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Discourse Markers Activate Their, *Like*, Cohort Competitors

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ABSTRACT

Speech in everyday conversations is riddled with discourse markers (DMs), such as *well, you know*, and *like*. However, in many lab-based studies of speech comprehension, such DMs are typically absent from the carefully articulated and highly controlled speech stimuli. As such, little is known about how these DMs influence online word recognition. The present study specifically investigated the online processing of DM *like* and how it influences the activation of words in the mental lexicon. We specifically targeted the cohort competitor (CC) effect in the Visual World Paradigm: Upon hearing spoken instructions to "pick up the beaker," human listeners also typically fixate—next to the target object—referents that overlap phonologically with the target word (cohort competitors such as *beetle*; CCs). However, several studies have argued that CC effects are constrained by syntactic, semantic, pragmatic, and discourse constraints. Therefore, the present study investigated whether DM *like* influences online word recognition by activating its cohort competitors (e.g., *lightbulb*). In an eye-tracking experiment using the Visual World Paradigm, we demonstrate that when participants heard spoken instructions such as "Now press the button for the, *like* . . . unicycle," they showed anticipatory looks to the CC referent (*lightbulb)* well before hearing the target. This CC effect was sustained for a relatively long period of time, even despite hearing disambiguating information (i.e., the /k/ in *like*). Analysis of the reaction times also showed that participants were significantly faster to select CC targets (*lightbulb*) when preceded by DM *like*. These findings suggest that seemingly trivial DMs, such as *like*, activate their CCs, impacting online word recognition. Thus, we advocate a more holistic perspective on spoken language comprehension in naturalistic communication, including the processing of DMs.

Introduction

Although spontaneously produced speech is riddled with discourse markers, such as *well, you know, I mean*, and *like*, they are typically absent from the controlled and carefully articulated speech materials used in many psycholinguistic studies. However, without a clear understanding of how such discourse markers are produced and processed, the study of spoken language comprehension is inherently limited. Therefore, the present article investigates how the discourse marker *like* influences online word recognition.

The use of *like* as a discourse marker (DM; also referred to as "collateral signal"; Fox Tree, 2006) is very common in many forms of colloquial English (D'Arcy, 2017), across a range of regional varieties (Andersen, 2001; Schweinberger, 2015; Tagliamonte, 2005), most frequently observed in the speech of young female speakers (Tagliamonte, 2005). DM *like* can be used as "a quotative marker,

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approximator, marker of exemplification, discourse link or hesitational device" (Andersen, 2001, p. 210). Like many DMs, it is syntactically flexible and nonobligatory: it can occur "between clause constituents, within phrases and between propositions" (Andersen, 2001, p. 210).

However, despite its widespread usage, little is known about how listeners process DM *like* in online word recognition. Most of the studies investigating the perception of DM *like* have focused on metalinguistic judgments or impressions, arguing that the use of DM *like* makes the speaker sound more friendly (Dailey-O'Cain, 2000) yet also less intelligent compared to controls (Fox Tree, 2006; Hesson & Shellgren, 2015). Similarly, overuse of DM *like* at job interviews reduces applicants' chances of success (Russell et al., 2008). The few studies on the *processing* of DM *like* show somewhat contradictory results. That is, Dall et al. (2015) used a Change Detection Paradigm to argue that DM *like* facilitates the memorability of following words. They presented participants with a short spoken passage, followed by an orthographic transcript of the passage and then asked participants to indicate whether the transcript was accurate or actually contained a substituted word. They found that detection rates of substituted words increased by about 15% if the substituted word had been preceded by a DM *like* compared to a fluent control (with *like* artificially excised). However, Liu and Fox Tree (2012) presented participants with an 8-minute spoken story and afterward tested participants' accuracy at answering comprehension questions for specific quantities that occurred in the story in two conditions: either preceded by *like* (e.g., " . . . charged me *like* 200 bucks") versus control (with *like* excised). They did not find statistical evidence that DM *like* enhanced memory recall, with similar accuracy for quantities preceded by *like* (77%) versus controls (73%).

In contrast to the paucity in studies of the processing of DM *like*, there is a considerably better grasp of the processing of another type of "collateral signal"—the filled pause *uh* (Ferreira & Bailey, 2004). Although these types of disfluencies have a negative effect on metalinguistic ratings of fluency (Bosker et al., 2013; van Os et al., 2020), they have been argued to actually *facilitate* online speech processing (Bailey & Ferreira, 2007). For instance, filled pauses can help listeners predict that the following referent is difficult to name, as evidenced by anticipatory looks to unknown or low-frequency referents in eye-tracking studies (Arnold et al., 2007; Barr & Seyfeddinipur, 2010; Bosker et al., 2014, 2019; Heller et al., 2014). They have also been argued to enhance listeners' attention to the following word based on EEG data (Collard et al., 2008). As a result, the encoding of these words in memory is facilitated as evidenced by greater accuracy recalling words that have been previously preceded by *uh* in surprise recognition memory tasks (Corley et al., 2007; MacGregor et al., 2010) and in the Change Detection Paradigm (Bosker et al., 2015; Dall et al., 2015). This also leads to faster reaction times (RTs) in word recognition tasks: Listeners are quicker to select a picture of, for instance, a camel in response to the spoken instruction "Now press the button for the *uh* camel, please" (compared to fluent control; Dall et al., 2014). Interestingly, the RT advantage of words preceded by filled pauses (vs. control) also holds for words preceded by silent pauses and even tones (Corley & Hartsuiker, 2011). This latter finding led Corley and Hartsuiker (2011) to propose the Temporal Delay Hypothesis. This account holds that any temporal delay facilitates word recognition because attention builds up over the course of the delay.

However, a critical distinction between filled pauses, such as *uh*, and discourse markers such as *like* is the fact that *uh* has very limited phonology: It is oftentimes merely a schwa. Discourse markers, on the other hand, share their phonology with content words in the English language (e.g., the verb *to like*). One consequence of the more elaborate phonology of discourse markers is that they thereby also have *cohort competitors* (Marslen-Wilson, 1987; Zwitserlood, 1989). Cohort competitors are words that share their initial sounds with the word of interest: *lightbulb, lifeboat*, and *lion* are all cohort competitors of DM *like* since they share the initial phonemes /laɪ/.

Since the advent of eye-tracking methodology in psycholinguistics, in particular the Visual World Paradigm (Cooper, 1974; Huettig et al., 2011; Tanenhaus et al., 1995), evidence has been accumulating that listeners incrementally map the sounds they hear onto possible lexical candidates. Specifically, it has been argued that listeners activate all words that are temporarily congruent with the phonetic input received, well before having heard the complete word (Marslen-Wilson, 1987; McClelland &

Elman, 1986). This is evidenced by *cohort competitor effects* in eye-tracking data (Allopenna et al., 1998). When viewing a scene with a beaker, a beetle, a speaker, and a carriage, and receiving spoken instructions to "pick up the beaker," listeners fixate both the beaker and beetle when they hear the initial sounds /bi-/ (before preferentially fixating the beaker when more phonetic input about the target referent comes in; Allopenna et al., 1998).

However, since the seminal work of Allopenna et al. (1998) in the past century, several studies have argued that cohort competitor effects are constrained by syntactic, semantic, and pragmatic constraints present in the sentence, the wider discourse, or in the larger communicative interaction (Magnuson, 2019; Tanenhaus et al., 1995). For instance, Magnuson et al. (2008) trained participants to recognize new "words" for different objects and textures from an artificial lexicon, including many cohort competitors (e.g., /tedu/ for a particular shape; /tedi/ and /tede/ for different textures; etc.). When hearing the instruction "Click on the tedi pibo" to select the /pibo/ object with the /tedi/ texture, participants showed the typical cohort competitor effect before disambiguation by the second vowel, fixating both the target object with /tedi /texture and another competing object with a /tede/ texture. However, listeners *did not* fixate a competing /tedu/ object, despite having similar phonological overlap with /tedi/. Magnuson et al. conclude that syntactic and pragmatic constraints can impact, and even completely preempt, phonological competition, as indexed by cohort competitor effects. That is, syntactic and pragmatic constraints appear to extinguish phonological competition across form classes.

More empirical evidence for constraints on cohort competitor effects is found in studies reporting that cohort competitor effects with a novel lexicon are modulated by action-based contingencies (Revill et al., 2008); cohort competition between *rug* and *run* depends on form classes in grammatically constrained sentences (e.g., "they thought about the . . . " vs. "they began to . . . "; Strand et al., 2018); cohort competition between Dutch *bok* 'goat' and *bot* 'bone' is modulated by semantic constraints (e.g., in "Never before climbed a /bɔk/ so high"; Dahan & Tanenhaus, 2004); and—compellingly—cohort competitor effects come and go in unscripted conversation between naïve participants as the referential context dynamically emerges and changes (Brown-Schmidt & Tanenhaus, 2008).

The present study investigates whether the discourse marker *like* activates its cohort competitors (*lightbulb, lion*, etc.). In our eye-tracking experiment using the Visual World Paradigm, we presented participants with visual displays containing four objects (see Figure 1), including one cohort competitor (e.g., the lightbulb), one matched unrelated control (e.g., unicycle), and two distractors. We

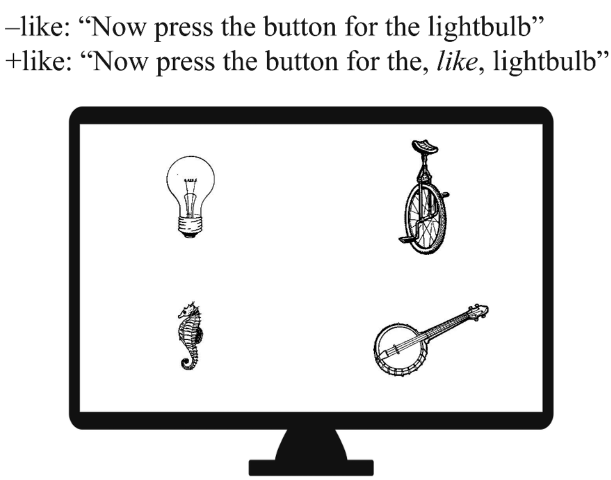

Figure 1. Example trial in the experiment. The visual display shows an example of an experimental trial where the target picture was a cohort competitor (*lightbulb*), the competitor picture was an unrelated picture (*unicycle*), and there were two distractors (*seahorse* and *banjo*). In the +like condition, the target referent was preceded by *like*, but in the –like condition it was not.

formulated two hypotheses about participants' behavior when hearing the spoken instruction "Now press the button for the, *like* ... unicycle." First, we predicted that, upon hearing *like*, participants would show anticipatory looks to the cohort competitor *lightbulb* well before hearing the target referent. This would provide evidence for activation of cohort competitors induced by encountering the discourse marker *like*, in line with Allopenna et al. (1998). Alternatively, cohort competition could be modulated by the word class of *like* (i.e., being a discourse marker; not a noun), hence reducing or even preempting activation of cohort competitors of *like*, in line with Magnuson et al. (2008).

If DM *like* does activate its cohort competitors, then one could even predict that the behavioral response to select the correct target (e.g., unicycle) is delayed. This would be indexed by slower RTs for *unicycle* when preceded by DM *like* versus when DM *like* was absent from the spoken instruction. Conversely, when the target is itself a cohort competitor of *like* (e.g., *lightbulb*) one might predict faster RTs to select this target object when it is preceded by DM *like* versus when it is not. However, these predictions are not set in stone, since few studies of cohort competitor effects report behavioral reaction times (but see Levy, 2014). Moreover, this prediction runs counter to the Temporal Delay Hypothesis that predicts faster RTs for target words preceded by DM *like*, irrespective of the phonological form of the target word. Therefore, although we predict clear cohort competitor effects in the looking behavior of our participants, our predictions regarding the RTs remain speculative.

Methods

Participants

Eighty-nine participants were recruited from the University of Edinburgh participant pool. Participants in all experiments reported in this study gave informed consent as approved by the PPLS Ethics Committee (no. 85–1819/5). Six participants did not complete a full experimental session; their incomplete data were excluded from analysis. Of the remaining 83 participants, 19 were highly proficient nonnative speakers of English with various L1 backgrounds. Their data were excluded from the following reported analyses; however, including their data did not qualitatively change the interpretation of results. The final data set included data from 64 native speakers of English (44 females, 20 males; age range = 18–30; 51 British English, nine American English, one Australian English, one Canadian English, two others).

Materials and design

We selected 16 *cohort competitor* referents: These words overlapped phonologically in their initial phonemes with the discourse marker *like*, such as *lightbulb, lime, lice* (i.e., all starting with /laɪ/). We also selected another set of 16 *unrelated* referents that did not have an /l/ as initial phoneme. These two sets were matched in terms of frequency of occurrence, age of acquisition, length of syllables, and length of phonemes (see Table A1 in the appendix). Lexical frequencies (in Zipf scores) and age of acquisition ratings were drawn from databases containing specifically British English norms (Kuperman et al., 2012; Van Heuven et al., 2014). Additional distractor referents were selected as distractors in experimental trials and as targets in filler trials (see the following description of lists). None of the distractor referents started with /l/. Simple line drawings of all referents were taken from the Severens database (Severens et al., 2005) or prepared in the same style.

A female native speaker of British English was recorded in a sound-attenuating booth, and her speech was digitally sampled at 44,100 Hz on a computer located outside the booth with Audacity software. The speaker produced all target words in two carrier sentences: once in the fluent carrier "Now press the button for the [target]" and once in the disfluent carrier "Now press the button for the, like, [target]." The speaker was instructed to produce the sentences in a natural and casual style—for instance, reducing the canonical form for *button* /ˈbʌ.tən/ to the more colloquial form /ˈbʌʔ.n̩/ with a glottal stop and a syllabic /n̩/. Thus, we aimed for naturalistic recordings that would license the use of the discourse marker *like*. From the disfluent recordings, henceforth referred to as the +like condition, we created the –like condition by digitally splicing out all speech before the onset of the target word and replacing it with the fluent carrier sentence using Praat (Boersma, 2001). This ensured that target words in the +like and –like condition were acoustically identical.

The average time from "Now" onset to target onset was 2,427 ms for the +like condition and 1,757 ms for the –like condition. For both conditions, the article *the* was always pronounced as "thuh", not "thee." In the +like condition, the average duration of *like* was 436 ms, with a mean fundamental frequency (F0) of 162 Hz, and it was followed by a brief pause of 188 ms on average.

Procedure

Participants were tested individually in a sound-attenuating booth. They were seated at a distance of approximately 80 cm in front of a 50.8 cm × 28.6 cm screen with a tower-mounted Eyelink 1000 eye-tracking system (SR Research) and listened to stimuli at a comfortable volume from two loudspeakers. Stimuli were delivered using OpenSesame version 3.2 software (Mathôt et al., 2012). Eye movements were recorded using pupil tracking (17 left; 47 right pupils) at a sampling rate of 500 Hz.

Each experimental session started with a cover story about the supposed goal of the experiment: Participants were told that the experiment was the second part of a study investigating the processing of visual and auditory information in real conversations. In the alleged first study, a set of speakers had had to give spoken instructions, and since this involved spontaneously recorded speech, participants were told that they should not worry if the speakers "fluffed their lines a bit." After this cover story, a familiarization phase was run in which participants were presented with each of the pictures used in the experiment in turn, together with the orthographic label for each picture that "their particular speaker" had selected in the purported first study. The order of pictures was randomized, and participants clicked through the pictures at their own pace. After the familiarization phase, we performed a nine-point calibration procedure followed by a validation procedure.

For the experiment proper, participants were instructed that their task was to listen to the stimuli and click with the computer mouse on one of four pictures presented on screen. A trial started with a preview of the pictures on screen for 2,000 ms (see Figure 1), followed by the presentation of the spoken instruction. Visual stimuli were presented centered in the four quarters of the screen, with the longest side of the pictures scaled to 300 pixels. The quarters in which the four pictures on screen appeared were randomized. At sound onset, the mouse pointer was presented in the middle of the screen, and participants moved the mouse pointer to the target picture. One thousand ms after selection of the picture, the trial ended, and an empty screen was presented for 100 ms, after which the next trial started. If no mouse click response was recorded within 6,000 ms of sound offset, the trial was recorded as a missing trial.

Four different lists were prepared, with an equal number of participants assigned to each list. List 1 included 64 trials in total, consisting of two blocks of 32 trials each. Half of the trials in a block (n = 16) involved experimental trials that included one cohort competitor picture, one matched unrelated picture, and two distractor pictures. In half of these experimental trials (n = 8), the cohort competitor was the target picture; in the other half, the unrelated picture was the target. Also, whether or not the spoken instruction included *like* or not was divided equally across experimental trials with cohort competitor targets versus unrelated targets. The other half of the trials in a given block (n = 16) involved filler trials where no cohort competitors were ever presented. None of the filler trials ever involved a +like condition, thus lowering the overall percentage of +like trials to 25%. The order of presentation of trials within a block was randomized.

The second block was identical to the first block, except that cohort competitor pictures were paired with another unrelated picture and different distractors. If the cohort competitor had been a target referent in the first block, it was a distractor in the second block and vice versa. To avoid participants learning over the course of the experiment, previously referred to cohort competitor pictures in block

1 would not be targets in block 2, and filler trials in block 2 included trials in which targets were repeated from block 1. List 2 was identical to List 1 except that whether or not the spoken instruction contained *like* was reversed throughout the list. Lists 3 and 4 were identical to Lists 1 and 2 except that whether or not a cohort competitor picture was a target or distractor was reversed throughout each list.

Participants received four practice trials, followed by the experimental trials in a random order. None of the practice trials included cohort competitor pictures, nor did any contain *like*. We recorded the locations and times of participants' mouse clicks, together with a record of their eye movements during each trial. After the experiment, participants were given a debriefing questionnaire, asking about their impression of the purpose of the experiment, the presumed identity of the speaker, and whether they noticed anything unusual about the stimuli. Finally, we revealed the true purpose of the experiment and that our research question was whether "participants consider a picture starting with 'lie' to become the target when they hear the speaker say *like*." When asked to earnestly indicate whether they were aware of this manipulation during the experiment, only four participants answered yes; one other participant replied that her suspicion only grew at the end of the experiment.

Results

Behavioral mouse clicks

Data from practice and filler trials were excluded from analysis. Behavioral accuracy on the experimental trials was high: On only six experimental trials (< 1%) did participants incorrectly click on a nontarget picture, and no time-out trials were recorded. The incorrect trials were excluded from further analyses, both in the RT analyses and in the eye fixation analyses. Reaction times of mouse clicks (in ms) were calculated from target word onset, and outliers were excluded (RTs outside 2 × *SD* from the mean; 2% of the data excluded); see Table 1.

The reaction times (RTs) were log-transformed and entered into a Linear Mixed Model (LMM; Baayen et al., 2008). The log-transformed RTs were predicted by the fixed factors *Like* Presence (categorical predictor; deviation coding, with –like coded as −0.5 and +like as +0.5), Target Category (categorical predictor; deviation coding, with unrelated targets coded as −0.5 and cohort competitor targets as +0.5), Trial Number (z-scored to improve model fitting), and all possible interactions (see Table 2). The use of deviation coding of two-level categorical factors (i.e., coded with −0.5 and +0.5) allows us to test main effects of these predictors, since with this coding the grand mean is mapped onto

Table 1. Mean (SD) reaction times (in ms) calculated from target word onset, separately for the factors Target Category and *Like* Presence.

		Like Presence	
		–like	+like
Target Category	Cohort Competitor	1,211 (290)	1,170 (297)
	Unrelated	1,201 (279)	1,202 (279)

Table 2. Outcomes of the statistical model of the log-transformed behavioral reaction times.

	β	*SE*	*t*	*p*
Intercept	7.063	0.022	323	**< 0.001**
Like Presence	−0.015	0.008	−1.847	0.065
Target Category	−0.012	0.022	−0.537	0.596
Trial Number	−0.018	0.008	−2.213	**0.030**
Like Presence × Target Category	−0.039	0.016	−2.413	**0.016**
Like Presence × Trial Number	−0.029	0.009	−3.320	**< 0.001**
Target Category × Trial Number	0.034	0.013	2.565	**0.013**
Like Presence × Target Category × Trial Number	−0.004	0.017	−0.247	0.805

Note. Statistically significant *p* values are marked in bold.

the intercept. The LMM included random intercepts for Participants and Targets and by-participant and by-target slopes for all fixed factors, following Barr et al. (2013). Statistical significance was assessed using the Satterthwaite approximation for degrees of freedom, as implemented in the package lmerTest in R (version 3.1–0; Luke, 2017).

We observed an interaction between *Like* Presence and Target Category, demonstrating that when there was a *like* in the spoken instruction, participants were faster to click on the target if it was a cohort competitor. The main effect of Trial Number indicated that participants produced faster responses over the course of the experiment. Interactions with Trial Number were also observed: The two-way interactions between Trial Number and *Like* Presence, and between Trial Number and Target Category, indicated increasingly faster responses for trials with DM *like* and for trials with unrelated targets. Finally, no three-way interaction was observed, suggesting that the faster RTs for cohort competitor targets if there was a *like* in the spoken instructions were relatively stable across the experiment. As such, the combined outcomes suggest that hearing the discourse marker *like* speeds up recognition of a following cohort competitor of *like*, such as *lightbulb*.

Eye fixations

Areas of interest (AOI: target, competitor, distractor 1, distractor 2) were defined as 300 × 300 pixels around the center of each picture. Analyses focused on fixations to the cohort competitor and unrelated pictures, since the two distractors were never targets in any of the experimental trials. Prior to analysis, blinks were excluded from the data. Proportions of fixations on the AOIs were calculated over bins with a width of 20 ms.

We hypothesized that upon hearing DM *like*, participants would show more fixations on cohort competitor pictures (e.g., *lightbulb*) compared to unrelated pictures (e.g., *unicycle*). Therefore, we ran a confirmatory analysis on a time window that showed participants' gaze behavior in response to hearing *like*. This pretarget time window was defined from *like* onset (i.e., on average 633 ms before target word onset) + 200 ms (to correct for oculomotor delay) until target word onset + 200 ms (i.e., −433 ms to 200 ms from target onset; see gray window in Figure 2). Note that the use of this time window entailed that we compared looking behavior in response to different lexical content in trials

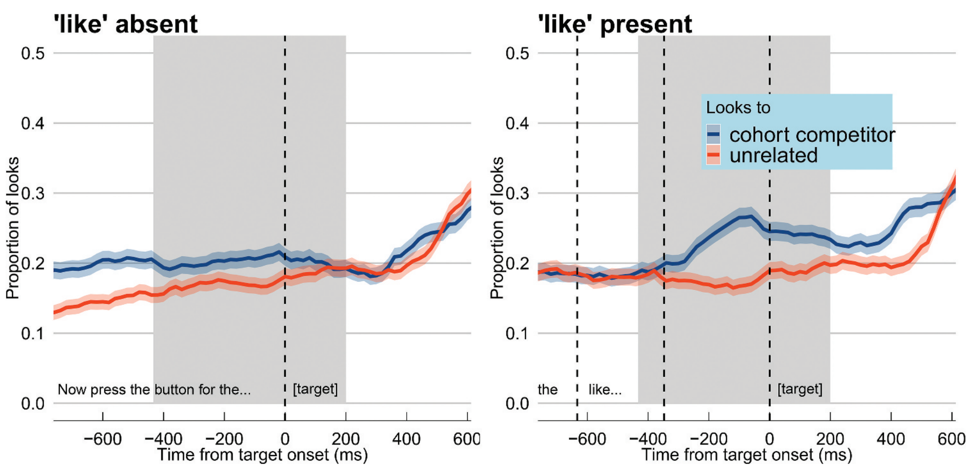

Figure 2. Proportion of looks to cohort competitor referents vs. unrelated referents. The proportion of looks to cohort competitor pictures (e.g., *lightbulb*; in blue) and unrelated pictures (e.g., *unicycle*; in red), separately for trials with *like* absent (−like; left panel) and trials with *like* present (+like; right panel). Note that different lexical content was heard in the −like and +like conditions. The dashed vertical lines indicate average *like* onset (at −633 ms; in right panel only), average onset of *like*'s word-final /k/ (at −347 ms; in right panel only), and target onset (at 0 ms). The gray rectangle indicates the time window of interest as identified for the statistical analyses. Ribbons around the lines indicate global standard errors on either side.

with *like* as compared to trials without *like*. This also meant that instead of analyzing fixations to target pictures, as is typical in visual world paradigm analyses, we compared the proportions of fixations to the cohort competitor picture (e.g., *lightbulb*) to the proportions of fixations to the unrelated picture (e.g., *unicycle*). Visual inspection of the looking behavior on trials with *like* (+like; right panel in Figure 2) seems to show anticipatory preferential looking to the cohort competitor picture (e.g., *lightbulb*) in response to hearing *like*: The blue line deviates from the red line at the onset of the critical time window.

The anticipatory preference for cohort competitor pictures in participants' looking behavior in the critical time window was quantified by, first, calculating logit values for the fixation proportions (logit values of 1 and 0 were replaced by logit[0.99] and logit[0.01] to avoid infinite values) and, second, by subtracting the looks to the unrelated pictures from the looks to the cohort competitor pictures (in logit space). This resulted in the measure Cohort Competitor Preference, which formed the dependent variable for the statistical analysis: Positive values indicated a preference to fixate the cohort competitor object more than the unrelated object; negative values indicated the reverse preference.

For our confirmatory analysis, the dependent variable Cohort Competitor Preference was entered into an LMM (see Table 3) with the predictor *Like* Presence (categorical predictor; deviation coding, with –like coded as −0.5 and +like as +0.5), Time Bin (index of fixation behavior over time within a trial; bin size = 20 ms), Trial Number (*z*-scored to improve model fitting), and all their interactions. In addition, we included the predictor Lag that contained the Cohort Competitor Preference in the previous time bin. This Lag predictor addressed the autocorrelated nature of our eye-gaze data: Fixations last much longer than a single sample, so the probability of a fixation on picture *x* at sample *s* is conditional upon whether picture *x* was fixated at sample *s-1* (Cho et al., 2018). Finally, the LMM included random intercepts for Participants and Targets and by-participant and by-target slopes for all fixed factors, following Barr et al. (2013). Statistical significance was assessed using the Satterthwaite approximation for degrees of freedom, as implemented in the package lmerTest in R (version 3.1–0; Luke, 2017).

We observed a main effect of *Like* Presence, suggesting that there was an anticipatory preference to fixate the cohort competitor referent when there was a *like* in the spoken instruction, corroborating our predictions. Furthermore, the negative estimate of the main effect of Trial Number indicated that participants increasingly fixated the unrelated object over the course of the experiment. Interestingly, no interaction between *Like* Presence and Trial Number was observed. We did observe an interaction between *Like* Presence and Time Bin, demonstrating that the effect of *Like* Presence decreased with time in the time window of interest. Finally, the LMM also showed an effect of Lag, confirming the autocorrelated nature of our data.

The combined outcomes suggest that hearing the discourse marker *like* led to an anticipatory preference to fixate a cohort competitor of *like*, such as *lightbulb*, before target onset. Note that the present cohort competitor effect in the eye-tracking data seems to be quite robust. First, we did not find evidence for modulation of the effect over the course of the experiment. Second, the effect seems to be quite extended in time. That is, in the original Allopenna et al. (1998) study, in a trial with *beaker* as target and *beetle* as cohort competitor, fixations to the cohort competitor increased from target

Table 3. Outcomes of the statistical model on the eye gaze behavior before target onset.

	β	*SE*	*t*	*p*
Intercept	−0.007	0.012	−0.631	0.533
Like Presence	0.030	0.014	2.159	**0.037**
Trial Number	−0.019	0.009	−1.999	**0.049**
Time Bin	−0.004	0.012	−0.341	0.734
Lag	0.971	0.001	794	**< 0.001**
Like Presence × Trial Number	−0.009	0.012	−0.796	0.426
Like Presence × Time Bin	−0.023	0.011	−2.112	**0.035**
Like Presence × Trial Number × Time Bin	0.010	0.008	1.178	0.239

Note. Statistically significant *p* values are marked in bold.

onset but started to deviate (i.e., drop) from target fixations from target offset onwards (see their Figure 4). However, in the present data, participants still seem to preferentially fixate cohort competitors (e.g., *lightbulb*) well after the offset of the DM *like*, and even well after target onset.

To further investigate the extent of the cohort competitor effect, we additionally analyzed fixation behavior after target onset as an exploratory analysis. Because, in this time window, participants are exposed to phonetic information about the target, we split the data for trials where the target was a cohort competitor (e.g., lightbulb) vs. an unrelated target (e.g., unicycle).

In the right panel of Figure 3, showing the +like condition, when participants hear a spoken instruction with DM *like*, it seems that they already preferentially fixate the cohort competitor at target onset. This corroborates the findings from the confirmatory analysis in the earlier pretarget time window. Moreover, in trials with an unrelated target, this cohort competitor preference only dissipates after approximately 300 ms after target onset (when the solid and dashed red lines diverge)—despite hearing disambiguating information about DM *like* from 347 ms before target onset (i.e., the onset of the /k/ sound in *like*; see right panel in Figure 2). Finally, it seems that participants recognize the cohort competitor faster in the +like condition than an unrelated target. That is, the red solid line (showing target fixations for unrelated targets) only converges with the blue solid line (showing target fixations for cohort competitor targets) after about 600 ms after target onset.

If the effect of DM *like*, observed previously, indeed extends well beyond target onset, then in our exploratory analysis we should be able to find statistical evidence that the difference in target fixations in +like versus –like conditions was greater for cohort competitor targets, compared to unrelated targets, even after target onset. We used the Bootstrapped Differences of Timeseries approach (BDOTS; Seedorff et al., 2018) to quantify this evidence because it has been designed specifically for the type of data currently under analysis (densely sampled timeseries obtained from eye-tracking; Oleson et al., 2017).

We used the BDOTS approach that is implemented in the R package BDOTS (version 0.1.19; Seedorff et al., 2018). It allows for statistical assessment of when in time a particular effect of interest can be reliably detected. As such, it is very suitable for the exploratory analysis at hand, asking whether any reliable lingering evidence for effects of DM *like* can be found after target onset. Here we evaluated target fixations as a difference between the +like and –like conditions, asking if this "*Like* Advantage"

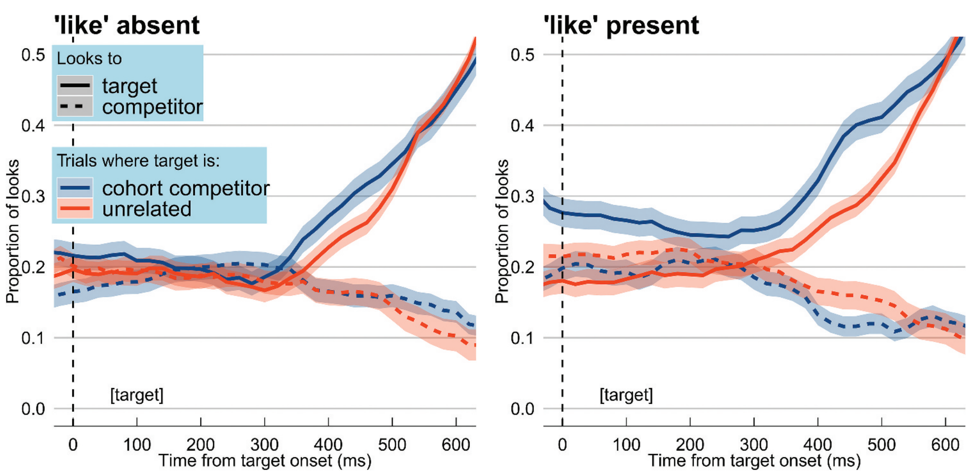

Figure 3. Proportion of looks to target vs. competitor objects. The proportion of looks to target objects (solid lines) and competitor objects (dashed lines) from target onset onwards for trials where the target was a cohort competitor (e.g., *lightbulb*; in blue) vs. an unrelated referent (e.g., *unicycle*; in red). Data are plotted separately for trials with *like* absent (–like; left panel) and trials with *like* present (+like; right panel). Note that, in this target window, identical acoustic content was heard in the –like and +like conditions. The dashed vertical line indicates target onset (at 0 ms). Ribbons around the lines indicate global standard errors on either side.

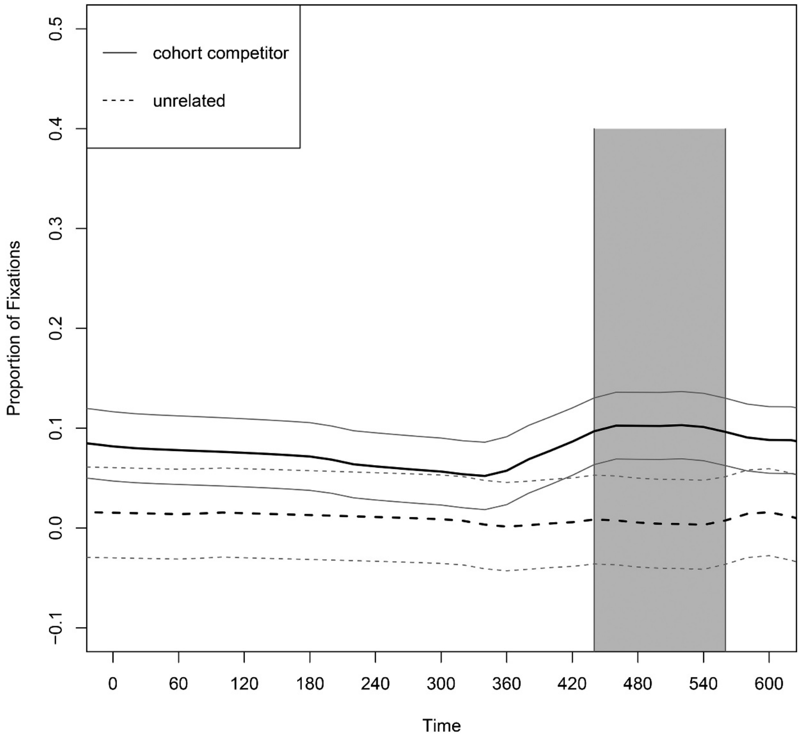

Figure 4. Difference curves in the BDOTS analysis after target onset. The curves show the difference in target fixations between the +like and –like conditions (higher values indicate a greater proportion of target fixations in +like trials), separately for trials with a cohort competitor target (solid lines) vs. trials with an unrelated target (dashed lines). Thick lines show the bootstrapped means and the thin lines the bootstrapped standard deviations. The gray time window indicates a time period of statistically significant difference between the two difference curves, from 440–560 ms after target onset.

differed significantly between the two Target Categories (cohort competitor vs. unrelated targets). BDOTS asks specifically at which time points these "difference curves" reliably differ (see Figure 4).

The BDOTS analyses started by fitting a four-parameter logistic function to individual participants' patterns of target fixations in the +like and –like conditions, separately for trials with cohort competitor versus unrelated targets (in a large time window from −100 to 1,500 ms from target onset to help the fitting). This helps to smooth the data, reducing the influence of idiosyncratic patterns of significance on the outcomes. This fitting stage involved visual comparison of fitted curves to observed data and subsequent refitting. In this fitting stage, 17 of the 256 logistic curves (<7%) were dropped due to poor fitting. The remaining 239 curves were fitted assuming autoregressive error (AR1; $n = 225$) or without AR1 if better fits could be obtained without it ($n = 14$). From these fits, standard errors of the mean and confidence intervals were estimated using bootstrapping. Finally, using these estimates, we conducted pairwise comparisons at each time to determine regions of difference in the difference curves, with a family-wise error adjustment with a modified Bonferroni corrected significance level that takes into account the autocorrelation among t-statistics (Seedorff et al., 2018). Autocorrelation of the t statistics was 0.951; the adjusted alpha was calculated to be 0.0023. This exploratory analysis demonstrated that a reliable difference was detected between 440 ms and 560 ms after target onset. This suggests that hearing *like* in the spoken instructions significantly facilitated target recognition even well after target onset, if the target was a cohort competitor of *like*. This finding corroborates the findings from the RT analysis presented previously.

General discussion

This is the first empirical study to assess the online processing of the discourse marker *like*. Specifically, it addresses the question whether DM *like* activates its cohort competitors, such as *lime, lifeboat, lion*, etc. We demonstrate that when people hear DM *like*, they activate its cohort competitors, as evidenced by anticipatory looks to cohort competitors upon hearing DM *like* (i.e., well before target onset). Interestingly, since the seminal study on cohort competition by Allopenna et al. (1998), several empirical studies have argued that cohort competitor effects are modulated—or even completely preempted—by syntactic, semantic, and pragmatic constraints in the discourse (Magnuson, 2019; Magnuson et al., 2008; Tanenhaus et al., 1995). They showed, for instance, that cohort competitor effects are reduced across word classes (e.g., nouns vs. verbs; no looks to *rug* when hearing "they began to run"; Dahan & Tanenhaus, 2004; Strand et al., 2018). Our findings suggest that DM *like* does activate its cohort competitors in a different word class (namely, nouns). This may be explained in terms of tolerant syntactic constraints: Since DM *like* is syntactically flexible, it can appear in the same position as nouns (here: after the article *the*). This proposal would predict that when more stringent syntactic constraints apply (e.g., hearing "I was gonna *like* ... "), cohort competitor effects induced by DM *like* may be reduced or eliminated, presenting a topic for future research.

One interesting constraint for future investigation is prosody. Recent research has argued that prosodic cues—for instance, intonation or speech rate—can guide listeners' expectations (Kaufeld, Naumann et al., 2020; Kaufeld, Ravenschlag et al., 2020; Kurumada et al., 2014). Our speaker produced DM *like* with naturalistic prosody, yet perhaps more pronounced prosodic cues to *like* being used as a discourse marker (e.g., decrease in fundamental frequency; slower speech rate; Drager, 2011; Schleef & Turton, 2018) could modulate the extent to which cohort competitors are activated in online word recognition.

However, note that the cohort competitor effect we observed was robust and stable in nature. First, we did not observe any statistical evidence for modulation of the effect after 64 trials with a *like* incidence of 25%. Second, the cohort competitor effect persisted for a relatively long time. That is, when hearing "Now press the button for the, *like* ... unicycle," the preference to look at the cohort competitor *lightbulb*—induced by DM *like*—could still reliably be detected until 560 ms after target onset. This suggests that clearly disambiguating phonetic information (i.e., all speech after the onset of /k/ in *like*) only reduced the cohort competition at a relatively late point in time. The robust nature of the effect would potentially also limit the modulating power of any early prosodic cues to reduce the cohort competition.

The extended nature of the cohort competition effect, observed in the eye-gaze data, was further corroborated by the analysis of participants' RTs. We found that hearing DM *like* speeds up recognition of a following cohort competitor of *like*, such as *lightbulb*. Interestingly, this outcome contrasts with the Temporal Delay Hypothesis (Corley & Hartsuiker, 2011) predicting that any temporal delay, including DM *like*, would speed up word recognition, irrespective of the target word. We do not believe that this is a principal distinction between DM *like* and other temporal delays in speech, such as disfluencies. In fact, other studies have also failed to substantiate the Temporal Delay Hypothesis for disfluencies, such as filled pauses (Bosker et al., 2014, 2019; Fox Tree, 2001; Wester et al., 2015). Although the present study was not designed for this purpose, future work could further assess and compare the processing of such "collateral signals" as *like* and disfluencies by testing, for instance, whether DM *like* also triggers the prediction of more complex referents, much like filled pauses do (Bosker et al., 2014, 2019).

The combined outcomes of the present study demonstrate that DMs, such as *like*, can activate particular lexical forms based on their own phonology, with consequences for the behavioral speed and efficiency of word recognition. Critically, this observation distinguishes "lexical" DMs, with their own specified phonological form, from disfluencies, such as silent and filled pauses that do not share their phonology with (many) words. This does not mean that disfluencies cannot induce cohort competition effects; in fact, in many of the world's languages, speakers use demonstratives as filled pauses (e.g., *eto* 'this' in Japanese; *zhè ge* 'this' in Mandarin). Our findings obtained with DM *like* likely generalize to disfluencies in other languages where lexical fillers with more elaborate phonology are much more common.

Finally, the present work opens up new avenues for follow-up research to further pinpoint the online processing of DMs. For instance, is the online processing of DMs modulated by talker-specific knowledge about a particular individual's use of DMs (see Van Bergen & Bosker, 2018)? If Talker 1 is a fervent user of DM *like*, does exposure to this talker decrease the cohort competition effect for that particular talker? Thus, the study of language processing will move forward toward a more naturalistic understanding of spoken communication in everyday conversation.

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Data availability statement

The experimental data of this study are available for download from https://osf.io/rmj4e under a CC BY-NC-ND 4.0 license.

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Appendix

Table A1. List of cohort competitor referents and unrelated referents. Lexical frequencies (freq; in Zipf scores) and age of acquisition ratings (AoA; in years) were drawn from British English norms (Kuperman et al., 2012; Van Heuven et al., 2014). *n/a* indicates missing data.

	Referents	Category	Freq	AoA	*n* Syllables	*n* Phonemes
1	*Lineup*	Cohort competitor	2.20	8.42	2	5
2	*Lifejacket*	Cohort competitor	2.54	*n/a*	3	8
3	*Lightbulb*	Cohort competitor	2.68	5.89	2	7
4	*Lice*	Cohort competitor	3.29	*n/a*	1	3
5	*License*	Cohort competitor	3.40	8.70	2	6
6	*Limestone*	Cohort competitor	3.62	12.00	2	7
7	*Lifeboat*	Cohort competitor	3.70	7.72	2	6
8	*Livestock*	Cohort competitor	3.75	7.89	2	7
9	*Lighthouse*	Cohort competitor	3.77	6.11	2	6
10	*Lightning*	Cohort competitor	4.00	4.76	2	6
11	*Lime*	Cohort competitor	4.09	6.61	1	3
12	*Lighter*	Cohort competitor	4.13	7.12	2	4
13	*Library*	Cohort competitor	4.34	4.95	2	5
14	*Lion*	Cohort competitor	4.45	4.42	2	4
15	*Line*	Cohort competitor	5.45	4.85	1	3
16	*Lightswitch*	Cohort competitor	*n/a*	5.94	2	7
17	*Horseshoe*	Unrelated	3.25	5.85	2	5
18	*Paperclip*	Unrelated	2.55	6.42	3	8
19	*Unicycle*	Unrelated	2.71	7.26	4	8
20	*Hinge*	Unrelated	3.26	7.95	1	4
21	*Skateboard*	Unrelated	3.39	6.94	2	7
22	*Ruler*	Unrelated	3.63	5.94	2	4
23	*Wagon*	Unrelated	3.73	5.22	2	5
24	*Sailor*	Unrelated	3.75	6.44	2	4
25	*Pumpkin*	Unrelated	3.79	4.78	2	7
26	*Organ*	Unrelated	3.99	8.72	2	4
27	*Artichoke*	Unrelated	3.13	10	3	7
28	*Hanger*	Unrelated	3.13	6.78	2	5
29	*Watermelon*	Unrelated	3.13	4.22	4	9
30	*Radish*	Unrelated	3.22	5.25	2	5
31	*Horseshoe*	Unrelated	3.15	7.78	2	8
32	*Paperclip*	Unrelated	3.17	5.06	2	6