compliance with the instructions in the Instructed-refreshing Delay condition. Because it is important that participants judged whether the probe matches any of the memory items, rather than judging whether the probe matches the just-refreshed memory item, we excluded the data of participants who scored below 55% on target-present probes that did not match the just-refreshed memory item (3, 2, 0, 1, and 10 participants in Experiments 1 through 5, respectively). These exclusions resulted in a final sample of 26 (out of 30), 28 (out of 30), 30 (out of 30), 29 (out of 30) and 31 (out of 45) participants in Experiments 1 through 5, respectively.¹

**Method of analysis**

To test the presumed local effect of refreshing on WM representations, we examined the evidence in the data for a just-refreshed benefit. Therefore, in the Instructed-refreshing Delay condition of each experiment, we compared correct RTs to probes matching the just-refreshed item to RTs to target-present probes that did not match the just-refreshed item. Per experiment, one-sided Bayesian t-test was run, testing whether RTs to probes matching the just-refreshed item were faster than RTs to target-present probes that did not match the just-refreshed item. Analyses were run using JASP (2016), with the default settings. No outlier detection/correction procedure was used because responses were not recorded after 2000 ms.

Next, in each Delay condition, we examined the evidence for or against a last-presented benefit. Therefore, per experiment, we compared RTs to probes matching the last-presented item to RTs to target-present probes that did not match the last-presented item. Importantly, in the Instructed-refreshing condition, target-present probes matching the just-refreshed item were excluded from this analysis. For each condition, a separate one-sided Bayesian t-test was run, testing whether the RTs to probes matching the last-presented items were faster than RTs to target-present probes that did not match the last-presented item. This was done for each experiment and the results of our analyses are shown in **Table 1**. A more detailed break-down of the results can be found in Supplementary material 1 (No delay and Empty delay conditions), Supplementary material 2 (Instructed-refreshing delay condition) and Supplementary material 3 (comparison across Empty delay and Instructed-refreshing delay conditions).

**Results and discussion**

**General performance**

As expected, participants had high rates of correct responses to the probes: 93%, 92%, 93%, 91%, and 78%, in Experiments 1 through 5, respectively. The requirement to perform articulatory suppression during encoding and retention in Experiment 5 resulted in lower performance than in the other experiments but performance remained quite high, see **Tables 2 and 3**.

**Instructed-refreshing delay condition**

What can be seen immediately in **Table 1** and **Fig. 2** is that clear evidence for a just-refreshed benefit was found in all five experiments. Indeed, in all Instructed-refreshing Delay conditions, very strong evidence was found for particularly fast responses to probes that matched the just-refreshed memory item (with a Bayes factor ranging between 178.30 and 5.88 × 10^4 for a just-refreshed benefit), demonstrating the privileged state of heightened accessibility of the just-refreshed item, regardless of the rate of presentation. The mean difference in RT for probes matching the just-refreshed memory item vs. other target-present probes, and corresponding Cohen’s d were the following: 102 ms (SE = 25 ms, d = 0.81), 115 ms (SE = 18 ms, d = 1.19), 111 ms (SE = 13 ms, d = 1.51), 87 ms (SE = 15 ms, d = 1.10), and 71 ms (SE = 15 ms, d = 0.87), for Experiments 1 through 5, respectively.

Moreover, consistent with the idea that instructed refreshing resulted in replacing the last-presented item by another list item in the focus of attention, we found strong evidence against a last-presented benefit in the Instructed-refreshing condition (with a Bayes factor ranging between 6.96 and 15.00 against a last-presented benefit), see **Fig. 3** and **Table 1**. This data pattern indicates that, in the Instructed-refreshing Delay condition, the accessibility of an item depended on its status in WM (in or out of the focus of attention), rather than on its status in the external world (last-presented or not).

**No delay and empty delay conditions**

Besides the instructed-refreshing Delay condition, we had included experimental conditions in which no cues were presented. In the No Delay and Empty Delay conditions, we assessed the evidence for or against a last-presented benefit, see **Fig. 3**. The rationale was that, if participants spontaneously refresh the memory items, then the last-presented memory item would have been replaced in the focus of attention and thus, a last-presented benefit would no longer be observed.

Concerning the No delay condition, our results suggest that the presence of a last-presented benefit depends on the rate at which the memory list was presented. In the experiments in which the memory items were presented at a slow rate (1000 ms per memory item; Experiments 1, 3, and 5), we found evidence against a last-presented benefit, with a Bayes factor ranging between 4.38 and 15.45 against a last-presented benefit. In the experiments with faster presentation rates, however, we found some evidence for a last-presented benefit when items were presented at a rate of 1 item per 500 ms (BF = 3.92 in Experiment 2) and very strong evidence for a last-presented benefit when items were presented at an even faster rate of 1 item per 350 ms (BF = 375.7 in Experiment 4). The mean difference in RT for probes matching the last-presented memory item vs. other target-present probes, and corresponding Cohen’s d were the following, for Experiments 2 and 4, respectively: 31 ms (SE = 13 ms, d = 0.44) and 72 ms (SE = 16 ms, d = 0.82). This pattern is consistent with the idea that, in contrast to fast presentation rates, slow presentation rates allow for

¹ It is worth noting that these participants were exceptional, in that performance on target-present probes that did not match the just-refreshed memory item was generally high, with a mean of 0.85 across participants and across Experiments 1 through 5.
attentional refreshing during list presentation, thereby replacing the last-presented item in the focus of attention. As a result, probes matching the last-presented item no longer elicit speeded responses when memory items are presented slowly. The fact that this was also observed in Experiment 5 under articulatory suppression strongly suggests that the absence of a last-presented benefit does not depend on the use of verbal rehearsal.

A formal across-experiment analysis of the last-presented benefit in the No delay condition provided very strong evidence for the idea that the presence of a last-presented benefit depends on the rate at which the memory list was presented. Using the BayesFactor package for the R statistical-analysis language with the default settings, a Bayesian analysis of variance (BANOVA; Rouder, Morey, Speckman, & Province, 2012) with Target-present probe type (last-presented vs. not-last-presented) as within-subjects variable and Presentation rate (Slow: Experiments 1, 3 and 5, vs. Fast: Experiments 2 and 4) as between-subject factor revealed that the best model was the full model including the interaction and that this model was about 380 times better than the two main–effects model not including the interaction.

In the Empty delay condition, we observed strong evidence against a last-presented benefit in all experiments, with a Bayes factor ranging between 10.78 and 17.31 against a last-presented benefit. This observation indicates that, after an empty delay, the last-presented item is no longer in the focus of attention, and is consistent with the idea that participants spontaneously refreshed the memory items during the empty retention interval. The fact that the last-presented benefit was also absent in Experiment 5 under articulatory suppression indicates that the absence of a last-presented benefit after an empty retention interval does not depend on the use of verbal rehearsal.

Concerning our observations in the Empty delay condition, one might argue that it is possible that people combine the list items into a single chunk during the empty delay, rather than serially refreshing the list items, resulting in the disappearance of the last-presented benefit after an empty delay. A recent study on the relationship between refreshing and chunking suggested that chunking consists of several sub-processes, one of them being the search in long-term memory for a knowledge component that matches the sequence of items currently represented in WM (Portrat, Guida, Phénix, & Lemaire, 2016). The duration of this sub-process was estimated at 1500–2000 ms, making chunking a time-consuming process. Under these assumptions, one would have to assume that an empty delay of 1000 ms would not be enough to combine the items into a single chunk and thus, a last-presented benefit should still be observed. We examined this in Experiment 4, which used a very fast presentation rate and for which we observed a clear last-presented benefit right after list presentation. Only examining the Empty delay trials in which the delay had a duration of 1000 ms, we observed strong evidence against a last-presented benefit (BF01 = 10.38). This indicates that the attentional process by which the last-presented benefit disappeared took less than 1000 ms, which does not appear to be in line with the chunking model.

| Table 1 |
| Evidence in the data for (in green) or against (in red) the last-presented benefit and the just-refreshed benefit in Experiments 1 through 5 as well as the Control Experiment. Bayes factors are from paired, one-sided t-tests testing the described benefits (for the last-presented benefit: faster responses for last-presented item, compared to other target-present probes; for the just-refreshed benefit: faster responses for just-refreshed item, compared to other target-present probes). Note: AS = articulatory suppression. |
| | No delay | Empty delay | Instructed-refreshing delay |
| | Last-presented benefit | Last-presented benefit | Last-presented benefit | Just-refreshed benefit |
| Experiment 1 | 1000 ms/item | 26 participants | 15.45 | 11.34 | 15.00 | 178.30 |
| Experiment 2 | 500 ms/item | 28 participants | 3.92 | 17.31 | 6.96 | 35408 |
| Experiment 3 | 1000 ms/item | 30 participants | 5.30 | 11.03 | 14.65 | 5.88x10^4 |
| Experiment 4 | 350 ms/item | 29 participants | 375.7 | 12.51 | 14.52 | 17342 |
| Experiment 5 | 1000 ms/item + AS | 31 participants | 4.38 | 10.78 | 7.40 | 1270 |
| Control experiment | 350 ms/item + Uniform probe distribution | 29 participants | -- | -- | 21.59 | 34606 |

| Table 2 |
| Mean accuracy (and SD) for probes matching the just-refreshed item vs. probes matching other list items in the Instructed-refreshing condition, for Experiments 1 through 5, as well as the Control Experiment. Note: AS = articulatory suppression. |
| | Just-refreshed item | Other target-present probes |
| | | |
| Experiment 1 | 1000 ms/item | 0.94 (0.06) | 0.89 (0.10) |
| Experiment 2 | 500 ms/item | 0.93 (0.08) | 0.84 (0.13) |
| Experiment 3 | 1000 ms/item | 0.94 (0.09) | 0.90 (0.11) |
| Experiment 4 | 350 ms/item | 0.92 (0.09) | 0.88 (0.12) |
| Experiment 5 | 1000 ms/item + AS | 0.77 (0.15) | 0.75 (0.12) |
| Control experiment | 350 ms/item + uniform probe distribution | 0.92 (0.09) | 0.89 (0.09) |
Table 3
Mean accuracy (and SD) for probes matching the last-presented item vs. probes matching other list items (excluding probes matching the just-refreshed item) per experimental condition (No delay, Empty delay, Instructed refreshing), for Experiments 1 through 5, as well as for the Control Experiment. Note: AS = articulatory suppression.

<table>
<thead>
<tr>
<th>Experiment</th>
<th>Condition</th>
<th>Last-presented item</th>
<th>Other target-present probes</th>
<th>Last-presented item</th>
<th>Other target-present probes</th>
<th>Last-presented item</th>
<th>Other target-present probes</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>No delay 1000 ms/item</td>
<td>0.96 (0.07)</td>
<td>0.96 (0.06)</td>
<td>0.92 (0.06)</td>
<td>0.93 (0.07)</td>
<td>0.86 (0.15)</td>
<td>0.92 (0.10)</td>
</tr>
<tr>
<td>2</td>
<td>No delay 500 ms/item</td>
<td>0.95 (0.05)</td>
<td>0.90 (0.11)</td>
<td>0.91 (0.09)</td>
<td>0.91 (0.11)</td>
<td>0.80 (0.17)</td>
<td>0.88 (0.13)</td>
</tr>
<tr>
<td>3</td>
<td>No delay 1000 ms/item</td>
<td>0.95 (0.07)</td>
<td>0.91 (0.10)</td>
<td>0.94 (0.07)</td>
<td>0.93 (0.08)</td>
<td>0.89 (0.12)</td>
<td>0.91 (0.11)</td>
</tr>
<tr>
<td>4</td>
<td>No delay 350 ms/item</td>
<td>0.96 (0.05)</td>
<td>0.86 (0.14)</td>
<td>0.93 (0.07)</td>
<td>0.92 (0.07)</td>
<td>0.87 (0.18)</td>
<td>0.89 (0.11)</td>
</tr>
<tr>
<td>5</td>
<td>No delay 1000 ms/item + AS</td>
<td>0.81 (0.15)</td>
<td>0.72 (0.19)</td>
<td>0.72 (0.21)</td>
<td>0.74 (0.18)</td>
<td>0.75 (0.18)</td>
<td>0.74 (0.13)</td>
</tr>
<tr>
<td>Control</td>
<td>Experiment 350 ms/item + Uniform probe</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>0.83 (0.15)</td>
<td>0.91 (0.07)</td>
</tr>
</tbody>
</table>

Fig. 2. Mean response times in ms for probes matching the just-refreshed item vs. probes matching other list items, for Experiments 1 through 5 as well as the Control Experiment. Error bars show standard errors of the mean.

with estimations of chunking speed. Of course, it might be possible that, as elements get more and more refreshed, some additional process in the form of chunking takes place as well.

Together, Experiments 1 through 5 present evidence for the presumed local effect of refreshing that is heightened accessibility of the just-refreshed item and for spontaneous occurrence of refreshing of to-be-remembered information during slow list presentation and during an empty delay following list presentation. However, there is an alternative account of the just-refreshed benefit that we cannot completely discard based on the data of Experiments 1 through 5. In all five experiments, the just-refreshed item was more likely to match the probe than other memory items; the probe corresponded to the just-refreshed memory item on 1/3 of the trials, to any of the other memory items on 1/3 of the trials, and to a new letter on the remaining 1/3 of the trials. This distribution was chosen to optimize our design in terms of data points per cell. However, there is the possibility that participants implicitly learned the probability distribution of matches in the experimental environment and applied a strategy of prioritizing the to-be-refreshed memory item because it was more likely to match the probe.

There is at least one element in the data of Experiments 1 through 5 that is not consistent with the idea that participants consistently and strategically prioritized the memory items that were more likely to match the probe. Because we aimed at assessing the evidence for or against a last-presented benefit in the No delay and Empty delay conditions of Experiments 1 through 5, the last-presented memory item was the item that was most likely to match the probe in those conditions (1/3 of the trials). If participants were consistently and strategically prioritizing the memory items that were more likely to match the probe, we would have expected participants to maximize the evidence for or against a last-presented benefit across all experiments. This is clearly not what we observed. Still, the most convincing and straightforward way of ruling out a prioritization account of the just-refreshed benefit would be to demonstrate the benefit with a uniform distribution of matches over all list items. Thus, we ran a control experiment in which we used a uniform distribution of matches over all list items, while also using the more typical distribution of 1/2 target-present probes vs. 1/2 target-absent probes.

Control experiment

Method

Participants
Thirty undergraduate students at the University of Geneva participated and received partial course credit or were paid for their participation. All had normal or corrected-to-normal vision. Performance-based exclusions resulted in a final sample of 29 participants.

Materials and procedure
This control experiment was a close replication of Experiment 4. Materials and procedure were identical to those used in Experiment 4 (i.e., remembered letters were presented at a rate of 350 ms per letter), except for the following modifications. First, we only included the Instructed-refreshing delay condition. Second, the distribution of probe identity was modified so that the probe corresponded to one of the to-be-remembered letters in half of the trials (target-present probes) and to a random new letter in the remaining half of trials (target-absent probes). Furthermore, a uniform

\[ \text{We opted to replicate the experiment with the fastest presentation rate (Experiment 4), to limit the duration of this control experiment.} \]
distribution over all list items was used for target-present probes, so that each list item had equal chances of being used as target-present probe. Third, participants were presented with 160 instructed-refreshing trials, resulting in 80 target-present probes and 80 target-absent probes. Because of the uniform distribution over all list items for the target-probe, this resulted in 20 trials in which the probe matched the just-refreshed item and thus in a similar amount of data per participant in the just-refreshed condition, relative to Experiments 1 through 5.

Performance-based exclusions and method of analysis
We applied the same exclusions as in Experiments 1 through 5. This resulted in discarding the data of 1 participant who scored below 55% on target-present probes that did not match the just-refreshed memory item. Thus, we analyzed the data of 29 participants. The same method of analysis was used as in Experiments 1 through 5.

Results and discussion
As expected, participants had high rates of correct responses to the probes (92%). The pattern observed in the Instructed-delay conditions of Experiments 1 through 5 was nicely replicated, see Fig. 2; very strong evidence was found for particularly fast responses to probes that matched the just-refreshed memory item (Bayes factor of 34606 for a just-refreshed benefit), demonstrating the just-refreshed benefit with a more typical ratio of target-present vs. target-absent probes and under a uniform distribution over all list items for target-present probes. The mean difference in RT for probes matching the just-refreshed memory item vs. other target-present probes, and corresponding Cohen’s $d$ was 55 ms ($SE = 9$ ms, $d = 1.15$). This observation suggests that the just-refreshed benefit observed in Experiments 1 through 5 cannot be accounted for by assuming that participants prioritized the memory item that was most likely to match the probe.

Consistent with the idea that instructed refreshing resulted in replacing the last-presented item by another list item in the focus of attention, and consistent with our observations in Experiments 1 through 5, we found again strong evidence against a last-presented benefit (Bayes factor of 21.59), see lower panel of Fig. 3.

General discussion
The reported experiments demonstrate that participants were faster to respond to probes that match the item that is currently represented in the focus of attention. In line with the presumed local effect of refreshing on WM representations, we found clear evidence for a just-refreshed benefit in all experiments. This finding supports the hypothesis of a privileged state of heightened accessibility of the just-refreshed item in WM. Moreover, consistent with the idea that instructed refreshing resulted in replacing the last-presented item by another list item in the focus of attention, we found strong evidence against a last-presented benefit in the instructed-refreshing conditions. Together, our experiments indicate that, under instructed refreshing, the accessibility of an item depended on its status in WM (in or out of the focus of attention), rather than on its status in the external world (last-presented or not).

When probing WM immediately after list presentation, evidence for a last-presented benefit varied with the presentation rate of the to-be-remembered information, from strong evidence against a last-presented benefit for slower rates (1 per 1000 ms; Experiments 1, 3, and 5), some evidence for a last-presented benefit for faster rates (1 per 500 ms; Experiment 2) and very strong evidence for a last-presented benefit for the very fast rates (1 per 350 ms; Experiment 4). This pattern suggests that slower presentation rates allow for more attentional refreshing during list presentation, thereby replacing the last-presented item in the focus of attention. As a result, probes matching the last-presented item no longer elicit speeded responses right after slow list presentation. This was also observed when there was no environmental support in between list items. The fact that this was also observed under articulatory suppression strongly suggests that the absence of a last-presented benefit does not depend on the use of verbal rehearsal during list presentation.
Finally, when WM was probed after an empty delay, either with or without environmental support, there was strong evidence against a last-presented benefit in all experiments. This suggests that, after an empty delay, the last-presented item is no longer in the focus of attention, and is consistent with the idea that participants spontaneously refreshed the memory items during the empty retention interval. The fact that the last-presented benefit was also absent in Experiment 5 under articularatory suppression indicates that the absence of a last-presented benefit does not depend on the use of verbal rehearsal during the empty delay.

Together, this pattern of results confirms the presumed local effect of refreshing that is the heightened accessibility of the just-refreshed item. Moreover, the pattern of evidence for and against the last-presented benefit across the instructed and spontaneous conditions, and across the different experiments, supports the notion that the last-presented benefit can be used as an independent, direct index of whether refreshing has occurred. We will discuss the implications of these conclusions in turn.

**Heightened-accessibility of the just-refreshed memory item**

Direct evidence for the presumed facilitative effects of refreshing on WM representations is scarce. Souza et al. (2015) have shown improvements of recall performance for colors that were refreshed more often, thereby providing direct evidence for the presumed final outcome of refreshing, i.e., less forgetting. Here, we provide, for the first time, evidence for the presumed local effect of refreshing, i.e., heightened accessibility of the just-refreshed item. As such, the current findings empirically demonstrate one of the main assumptions underlying attention-based maintenance in WM. Indeed, the WM maintenance mechanism of refreshing relies heavily on the assumption of heightened accessibility of the information represented in the focus of attention but, up till now, this privileged state of the memory item in the focus of attention had not yet been empirically demonstrated. Here, we provide direct evidence for that assumption. To our knowledge, we are among the first to demonstrate the local effects of refreshing in terms of increased accessibility. McElree (2006) reported an experiment in which participants were instructed to verbally rehearse memory items at an imposed rate and observed faster retrieval rates for the just-rehearsed item (see Seamon, 1976, for a similar finding using controlled verbal rehearsal). These findings under instructed verbal rehearsal are relevant for the current study because they also use specific maintenance instructions to control and examine covert maintenance processes. However, they do not inform us on the local effect of refreshing in terms of increased accessibility, which we demonstrated under articularatory rehearsal to explicitly minimize the use of verbal rehearsal. That is, while McElree (2006) and Seamon (1976) used verbal rehearsal instructions, we explicitly tried to minimize the use of verbal rehearsal, so to isolate the local effects of refreshing.

It is currently unclear what exactly participants do when following instructions asking them to “think of” a particular item (see Johnson et al., 2013; Souza et al., 2015, for similar instructions). The observation of a just-refreshed benefit under articulatory suppression shows that participants can still think about an item even though subvocal articulation is prevented, but more research is needed to examine the nature of the refreshing process and the nature of the representations involved, both when refreshing occurs spontaneously and when it occurs under instructions. Also, while our observations are mostly consistent with the idea that participants were following the refreshing instructions and were thus thinking about the item that was cued, participants could also have refreshed the entire list, or the cued list item as well as the preceding list items. The current data set does not allow us to distinguish between these possibilities in a convincing way.

More broadly, our present demonstration of heightened accessibility of the information represented in the focus of attention in WM is consistent with the view that common attentional mechanisms exist in perception and WM (e.g., Johnson et al., 2013; Kiyonaga & Egner, 2013), with an attentional facilitation effect in both domains. Attentional facilitation in the domain of perception refers to the facilitative effect observed for stimuli presented in the current focus of external attention, i.e., the observation of faster behavioral responses to external stimuli presented at cued locations (e.g., Johnston, McCann, & Remington, 1995; Posner, 1980). The observed facilitation for targets presented in attended locations shows how attention can expedite human perception, resulting in faster behavioral responses to stimuli in the focus of external attention. We observed a similar attentional facilitation effect in WM, with faster behavioral responses to external stimuli that match the information that is currently represented in the focus of internal attention.

Of high relevance to the idea of common attentional mechanisms for perception and WM is the Johnson et al. (2013) study which, in sharp contrast to the current study, reported evidence for decreased immediate access to the just-refreshed item in WM. There are several methodological differences between the two studies, but one main difference concerns the time scale. Whereas Johnson et al. observed an inhibition effect in WM with the probe appearing 1600 ms after the onset of the refreshing cue, we observed a facilitation effect in WM with the probe appearing 1000 ms after the onset of the refreshing cue. It is possible that in WM, like in perception, but on a slower time scale, attentional facilitation occurs for the item in the focus of attention, followed by later inhibition for that item. Johnson et al. (2013) suggested this possibility but attentional facilitation in WM had not been demonstrated yet. Here, we provide evidence for this view, thereby strengthening the analogy between attentional effects in perception and WM. Of course, analogous effects in perception and WM can occur because of common attentional mechanisms or because of separate attentional mechanisms embodying similar principles, and more research will be needed to disentangle these possibilities. Also, it is worth noting that the time-line of facilitation vs. inhibition in WM is consistent with active removal if an item first needs to be brought into the focus of attention in order to be removed (e.g., Ecker, Lewandowsky, & Oberauer, 2014; Ecker, Oberauer, & Lewandowsky, 2014; Oberauer et al., 2012).

The current results are consistent with the existence of a flexible 1-item focus of attention in WM, with the item represented in the focus of attention having a special status (e.g., Oberauer, 2002; Sandry, Schwark, & MacDonald, 2014). When it comes to the specific nature of this special status, we observe a clearly positive effect on the accessibility of the item in the focus of attention, whereas Johnson et al. (2013) observed a negative effect on the accessibility of the item in the focus of attention. This is, however, not the first instance of contrasting findings concerning the nature of the special status of the information in the focus of attention. For example, while studies have shown that the information in the focus of attention is protected from interference (e.g., van Moorselaar, Gunseli, Theeuwes, & Olivers, 2015), other studies have shown that the information in the focus of attention is particularly vulnerable to interference (e.g., Hu, Hitch, Baddeley, Zhang, & Allen, 2014). The current study adds to that debate by demonstrating a special status of the information in the focus of attention and suggesting that temporal parameters might be crucial in observing benefits vs. costs of this special status.

**The last-presented benefit as an independent, direct index of refreshing**

The pattern of results that emerges from the ensemble of our conditions and experiments suggests that, in future studies, the
last-presented benefit can be leveraged to infer whether or not refreshing has taken place during encoding and/or retention in WM tasks. The use of the last-presented benefit as an index for the occurrence of refreshing would avoid situations in which the occurrence of refreshing has to be inferred from changes in memory accuracy with memory accuracy being the to-be-explained data. As we mentioned in the introduction, we have used the last-presented benefit as an independent measure of refreshing in a previous study in which we only studied spontaneous refreshing and never included refreshing instructions. Across four experiments, we had found a clear last-presented benefit across several experimental manipulations that aimed at creating optimal conditions for refreshing, suggesting either that no refreshing had spontaneously occurred or that the last-presented benefit cannot be used an independent, direct index of refreshing (Vergauwe et al., 2016).

Here, however, instructed-refreshing abolished the last-presented benefit and we also observed strong evidence against a last-presented benefit when the probe is presented after an empty delay or immediately following slow list presentation, suggesting that, in the current study, spontaneous refreshing occurred in conditions with enough time available for refreshing. One possibility might be that the presence of a probe, in between each letter presentation, in the probe-span task used by Vergauwe et al. (2016) might have discouraged the spontaneous use of refreshing, and/or that including a condition of instructed refreshing in the current study might have encouraged the spontaneous use of refreshing, even in the conditions without instructions.3 The observations that people can refresh items when they are instructed to do so and that people spontaneously refresh in experiments that also include instructed-refreshing conditions do not necessarily imply that refreshing occurs spontaneously in all situations. There is the possibility that refreshing does not occur spontaneously in task contexts that are entirely free of refreshing instructions. Also, there is the possibility that spontaneous refreshing does not take place in a serial manner in task contexts that are entirely free of serial refreshing instructions. We have a way of testing these ideas in our data. Across Experiments 2 through 5, we have 39 participants for whom the Empty-delay condition was the first block of trials. When these participants performed these empty-delay trials, they were unaware of the fact that refreshing instructions would follow later in the experiment. Analyzing these data in the same way as we described before, we still observed strong evidence against a last-item benefit after an empty delay ($BF_{01} = 18.55$). This indicates that spontaneous refreshing occurred during an empty delay, even for participants who had not been confronted yet with instructed refreshing. Future studies will have to explore the boundary conditions for spontaneous refreshing as assessed through the last-presented benefit.

An alternative account of the last-presented benefit might be proposed within a temporal distinctiveness framework (Brown et al., 2007) by which the benefit would reflect the item being more temporally isolated rather than being in the focus of attention. Our observation that adding an empty delay leads to the disappearance of a last-presented benefit might be consistent with a temporal distinctiveness account: adding an empty delay would lead to more compression and the last-presented item might thus lose its temporal isolation advantage. However, within a temporal distinctiveness view, it is difficult to account for the disappearance of the last-presented benefit right after slower presentation, compared to right after fast presentation because temporal distinctiveness is assumed to be scale-invariant and thus, presentation rate is not assumed to change the temporal distinctiveness of the list items across the No-delay conditions of our experiments. Thus, while temporal distinctiveness might play a role in some of our results, it cannot account for the ensemble of our results.

Another alternative account of the last-presented benefit observed immediately after fast list presentation consists in assuming that the last-presented item is faster responded to than the earlier list items because the earlier items were not fully encoded. For this account to work, one would also have to assume that the last-presented item was fully encoded, or at least more than the earlier items. It is unclear why one would assume that the last-presented item would be more fully encoded/consolidated, relative to the earlier items, since the same presentation rate was used for all list items. Moreover, our data is not consistent with an encoding account of the last-presented benefit. For example, in Experiment 4, all items had 350 ms for encoding/consolidation. Thus, there was as much time for encoding/consolidation of the last-presented item as there was for each of the earlier list items. Moreover, in the No-delay condition, there was no additional delay before the probe and thus there was no additional time during which encoding/consolidation of the last-presented item could continue. Despite the fact that all items had the same time for encoding/consolidation in this condition, we observed very strong evidence for a last-presented benefit.

It is worth noting that the current pattern is reasonably in line with common estimations of encoding speed (500 ms per item; e.g., Jolicoeur & Dell’Acqua, 1998; Oberauer & Lewandowsky, 2011), with clear evidence for spontaneous refreshing when items are presented at a rate that allows for refreshing after the just-presented memory item is fully encoded (i.e., when items are presented at a rate of 1 per 1000 ms), and clear evidence against spontaneous refreshing when items are presented at a rate that is barely enough for encoding to be fully accomplished before the next list item is presented and thus, does not allow for refreshing after encoding (i.e., when items are presented at a rate of 1 per 350 ms). In contrast, the current pattern is not in line with slower estimations of encoding speed (1000 ms and more per item; e.g., Bayliss, Bogdanovs, & Jarrold, 2015) because our results indicate that the last-presented item is no longer in the focus of attention 1000 ms after its onset (i.e., No delay conditions of Experiments 1, 3 and 5). Future studies will have to provide a more fine-grained test of how the last-presented benefit as an index for refreshing relates to proposed estimates of encoding speed and refreshing speed (50 ms per item; Camos & Barrouillet, 2014; Vergauwe & Cowan, 2014; Vergauwe et al., 2014).

Still, the current pattern of findings is promising when it comes to the use of the last-presented benefit as a direct, independent index of whether refreshing has occurred. This index might help in pitting decay-based accounts of forgetting against other accounts of short-term forgetting. For example, it might be used in further studies to directly examine how the processes that are assumed to counteract decay and interference relate to each other. Whereas decay-based forgetting is assumed to be counteracted by refreshing, interference is assumed to be counteracted by removing the representations that are causing interference with the to-be-remembered information (e.g., Ecker, Lewandowsky, et al., 2011),
2014; Oberauer, et al., 2014; Oberauer & Lewandowsky, 2016; Oberauer et al., 2012). Even though these processes were proposed within opposing theoretical frameworks, their existence is not necessarily mutually exclusive. Ecker, Lewandowsky, et al., (2014), Ecker, Oberauer, et al. (2014) have examined the local effects of removal and have provided an index of whether removal has taken place; the current study proposes an analogue for refreshing. Future studies can combine these indexes within a single study. Moreover, future studies could also examine to what extent the current index of refreshing relates to other effects assumed to rely on refreshing (e.g., cognitive load-effect, refreshing-frequency effect, secondary RT-set size), and to what extent these indexes predict WM capacity. While a direct link between refreshing and WM capacity has been proposed (e.g., Barrouillet & Camos, 2012; Vergauwe & Cowan, 2014), with refreshing efficiency predicting WM capacity, there is currently no direct evidence for that claim.

Finally, the current results also shed light on why some studies have observed a last-presented benefit in the Sternberg item-recognition task, while others have not (e.g., Buitows & Okada, 1971; Clifton & Birenbaum, 1970; Donkin & Nosofsky, 2012; Nee & Jonides, 2008; Öztok et al., 2010). Donkin and Nosofsky (2012) hinted at the possibility of maintenance processes taking place during and after list presentation, potentially altering the psychological recency of the memory items. This might have resulted in no last-presented benefit in studies with slower presentation rates and/or the presence of a retention interval. Our findings confirm this hypothesis and indicate that the maintenance process involved in the disappearance of the last-presented benefit is likely to be refreshing, rather than verbal rehearsal. This does, however, not imply that verbal rehearsal plays no role in WM. Even though our main results were replicated under articulatory suppression with similar strength of evidence, we did observe lower probe accuracy and slower probe RTs under articulatory suppression. This is consistent with the idea that verbal rehearsal plays a role in WM, an assumption that is not incompatible with our interpretation of the main results.

To conclude, the current study has revealed evidence for (1) the presumed local effect of refreshing that is heightened accessibility of the just-refreshed item, (2) the use of speeded responses to WM probes as a direct, independent index of the occurrence of refreshing, and (3) spontaneous occurrence of refreshing of to-be-remembered information during slow list presentation and during an empty delay following list presentation. These findings advance our understanding of the process of refreshing and future research can build on the current findings to address the debate concerned with the causes of short-term forgetting.

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A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.jml.2017.05.001.

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