Referential precedents in spoken language comprehension: A review and meta-analysis

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Abstract

Listeners' interpretations of referential expressions are influenced by established referring precedents—temporary conventions for making linguistic reference to entities in the discourse. A number of psycholinguistic studies have sought to determine the extent to which precedent effects reflect the use of common ground (information that is known to be shared) versus more egocentric, domain-general processes. We provide a review and meta-analysis of visual-world eyetracking studies of precedent use, focusing on three principal effects: (1) the same-speaker advantage for maintained precedents; (2) the different-speaker advantage for broken precedents; and (3) the main effect (partner-independent) advantage. Our meta-analysis suggests distinct temporal profiles for these effects, which are generally consistent across studies, despite wide variation in methodology. Our findings challenge explanations of precedent use in terms of common ground, as this factor explains only for a minor portion of the phenomenon. Explaining the full set of phenomena is likely to require looking beyond common ground to a variety of domain general mechanisms related to memory and language processing.

Keywords: PERSPECTIVE-TAKING, META-ANALYSIS, PRAGMATICS, CONVERSATION, EYE TRACKING

Referential precedents in spoken language comprehension: A review and meta-analysis

One of the central questions in research on spoken language communication concerns how listeners derive context-specific interpretations from the linguistic form of spoken utterances. Explaining how this derivation works is challenging because of there is a many-to-many mapping between linguistic utterances and a given speaker's communicative intention in a particular setting. This complexity is evident in referential expressions such as *the small candle*, which could be used by a speaker to refer to many different individual candles of varying size. The linguistic content of such an utterance provides information only about the type of entity that is being referred to, leaving it up to the listener to decipher which particular token the speaker has in mind. How do listeners identify relevant contextual information, and how is this information brought to bear on language processing?

Given that understanding a speaker's intended meaning is paramount, some theorists have argued for a central role of mutually shared information or *common ground* (Clark & Carlson, 1981; Clark & Marshall, 1981). Common ground is a special kind of shared information that is not only shared, but is also known (or believed) to be shared. According to this view, listeners employ various *co-presence heuristics* to identify common ground, which include *perceptual co-presence*, *linguistic co-presence*, and *community membership*. For example, listeners can know that a particular candle is the referent of *small candle* because (1) it is the smallest candle that the interlocutors can see (perceptual co-presence); (2) it was previously mentioned in the current discourse in the presence of the current interlocutors (linguistic co-presence); or (3) the interlocutors are members of a community in which this particular candle is particularly salient (community membership).

An early view proposed that listeners resolve referential ambiguity by restricting the information they consider to their common ground with the speaker (Clark & Carlson, 1981). However, studies have failed to support this strong view (Keysar, Barr, Balin, & Brauner, 2000), and some have presented evidence that the effect of common ground is

probabilistic and partial rather than deterministic and absolute (Hanna, Tanenhaus, & Trueswell, 2003; Nadig & Sedivy, 2002). In other words, knowing that certain information is in common ground makes this information more likely to be used, without imposing an absolute restriction. This view is influenced by the more general *constraint-based view* of language comprehension, which regards interpretation as a constraint-satisfaction process, in which multiple cues are probabilistically weighted, with those interpretations that best satisfy the combination of cues winning out (MacDonald, Pearlmutter, & Seidenberg, 1994; Tanenhaus & Trueswell, 1995). Within this framework, common ground is but one of multiple cues integrated simultaneously during processing. This view assumes that listeners actively track common ground, and that its effect on interpretation will be proportional to its contextual salience and reliability. Furthermore, this view assumes no limit on listeners' ability to integrate this information with incoming speech at any stage of processing. Thus, common ground should modulate language processing routinely, and should do so from the earliest moments of comprehension.

An alternative view assumes a more peripheral role for common ground in referential interpretation, claiming that interpretation is based on certain strategies and heuristics that are deployed largely egocentrically, i.e., without reference to the beliefs and intentions of the speaker (Keysar et al., 2000; Pickering & Garrod, 2004). Successful interpretation may be the result of ordinary memory processes such as episodic priming and encoding specificity (Barr, Jackson, & Phillips, 2014; see also Horton & Gerrig, 2005). They may also reflect the deployment of heuristics that exploit regularities in language use, such as the association between discourse-novel expressions and discourse-novel referents (Kronmüller & Barr, 2007) or the use of a pronoun to refer to an entity that is highly salient (Fukumura & van Gompel, 2012). In short, this approach views reference resolution as involving an eclectic set of domain-general systems whose operations have been adapted to the demands of conversation. It does not rule out the possibility of common ground modulating interpretation, but assumes that it plays a limited role, with some proponents

arguing that situational assessments do not constrain certain low-level linguistic processes such as lexical access (Barr, 2008b). This view predicts that effects of common ground will emerge late in processing, if at all.

One way researchers have sought to distinguish these views is by investigating the nature of discourse effects on reference resolution. How do listeners make use of the conversational history of referring—the discourse record—to resolve references? Forming a key part of the discourse record are the temporary referring conventions that interlocutors establish during a conversation, known as *referential precedents* (or conceptual pacts; Brennan & Clark, 1996). When speakers introduce a referent into discourse, they choose from among a potentially wide variety of options provided by the language, each encoding a different conceptualization of the same referent (Brown, 1958). For example, the same object could be called a dime, a coin, money, a metal object, etc. Because language is a cooperative activity, settling upon a particular way of describing a referent creates an expectation that the interlocutors will continue referring to that referent in the same way throughout the discourse. Furthermore, it causes them to also expect that they will not use the same expression to refer to a different referent, as that would risk confusing the listener. Referential communication studies show that speakers, by and large, fulfill these expectations (Brennan & Clark, 1996; Gann & Barr, 2014; van der Wege, 2009).

Referential precedents not only reduce linguistic variability, making speech more predictable, but also provide a powerful basis for reducing ambiguity in reference resolution. When speakers repeat a description they used earlier in the discourse, they are likely to be referring to the same thing they referred to on the last occasion they used that description. Likewise, when speakers use a description they have not used before, it is likely they are referring to something that has not yet been mentioned. Listeners indeed have these expectations about speaker consistency, and these expectations powerfully influence comprehension (Barr & Keysar, 2002; Metzing & Brennan, 2003). But to what extent does this influence reflect the probabilistic use of common ground versus more

egocentrically-based memory phenomena?

To address this key question, researchers have examined the *speaker specificity* of referential precedent use—that is, the extent to which the effect of a precedent depends on whether or not the precedent was established by the current speaker or by a different speaker. Studies typically involve listeners who interact with two different speakers, establishing different interactional histories with each speaker and thus different knowledge of established precedents. Listeners are given reason to believe that neither speaker is aware of the other speaker's precedents. To the extent that precedents are speaker specific, precedents should have a stronger influence with the speaker who established them; to the extent they are speaker independent, the influence should be independent of the identity of the speaker.

Studies have looked at two main cases of precedent use: maintained and broken precedents. Speakers are said to maintain a precedent when they re-use an expression they have already established as a means for referring to a given referent; for example, continuing to refer to a particular car as *the sportscar*. To the extent listeners expect speakers to be consistent, they should benefit from this repetition. To the extent that precedent use is speaker-specific, this benefit should be greater when the speaker who repeats the expression is the one who established it precedent in the first place, a prediction we refer to as the *same speaker advantage for maintained precedents*. Speakers are said to break a precedent when they refer to something using an expression that differs from how they referred to it before; for example, referring to the same *sportscar* as the *Ferrari*. Breaking a precedent violates listeners' expectations, and thus should elicit confusion and delay identification of the intended referent. To the extent precedent use is speaker-specific, breaking a precedent should be most confusing when the person who breaks it is the same speaker who established it in the first place. While it would be perfectly expected that different speakers might independently arrive at different ways of describing a referent, under most circumstances it is uncooperative for a speaker to change

how they refer to something. Thus, broken precedents should be understood more easily when the precedent is not in common ground with the current speaker, an effect we refer to as the *different speaker advantage for broken precedents*.

Despite considerable work testing the prediction of speaker specificity for maintained and broken precedents, substantial disagreement remains, with the body of data failing to fully support either absolute specificity or absolute independence. Early claims of complete speaker independence (Barr & Keysar, 2002) have gone unsupported by later studies (Metzing & Brennan, 2003; Brown-Schmidt, 2009a), which find speaker specific effects under some circumstances. What is therefore critical to understand is not whether such effects exist at all, but when they exist during online processing: are they present from the earliest moments of comprehension, as constraint-based models predict, or do they emerge late, as more egocentric models predict?

Researchers have sought to explain the apparent heterogeneous findings in the literature in terms of differences in experimental methodology and data analysis. In this paper, we present results from a meta-analysis that challenges the assumption of heterogeneous findings, revealing instead a surprisingly uniform set of phenomena. Our analysis, which combined results from 10 published experiments using visual-world eyetracking (Cooper, 1974; Tanenhaus, Spivey, Eberhard, & Sedivy, 1995), detected three main patterns, namely: (1) that the same speaker repetition benefit for maintained precedents arises from the earliest moments of comprehension; (2) that the different speaker novelty benefit for broken precedents emerges late, well after the onset of referential processing; and finally (3) that the lion's share of the influence of precedents on comprehension reflects speaker-independent (and thus, egocentric) processing. Although these results do not definitively support any existing theoretical position on partner-adaptation in language comprehension, they offer important constraints on theorizing, and point the way toward more definitive future studies. In the next section, a review of the literature on precedent use in comprehension sets the stage for our analysis.

Referential precedents and expectations of consistency

Our review focuses on studies of spoken language comprehension using the visual-world eyetracking paradigm with adult populations, as these studies provide the clearest, most detailed picture of moment-by-moment processing in the typically developing adult. Studies of precedent use in child populations (Graham, Sedivy, & Khu, 2014; Matthews, Lieven, & Tomasello, 2010) have generally yielded similar results to those we consider below. In addition, we excluded from our analysis studies using neuroimaging (Bögels, Barr, Garrod, & Kessler, in press) or that examine long-term naming conventions established outside of the experimental laboratory, such as conventions for proper names (Barr et al., 2014). Where relevant, we describe the results of these studies during our review of the main visual-world eyetracking studies on discourse conventions with adults.

The visual-world eyetracking paradigm (Cooper, 1974; Tanenhaus et al., 1995) is an approach to studying language processing that is well-suited to the study of precedent use. This approach enables the measurement of listeners' moment-by-moment comprehension in conversational settings in a relatively unobtrusive manner. In a typical visual-world eyetracking study, listeners' gazes are monitored while they participate in a collaborative task that requires them to follow a speaker's spoken instructions to manipulate objects, which can either be actual physical objects located in a display apparatus, or pictures of objects shown on a computer monitor. On a typical trial, the speaker will mention a particular intended referent or *target object*, such as *the small candle*.

There are various ways to measure and interpret visual behaviour during language processing. Early studies using visual-world eyetracking usually measured the latency of eye gaze to target objects relative to the onset of a referential description (Altmann & Kamide, 1999; Keysar et al., 2000; Tanenhaus et al., 1995). For example, in Experiment 2 of Barr and Keysar (2002), listeners' latency to fixate the target was measured as a function of whether or not a precedent had been established, and whether or not the precedent was in common ground with the current speaker. However, a more complete

picture of processing can be obtained by calculating the moment-by-moment probability of gazing at the objects in the display (Allopenna, Magnuson, & Tanenhaus, 1998). More recent studies on precedent use have tended to report and base statistical inferences on these more detailed gaze probability profiles. Typically, the researcher computes a *target advantage score* over a sequence of time windows, defined as the difference between the probability of gazing at the target object and the average probability of gazing at any other object within a given display. This score is calculated for each condition over a series of time-bins (e.g., 50 ms) and typically presented in a graph, while statistical inferences are generally performed on larger-sized time-windows (e.g., 300 ms) in order to minimize the number of statistical tests.

Experiments on precedent use in comprehension require an experimental structure in which precedents are introduced (establishment phase) and then referred to again during later (in a test phase). It is usually only the processing of the test phase utterances that is analyzed. Studies on precedents usually use unconventional objects such as abstract tangram shapes or other kinds unusual objects that lack conventional names in the language, as precedent effects are much more pronounced for these objects (Barr & Keysar, 2002). The wide variety of possible descriptions that can be given to these unusual objects encourages a reliance on referential precedents. For example, a speaker could establish a precedent to refer to an object as "the blueprint," but that same object could be called "the spaceship" or even be given a structural description such as, "lots of white lines forming different shapes." The large number of ways that speakers could describe these objects makes it unlikely that two speakers would by chance arrive at precisely the same description. The use of unconventional objects also avoids the potential problem of objects having preferred labels in everyday language.

The first visual-world study on precedent use in language comprehension (Barr & Keysar, 2002) presented findings from three experiments. Experiment 1 found evidence that listeners benefitted greatly from established precedents, with referential search

shortened by about 1300 ms when precedents were maintained. Experiments 2 and 3 probed the extent to which this benefit was attributable to common ground by having listeners interpret expressions from two different speakers (a male and a female speaker). In Experiment 2, the goal was to test for the existence of the same-speaker advantage for maintained precedents. One of the speakers interacted live with the listener, working together to rearrange objects in a vertical set of shelves. The other speaker gave instructions that were pre-recorded, and was only heard by the listener through a set of headphones. On test trials, the live speaker referred to a target object under one of four conditions created by combining two factors: (1) whether or not a precedent had been established for the target; and (2) the identity of the speaker who established it, which was either the same as the current speaker (i.e., the live speaker) or a different speaker (i.e., the pre-recorded speaker). To the extent precedent use is speaker specific, the effect of precedent should be larger when the precedent is in common ground. An analysis of the latency of fixation on the target found a large benefit of precedents, with listeners identifying the target about 1500 ms faster when they could rely on a precedent. However, there was no evidence that this advantage was any larger when the precedent was in common ground than when it was not (a difference of 50 ms that was not statistically reliable). However, this conclusion is limited because it only considered the latency of the final fixation on the target, and thus might miss out on any effects that are early and transitory in nature.

Experiment 3 of Barr and Keysar (2002) examined the role of precedents in listeners' expectations about the level of specificity that speakers would use when talking about conventional objects. For example, one and the same car can be referred to as *the vehicle* (superordinate level), *the car* (basic level), or *the sportscar* (subordinate level). Despite the overwhelming tendency for speakers to refer to objects at the basic level, Brennan and Clark (1996) showed that if speakers entrained on subordinate level terms for referents that were paired with another member of the same basic-level category (e.g., referring to a car

as *the sportscar* to distinguish it from a station wagon) they would tend to persist in using these subordinate-level terms even in contexts where the basic level term would be sufficient (e.g., continuing to call it *the sportscar* even though it was the only car). Based on this finding, in Experiment 3, Barr and Keysar (2002) asked whether listeners expect speakers to be overly specific, and whether such expectations were driven by common ground, or by their own egocentric experience.

To test this, pairs of pictures from different categories were identified such that the basic-level name for one of the pictures (car) overlapped phonologically with the onset of the subordinate-level name for the other member of the pair (carnation), but where there was also no overlap between the subordinate level name of the former (sportscar) and the basic-level name of the latter (flower). Listeners heard references from two different speakers, with half of the listeners led to believe they were speaking to them live over an internet link from another room (in reality, all speech was pre-recorded).[1] During the first part of the experiment, listeners heard a female speaker entrain on subordinate-level names for the objects (sportscar and carnation). Each member of a picture pair appeared alongside another member of the same category (e.g., two cars or two flowers), requiring the speaker to entrain on subordinate level descriptions. In the second part of the experiment, listeners either continued with the female speaker or with a male speaker who had just arrived, and who therefore would have no knowledge of the objects or of the precedents established by the original speaker. During this part of the experiment, the pairs of objects appeared together in the same display, so that the basic level names would be sufficient to identify targets, and speakers referred to them using the basic level names, thus breaking the subordinate-level precedent. For example, they viewed the car (entrained as *sportscar*) and flower (entrained as *carnation*) and heard the speaker say the word "car". To the extent that they expect speakers to follow established precedents, they should show

[1] There was no evidence that listeners who believed they were hearing live speech performed any differently from listeners who believed they heard recordings, so this factor was ignored in the analysis.

a tendency to gaze at the carnation upon hearing "car"; to the extent these expectations are speaker specific, this tendency to gaze at the carnation should be stronger with the same speaker than with the new speaker. Although listeners clearly expected speakers to continue using subordinate-level terms, there was no evidence that they expected this more strongly in the case of the same speaker.

A subsequent study by Metzing and Brennan (2003) examined the issue of broken precedents in more depth, focusing on unconventional objects. In their experiments, participants interacted with two live confederate speakers (one male and one female) who swapped roles with one another, entering and exiting the room so that the listener would have no reason to believe they shared knowledge of precedents. In this study, speakers either maintained or broke an established precedent, and the relevant precedent was one that either they themselves or the other speaker had established (forming the same speaker and different speaker conditions, respectively). Their analyses considered latencies of first and final fixations to the target object. As in Experiment 2 of Barr and Keysar (2002), they found no evidence for speaker-specific effects for maintained precedents. However, they did find a different speaker advantage for broken precedents: namely, listeners were less delayed ($\tilde{2}$86 ms) in identifying the target object when a speaker broke another speaker's precedent than when a speaker broke her (or his) own. Indeed, there was little evidence that listeners experienced any difficulty interpreting a new description of an old referent from a new partner. They also found that listeners looked more at non-target objects before looking at the target in the self-violation case than in the other-violation case.

Inspired by these results, researchers in developmental areas sought to determine whether young children also expect consistency in adult referential behavior. Matthews et al. (2010) recorded video of children between three and five years old as they performed a referential communication task similar to Metzing and Brennan (2003) with two different adult experimenters who instructed them to manipulate objects in a set of vertical shelves. The experiment had a similar design to the study by Metzing and Brennan, with the same

or a different experimenter either maintaining or breaking precedents established earlier in the experiment. As a measure of interpretation, Matthews et al. (2010) measured the time it took children to touch objects mentioned by the adult experimenter. Children exhibited confusion when they heard new expressions for old referents, even when listening to the new speaker, but they showed most confusion when precedents were violated by the same speaker. Interestingly, children often verbally protested the use of the new term, even with the new partner. It was clear that this behavior was the result of the precedent, as pre-experiment norming had shown that each term was equally good for describing the target.

A similar study by Graham et al. (2014) used visual-world eyetracking with four-year-old children as they listened to two adult experimenters and viewed pairs of pictures on a computer screen. In the establishment phase, one of the two experimenters established a particular way of referring to a given target object (e.g., the striped ball), and in the later test phase, that object was referred to once again using either the same adjective (striped) or a different adjective (yellow), and by either the same or a different speaker. All referring expressions used a similar expression format, a head noun preceded by a modifying adjective. Evidence for speaker-specificity emerged at the head noun, with a reliable difference between maintained versus broken precedents only in the case of the same speaker. It is difficult to say whether there was a same speaker advantage for maintained precedents or a different speaker advantage for broken precedents, as these effects were not tested in the statistical analysis. However, the graphs of the observed effects suggests some numerical support for the former of the two effects.

A study by Kronmüller and Barr (2007) that used visual world eyetracking with adult participants sought to follow up on the findings of Metzing and Brennan (2003). Because Metzing and Brennan (2003) reported only gaze latencies, Kronmüller and Barr (2007) noted that the findings could not distinguish between two different effect profiles for the speaker-specific effect, each of which would carry distinct theoretical implications.

First, they noted that violating a precedent is confusing because of the phenomenon of preemption—the established association of a particular referent with a particular expression preempts the association of a new expression with that same referent within the discourse. The different speaker advantage for broken precedents could arise from two possible sources: (1) speaker-specific preemption, in which listeners only experience preemption from precedents in common ground; or (2) speaker-independent preemption, followed by speaker-specific correction, a pattern they called *recovery from preemption*. The recovery-from-preemption explanation assumes that listeners experience preemption from any available precedent regardless of whether or not it is in common ground with the current speaker. Under this account, the different speaker advantage emerges because listeners recover from this preemption more easily when the precedent is not in common ground, as this fact gives them reason to suspend their expectations of consistency. Thus, when listeners hear a new expression for an old referent, there should be a period in which looks to the target are inhibited regardless of whether the precedent is in common ground, followed by a correction in which looks to the target rise faster in the different speaker case.

To distinguish these possibilities, Kronmüller and Barr (2007) conducted time-course data from two experiments in which listeners heard pre-recorded descriptions of unconventional referents from two speakers (one male and one female). In the Experiment 1, listeners searched for the target within an array that contained a varying number of objects, typically around eight. Like in Metzing and Brennan (2003), listeners heard expressions that either maintained or violated established precedents, which in turn were either in or not in their common ground with the current speaker. Consistent with recovery from preemption, listeners showed an early advantage for maintained precedents over broken precedents that was not modulated by speaker (emerging in 300–600 ms), with the different speaker advantage not emerging until much later, at around 1500 ms. A second experiment sought to simplify referential search by using only three objects in each display, including the target, a previously mentioned non-target object, and an

unmentioned object. Furthermore, for half of the trials in Experiment 2, listeners had to maintain a string of digits in working memory, inducing a kind of cognitive load. The hypothesis was that the load manipulation would have greater impact on the correction process than on the early partner-independent preemption effect. Confirming these expectations, the advantage for maintained over broken precedents was apparent as early as 300 ms in both load conditions, with the different speaker advantage for broken precedents emerging only at around 900 ms, but only in the condition where listeners were not under load. In the load condition, a post hoc analysis only found a recovery process extremely late in processing (after 2400 ms).

Brennan and Hanna (2009) later attributed the delay in the different speaker advantage that Kronmüller and Barr (2007) observed to their use of a noninteractive paradigm, in which listeners heard pre-recorded expressions from non-present speakers. From the point of view of collaborative models, interactivity is important since it is only possible to effectively "ground" a referential expression with a co-present interlocutor. This would imply that common ground is established more weakly (if it can be said to be established at all) in a noninteractive setting. As a result, Brennan and Hanna argue, effects of common ground might be smaller and possibly emerge later than in an interactive setting. To test this idea, they revisited the data from Metzing and Brennan (2003), which was conducted in an interactive setting with confederate speakers, and performed a bin-by-bin analysis, similar to that used by Kronmüller and Barr. They found less interference for broken precedents with the new speaker at a relatively early time window, 600-900 ms. However, consistent with recovery-from-preemption, this was preceded by a partner-independent main effect of precedent at 300-600 ms. In other words, just as Kronmüller and Barr (2007) found, the speaker-by-precedent interaction was preceded by a main effect of precedent. Brennan and Hanna discounted this earlier main effect of precedent by noting that the target advantage score for this window was not yet statistically above zero. Still, the fact that there is a main effect does seem to indicate that

the referential process had already begun. It is therefore premature to view these results as undermining the evidence for recovery from preemption, especially in light of the fact that interactivity was far from the only difference between the experiments (see Table 1).

Findings from Shintel and Keysar (2007) provide a further challenge to the assumption that interactivity should result in larger effects of common ground. In two experiments, Shintel and Keysar tested whether expectations of consistency depend on mutual knowledge by holding constant listeners' knowledge about precedents but manipulating their beliefs about whether these precedents were mutually known. Listeners observed speakers establishing precedents in one of two conditions: co-present with the speaker or remotely, watching a video of the speaker over a video link. It is only in the former of these two cases that the precedent can truly be said to be part of the common ground with the speaker. In the latter case, the listener knows what preferences the speaker has for referring to objects, but the precedents are not mutually agreed upon with the speaker. Listeners were delayed when speakers failed to be consistent in how they spoke about referents, but the magnitude of this delay did not appear to depend in any way on whether the precedent was mutual.

The three experiments reported in Barr (2008b) were designed to address issues other than precedent use, but one of them, Experiment 3, included a setup in which a precedent was established for a target object (e.g., a bucket), either by the speaker who later referred to that object, or by the experimenter. The analysis focused mainly on looks to a different object which was a phonological competitor to the target (e.g., a buckle), and which was also named either by the same or by a different speaker. There was no evidence that the competitor interfered more when the precedent had been established by the same speaker than by the experimenter.

Up to this point in the development of the literature, there seemed to be general agreement in the existence of a different speaker advantage for broken precedents, although it was not clear whether this effect was better explained by speaker-specific preemption or

by recovery from preemption. It was also the case that every attempt to find a same speaker advantage for maintained precedents had not succeeded, thus suggesting the possibility that such an effect did not exist. However, several later studies eventually succeeded in documenting the same speaker advantage. Barr (2008a) reanalyzed the maintained precedent conditions of Experiment 2 of Kronmüller and Barr (2007) using multilevel logistic regression. The analysis considered a time window from 300–450 ms, and found that the likelihood of gazing at the target increased at a faster rate when the speaker was the same than when the speaker was different. Barr suggested that the original analysis by Kronmüller and Barr (2007) failed to detect this difference due to the use of 300 ms time windows as well as due to the fact that it was masked by an anticipatory baseline effect in the opposite direction. The speaker specific advantage was furthermore not affected by a cognitive load manipulation, suggesting that it might be the result of episodic priming. However, this effect was detected only while looking at a time-window that was defined post-hoc and without any statistical correction.

More convincing evidence for speaker-specific effects for maintained precedents was reported in a series of experiments by Brown-Schmidt (2009a). Like Brennan and Hanna (2009), Brown-Schmidt argued that it was the lack of interactivity in Kronmüller and Barr (2007) that prevented the detection of the same speaker advantage for broken precedents. Participants in Brown-Schmidt's three experiments interacted with two different speakers, who were presented as experimenters rather than as confederates. The design was similar to Metzing and Brennan (2003), in that precedents were maintained or broken with the same or with a different speaker. Experiment 1 supported the existence of a same speaker advantage for maintained precedents within the 200–600 ms window. Although there was no different speaker advantage for broken precedents in any of the planned windows, an (uncorrected) post-hoc analysis on an unplanned time window from 180–300 ms did reveal some evidence for such an effect. In a second version of this experiment, the live recordings from the experimental sessions were edited to remove all speech from the participant so

that only the experimenter's speech remained. This speech was then presented to a new set of participants who performed the task noninteractively. No same speaker advantage for maintained precedents was obtained. However, the same speaker advantage was once again demonstrated in a final experiment that once again involved live interaction with experimenters. It is interesting to note that none of the experiments in this paper reported the different speaker advantage for broken precedents, apart from the uncorrected post-hoc analysis in Experment 1.

Horton and Slaten (2012) provided further evidence for a same speaker advantage for maintained precedents across two experiments. Listeners heard pre-recorded speakers describe referents during an early *association* phase intended to build up knowledge of precedents and association these with particular speakers. The goal was to examine the hypothesis that speakers serve as retrieval cues for expression-referent associations stored in memory. The objects being described were tangram-based shapes designed to bear resemblance to various categories of objects (cats, people, cars, etc.). During a later test phase, listeners viewed pairs of objects from the same category (e.g., two shapes that looked like cats). Descriptions of these objects were of the form *the cat with the very long tail*, such that the utterance would be ambiguous at the head noun (e.g. cat). In the between-speaker condition, each picture had been previously associated with descriptions from different speaker. In the within-speaker condition, both of the pictures forming a given pair had been associated with the same speaker, such that listeners would be unable to identify the referent until they heard linguistically disambiguating information. The factor of between- versus within-speaker associations was crossed with the factor of speaker, which referred to whether the target was described by the same speaker who had originally established the association or by the other speaker. Two experiments detected a same speaker advantage for maintained precedents: listeners in the between-speaker association condition looked at the target sooner when it was described by the same speaker.

To summarize, studies on precedent use in spoken language comprehension generally

support the idea that listeners expect speakers to be consistent, and that when speakers conform to these expectations, comprehension is facilitated. However, the mechanisms underlying these effects are not clearly understood. To the extent that precedent effects are supported by common ground, there should be robust speaker effects: a same speaker advantage for maintained precedents and a different speaker advantage for broken precedents. But these effects appear in some studies and not others. It is possible that these speaker effects exist but are fairly small, thus accounting for the fact that they are not always detected. Furthermore, the time course of these effects, when they appear, has yet to be definitively established. One powerful way to address these problems is to combine effects across studies in a meta analysis. In the next section, we provide further details about how we derived the effects used in the analysis, and also attempt to characterize the methodological dimensions on which the studies differ.

Method

The studies on precedents considered in the review above are shown in Table 1, which lists sample characteristics as well as methodological features of these studies.

Composition of the sample of experiments

We sought to include the overall mean target advantage curves for each condition for all other experiments that contained a speaker manipulation and maintained and/or broken precedents. When the original data was not readily available, as was the case for all experiments which we did not co-authored, we estimated the target advantage curves from published graphs using photo-editing software (GIMP; `http://www.gimp.org`), mapping the height of the curves to the vertical scale on the graph. All of the analyses were performed using R (R Core Team, 2014). The raw data and analysis scripts are publically available at `http://github.com/dalejbarr/precmeta`.

Our meta-analysis focused on visual-world experiments with typically-developing adult participants, and thus we excluded experiments using young children as listeners

(Graham et al., 2014; Matthews et al., 2010). Although Barr et al. (2014) used visual-world eyetracking with adults and addressed the issue of maintained referential precedents, we opted to exclude this experiment because the precedents under study were in the form of long-standing associations between proper names, voices, and persons, in contrast to the temporary discourse conventions used in standard experiments on precedent use. Finally, we were unable to include Shintel and Keysar (2007) because there was no manipulation of speaker (same or different), only a manipulation of maintained versus broken precedent, and whether the establishment of the precedent was in common ground with the speaker.

Experiments 2 and 3 of Barr and Keysar (2002) both contain speaker manipuluations, but only Experiment 2 provides clear data for our analysis. Although gaze probabilities were not reported for Experiment 2 in the original article, we were able to access the original data and calculate the values we needed to include it in the meta-analysis. Experiment 3 could not contribute unambiguously to our analysis because the speech was temporarily ambiguous between maintaining and breaking the precedent; that is, it would be potentially unclear to listeners whether "car" was the first syllable of the subordinate-level term *carnation* (maintaining the precedent) or just the basic-level term *car*.

The target advantage scores for Metzing and Brennan (2003) were derived from the graph published in the reanalysis of these data by Brennan and Hanna (2009). We included both experiments from Kronmüller and Barr (2007), but separated out the data from the (within-subject) cognitive load conditions, treating them as though they were separate experiments, as load had a strong impact on processing. Data for looks to the target from Experiment 3 of Barr (2008b) were also included, as these data can be used to test for the same speaker advantage for maintained precedents, even though this was not the main goal of the experiment. Finally, from the graphs in Brown-Schmidt (2009a) we extracted the mean curves for the three experiments. In a similar way, we extracted the curves for Experiment 1 of Horton and Slaten (2012) (no curves for Experiment 2 were

reported in the article).

As reported in Table 1, our final sample included ten experiments. Data from all ten these experiments could address the same speaker advantage for maintained precedents, whereas only seven of the ten had appropriate manipulations to test for the different speaker advantage for broken precedents. Finally, eight of the experiments provided appropriate baselines with which to test the main effect of precedent.

Characteristics of the experiments

In addition to variation in sample size, there are some important differences that are related to the many possible methodological and analytical variations on the basic paradigm. Some of these differences are summarized in Table 1. We define and review these differences here.

Live Interaction. Some experiments involve live interaction between speakers and listeners while in others listeners heard pre-recorded speech by a speaker that was not present in the experiment. There are pros and cons to each of these alternatives. Live interaction is more naturalistic, representing what happens in a everyday face-to-face conversation. However, live interaction also adds variability to data since every critical expression is a unique token with different acoustic characteristics. In contrast, pre-recorded expressions allow having exactly the same expression for each test trial in all different conditions, thus reducing variability.

Confederate Identity Masked. Independently of whether experiments use pre-recorded versus live speech, is how the social identities of the speakers are presented to participants. In most experiments, the fact that the speakers were associates of the lab was masked, with participants led to believe that the speakers were either previous participants in the experiment (in the case of pre-recorded speech) or that they were actual participants (in the case of live interaction). This is generally done so that listeners will have the appropriate cooperative expectations about participants' speech. The exception is

Brown-Schmidt (2009a), where speakers were presented as experimenters or lab assistants. It is possible that making speakers' affiliation with the lab explicit could reduce listeners' expectations of cooperativity.

Speakers Blind to Condition. Experiments differ in the knowledge of the speakers related to what condition a given test trial appears in (maintained versus broken, same versus different speaker). A criticism that is applicable to studies using live interaction is that speakers generally know whether they are using the same or a different expression to refer to an object (at least in the same-speaker condition), and this could potentially influence their linguistic performance. Breaking one's own precedent is uncooperative and thus a face-threatening act. It is possible that this knowledge could influence speakers' prosody or fluency in delivering the critical utterance, which creates a potential confound.

Fixed Speaker at Test. The test expressions used in experiments also vary in terms of how the speaker manipulation was implemented for a given experimental item. For most experiments considered in the meta-analysis, the test utterance for a given item was produced by a single speaker in all conditions, with the identity of the speaker (same/different) implemented by varying the identity of the speaker who established the precedent. Another method is to manipulate the identity of the speaker at test. Experiments that follow this latter strategy will have additional variability in measurement due to variation in the acoustic realization of the same expression by different speakers.

Fixed Expression at Test. Another variation was how the maintained versus broken precedent manipulation was implemented. In the broken precedent condition, the expression at test was a different expression from the one used previously. The best way to accomplish this is to hold the test expression constant (since that is where the measurement take place) and vary the identity of the precedent established earlier. So for example, if the test expression is *the tent*, then the precedent would consistent of *tent* in the maintained condition versus, for instance *folded paper* in the broken condition. The alternative would

be to hold the established precedent constant as *tent*, and break it by using *folded paper* as the test expression. Experiments following the latter approach will have additional variability in measurement due to variation in the linguistic form of the test expression.

Fixed Expression Format. Another way to control variability and thus increase power is to use the same expression syntax across all items used in the experiment; by, for example, having all test expressions of the format *determiner+adjective+noun*, e.g., *the multicolored squiggle*. Most experiments have different syntactic constructions for different items, which would add variability to the measurement of the time-course of the various effects.

Time-Course Analysis. Finally, experiments varied in the way data and results were presented in the paper and also in the statistical techniques used to analyze those data. Most experiments presented time course eye-tracking data in graphs as well as for the main analysis. Two of the early publications presented data in terms of fixation latencies (Barr & Keysar, 2002; Metzing & Brennan, 2003), but fortunately, later analyses (Brennan & Hanna, 2009) as well as access to the original data from Barr and Keysar (2002) make it possible to include time-course data from these experiments in the meta-analysis.

Baseline Control. Listeners can sometimes develop expectations regarding the identity of the target object prior to hearing the test expression. For example, listeners might expect speakers to refer to something they have not yet mentioned in the discourse, which would differ depending on the identity of the current speaker. When listeners have such prior expectations, these introduce attentional biases to particular objects in a display (anticipatory baseline effects), which can be confounded with referential effects associated with the processing of the test expression (Barr, 2008a; Barr, Gann, & Pierce, 2011). Such effects can be controlled statistically by assessing the changes induced by the processing of the critical words in the expression relative to a earlier time-interval that serves as a baseline for attentional biases. Barr (2008a) found evidence that the same-speaker advantage for maintained precedents was masked by an anticipatory baseline effect in the

opposite direction. To date, few studies have implemented such statistical controls.

Deriving and analyzing the three principal effects

We now identify and discussion how we derived the three principal effects that have supplied critical supporting evidence for various accounts of precedent use in comprehension. The first effect is the *Main effect of precedent*. This effect accounts for the finding that listeners recognize a target object as the intended referent faster and more accurately when an expression conforms to an established precedent. As a main effect, this effect ignores whether or not the precedent is in common ground with the current speaker. The interpretation of this main effect is ambiguous in the presence of an interaction, a complication that we deal with at the end of the Results section.

The second principal effect is the *Same-speaker advantage for maintained precedents* which refers to the differential facilitation on comprehension that occurs when an expression is repeated by the same speaker compared to a different speaker. Finally, the third principal effect is the *Different-speaker advantage for broken precedents*. This effect refers to the advantage that occurs when a new speaker refers to an old referent using a new expression, relative to an old speaker referring an old referent using a new expression (and thus violating his or her own precedent).

In Figure 1 a schematic representation of the prototypical experimental design is depicted, combining the levels of the factor precedent (maintained vs. broken) and speaker (same vs. different). To explain how the effects were derived, the cells of the design have been labeled A–D. The Main effect of precedent is computed as the difference in the marginal means for the precedent manipulation, that is, $(A + B)/2 - (C + D)/2$. The other two effects correspond to the two simple effects of the variable speaker in the two levels of the variable precedent. The Same-speaker advantage for maintained precedents corresponds to the simple effect of speaker when the precedent is maintained and is computed by substracting the different speaker maintained condition to the same speaker

Table 1

Experimental datasets, effects detected, and properties of the experimental setups

Article	Exp.	Sample			Effects detected			Characteristics of the experiment							
		No. subjects	No. items	No. cells in design	Main effect of precedent	Same-speaker advantage for maintained precedents	Different-speaker advantage for broken precedents	Live Interaction	Confederate identity masked	Speakers blind to condition	Fixed speaker at test	Fixed expression at test	Fixed expression format	Time-course analysis	Baseline control
Barr & Keysar (2002)	2	36	12	4	✓	✗	—	✓	✓	✓	✓	✓	✗	[c]✓	✗
Metzing & Brennan (2003)	1	24	8	4	✓	✗	✓	✓	✓	✗	✗	✗	✗	[c]✓	✗
Kronmüller & Barr (2007)	1	52	8	4	✓	✗	✓	✗	✓	✓	✓	✓	✓	✓	✗
No Cognitive Load	2	56	16	4	✓	[a]✓	✓	✗	✓	✓	✓	✓	✗	✓	[a]✓
Cognitive Load	2	56	16	4	✓	[a]✓	[b]✓	✗	✓	✓	✓	✓	✗	✓	[a]✓
Barr (2008b)	3	36	24	6	—	✗	—	✗	✓	✓	✓	✓	✗	✓	✓
Brown-Schmidt (2009a)	1a	48	16	4	✓	✓	[b]✓	✓	✗	✗	✓	✓	✓	✓	✗
	1b	48	16	4	✓	✗	✗	✗	✗	✗	✓	✓	✓	✓	✗
	2	32	32	4	✓	✓	✗	✓	✗	✗	✓	✓	✓	✓	✗
Horton & Slaten (2012)	1	32	16	4	—	✓	—	✗	✓	✓	✗	✗	✓	✓	✗
Additional experiments not included in the analysis															
Barr & Keysar (2002)	3	64	8	4	✓	[h]✗	[h]✗	✗	✓	✗	✗	✓	✓	✓	✗
Shintel & Keysar (2007)	1	36	10	4	[g]✓	—	—	✓	✓	✗	✓	✗	✗	✗	✗
	2	39	12	6	[g]✓	—	—	✓	✓	✗	✓	✗	✗	✗	✗
Matthews et al. (2010)	1	[d]126	8	4	[g]✓	*nt*	*nt*	✓	✗	✗	✗	✗	✗	—	—
Barr et al. (2014)	1	20	48	8	✓	✓	—	✓	[e]✓	✗	✗	✗	✓	✓	✓
Graham et al. (2014)	1	[d]72	3	4	✓	*nt*	*nt*	✓	✗	✗	✗	✗	✓	✓	✗

✓ Yes; ✗ No; — Not Applicable; *nt* Simple effects of speaker for each expression type not tested

[a] In post-hoc analysis with unplanned time windows and different statistical approach (see Barr, 2008a)

[b] In post-hoc analysis with unplanned time windows

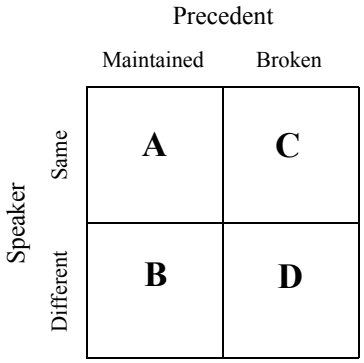
[c] In later re-analysis, with original paper reporting fixation latencies

[d] Child sample

[e] Speaker was a naïve participant

maintained condition ($A - B$). The Different-speaker advantage for broken precedents is computed by substracting the same speaker broken precedent to the different speaker broken precedent ($D - C$).

Figure 1. Design

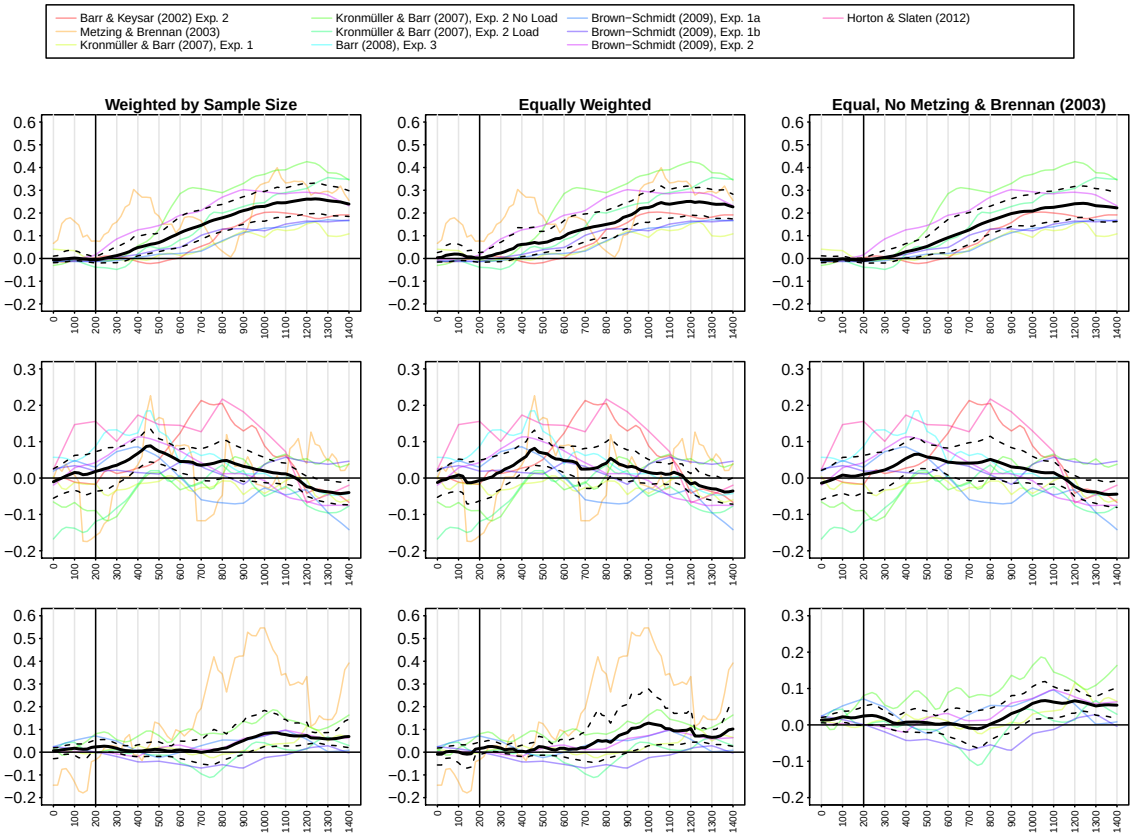


Results and Discussion

Figure 2 presents the effect profiles from the individual experiments, along with mean trendlines and 95% confidence intervals derived from bootstrap resampling of the individual studies. In the left column of the figure, each study was weighted equally in deriving the mean and bootstrap interval. Given the variability in sample sizes, we re-ran the bootstrapping procedure, inversely weighting each of the studies by a rough index of the sample size (specifically, the number of observations per cell of the design). These results appear in the middle column of Figure 2. Finally, the data from Metzing and Brennan (2003) seemed to show an outlying pattern that may have disproportionally affected the bootstrap intervals, so we ran a third version where this study was excluded (right column of Figure 2). Reassuringly, all three bootstrap analyses yielded similar mean trendlines and confidence intervals.

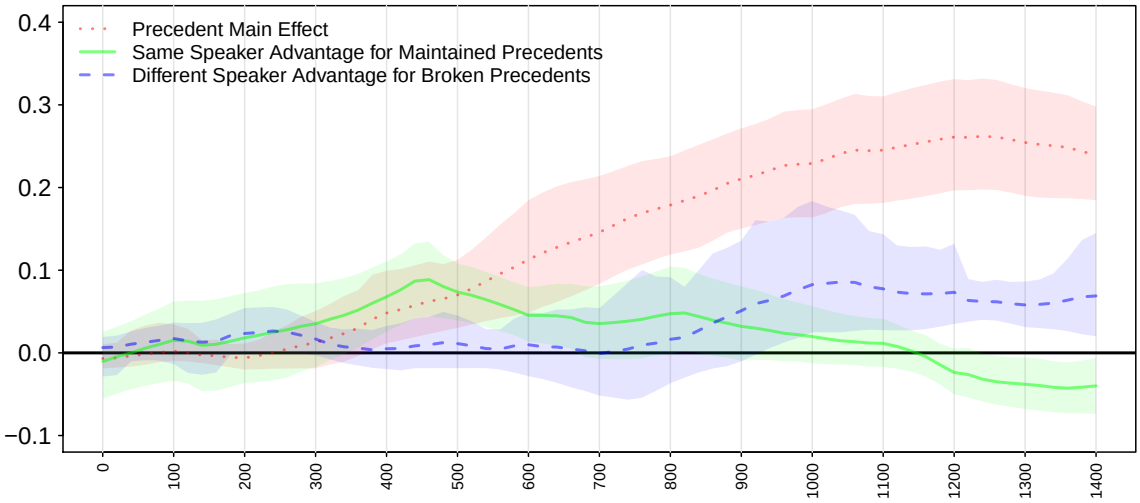
To enable comparison of the magnitudes of the three principal effects, Figure 3 presents the main trendlines and confidence intervals together in a single graph (from the

Figure 2. Temporal profiles of effects in individual experiments (colored lines) with mean experiment trendlines (dark line) and bootstrap confidence intervals (dashed lines), for each of three bootstrap analyses (columns). Top row: main effect of precedent; Middle row: speaker-specific repetition benefit; Bottom row: different-speaker novelty benefit.



equally-weighted analysis). The main effect of precedent emerged from the earliest moments, with an increasing trend starting at 200 ms that became reliable by 400 ms and remained so until the end of the analysis window. The speaker effect for maintained precedents showed a similar early pattern, closely tracking the main effect of precedent until around 450 ms. This pattern was consistent across experiments, with eight of the ten showing a positively-sloped curve from 200 ms to 450 ms, the two exceptions being Experiment 1 of Kronmüller and Barr (2007) and Experiment 1b of Brown-Schmidt (2009a). Interestingly, from its peak at 450 ms, the effect gradually decayed, becoming

Figure 3. Relative effect sizes of main effects and speaker effects.



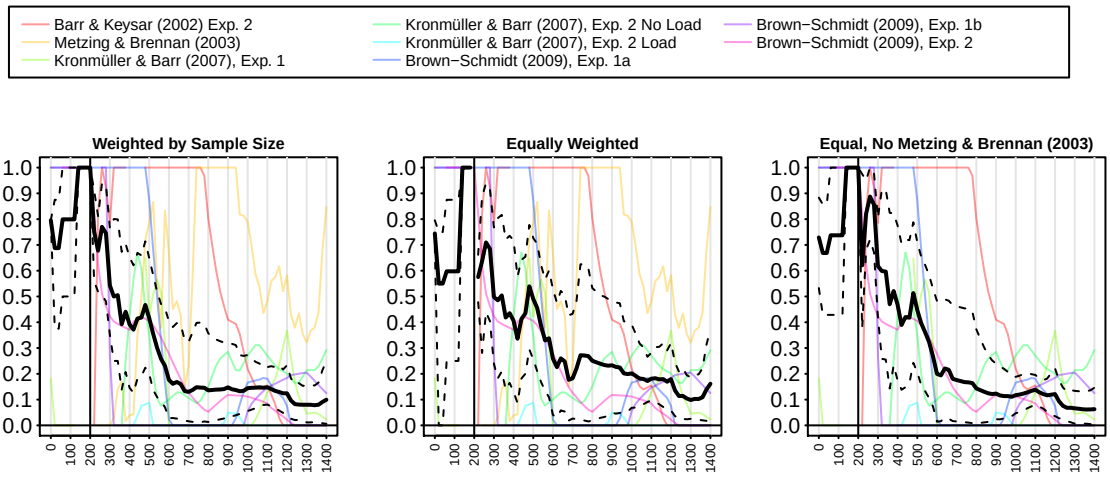
statistically unreliable by 700 ms. Curiously, it even seemed to show a reversal around 1200 ms, with listeners becoming slightly more likely to look at the target when a new speaker followed another speaker's precedent. This reversal may not be statistically robust, as no single study has statistically detected such a reversal, and three of the 10 studies do not follow the pattern. In sum, the same-speaker advantage is likely to exist, and arises early in processing, but yields only a short-lived advantage.

The different speaker advantage for broken precedents, in contrast, had a drastically different overall temporal profile from either the main effect or the speaker effect for maintained precedents. This effect emerged much later than the same-speaker advantage for maintained precedents, no earlier than 700 ms after speech onset, well past the peak of the same-speaker repetition benefit, and well after the onset of the precedent main effect. The effect became reliable by 900 ms and remained so for the rest of the analysis window. Also, rather than being short-lived, like the speaker effect for maintained precedents, it was sustained throughout the interval.

The pattern was fairly consistent across the seven studies that we considered, and showed little change even when the extreme case of Metzing and Brennan (2003) was removed. By about 1150 ms, all studies showed the same numerical benefit for different

speakers. Note that the two experiments showing the weakest different speaker benefit were the cognitive load group in Experiment 2 of Kronmüller and Barr (2007), and the non-interactive Experiment 1b of Brown-Schmidt (2009a). The weak speaker effect in the former study can be explained by cognitive load impeding listeners' ability to use common ground. The latter study had no load manipulation, but is unusual in how the speech stimuli were generated. Unlike other studies, Brown-Schmidt's Experiment 1b used recorded speech from actual spontaneous dialogs between experimenters and actual participants, with the speech of the participants edited out. Research indicates that hearing only half of a conversation imposes attentional demands on listeners (Emberson, Lupyan, Goldstein, & Spivey, 2010). Perhaps the use of such "half-a-logues" in Experiment 1b of Brown-Schmidt (2009a) imposed attentional demands on listeners that, like the cognitive load manipulation in Experiment 2 of Kronmüller and Barr (2007), reduced the different speaker benefit for broken precedents.

Figure 4. Temporal profiles of the partner-specificity index in individual experiments (colored lines) with mean experiment trendlines (dark line) and bootstrap confidence intervals (dashed lines), for each of three bootstrap analyses (columns). Higher values are associated with greater partner-specificity.



Lastly, we note that across all studies, the effect that was largest and most

consistently detected was the main effect of precedent—whether a precedent was maintained or broken *for the listener*. Indeed, every single one of the experiments included in the analysis (Table 1) for which a precedent effect was possible to calculate reported a statistically significant main effect of precedent, in contrast to only 5/10 reporting a significant speaker effect for maintained precedents (one post-hoc only), and 5/7 reporting the speaker effect for broken precedents (two post-hoc only). What does this large and statistically robust effect imply about precedent use? One way to think of the main effect of precedent is as the mean of the two simple effects of precedent (maintained minus broken), one for each level of speaker conditions (same and different). Denoting these two simple effects as X_{same} and X_{diff}, the main effect M is given by $M = \frac{X_{same}+X_{diff}}{2}$. To the extent that precedent use is partner specific, X_{same} will be greater than zero, and X_{diff} will approach zero. Complete partner specificity entails that $M = \frac{X_{same}}{2}$. In contrast, complete partner independence implies that $X_{same} = X_{diff}$, and thus that $M = X_{same}$. These observations suggest the possibility of creating an index of partner specificity using the following formula:

$$\frac{2X_{same}}{X_{same} + X_{diff}} - 1$$

where zero implies pure partner independence (both simple effects are equal) and one implies pure partner specificity. The formula assumes that $X_{same} \geq 0$ and $X_{diff} \geq 0$, so in any case where either effect was negative, we set it to zero.

Figure 4 shows the indices for all eight experiments contributing to the main effect. The indices are plotted as a function of time, along with mean trendlines and 95% confidence intervals. Note that this index will be extremely noisy while the main effect itself is close to zero (i.e., before 400 ms). What this suggests is that while the early moments (200–400 ms) of the main effect of precedent show greater partner specificity, beyond 400 ms the main effect reflects processes that are largely partner-independent. Indeed, beyond 800 ms, less than 20% of the main effect could be attributable to partner

specific processing.

General Discussion

Against the contradictory picture presented by the literature on precedent use in comprehension, our meta-analysis suggests a fair degree of consistency across studies. We focused on three principal effects: the speaker-speaker advantage for maintained precedents, the different-speaker advantage for broken precedents, and the main effect of precedent. The three principal effect profiles differ drastically from each another in their timing, temporal profiles (monotonically increasing, sustained, or ephemeral), and effect sizes.

Against Barr and Keysar (2002) and Metzing and Brennan (2003), and supporting Brown-Schmidt (2009a), there was clear evidence for a same-speaker advantage for maintained precedents, which was present from the earliest moments of comprehension. Eight of the ten studies we considered exhibited a positive slope in the 200–450 ms window. Without assuming the existence of the effect, it would be difficult to explain this consistency in timing and direction of the effect profile. Our investigation also supports the existence of a different-speaker advantage for broken precedents, which in comparison to the speaker effect for maintained precedents, only emerged after a substantial delay. The different-speaker advantage appears robust, despite the fact that it has not been reliably detected in all individual studies, such as in two experiments by Brown-Schmidt (2009a).

In this section, we begin with discussion of how the differences in the profiles of the three effects support the view that they are attributable to different underlying mechanisms. Then, we attempt to account for the methodological reasons why particular experiments may succeed or fail in detecting these effects. Finally, we close with some practical implications for the use of visual-world eyetracking data to advance psychological theory about language comprehension in dialogue.

Potential mechanisms behind the effects

The distinct temporal profiles of the three principal effects in the studies considered in our analysis, taken together with findings from other studies, broadly support the view that precedent effects are realized by a variety of underlying and functionally distinct cognitive mechanisms. In what follows, we suggest a central role for egocentric computations arising out of domain-general processes, with a largely peripheral role for common ground in monitoring and correction. Specifically, we propose that the same-speaker advantage for maintained precedents, the different-speaker advantage for broken precedents, and the partner-independent main effect of precedents, respectively are the result of: (1) episodic priming; (2) a correction process involving invocation of common ground; and (3) predictions based on egocentric heuristics. Before considering these explanations in turn, we briefly discuss why these patterns cannot be readily explained by constraint-based models.

Constraint-based models view comprehension as a single undifferentiated system, with interpretation resulting from the interactive simultaneous combination of multiple probabilistic cues. As such, constraint-based models are essentially notational variants of Bayesian models (Jurafsky, 2003). This view assumes that various cues exert immediate effects on comprehension in a manner proportional to their salience and reliability in the environment. It is not readily apparent who such a model could simultaneously explain the three distinct profiles in the current meta-analysis. Primarily, constraint-based models assume that cues differ only in salience and reliability, but not in their temporal availability. It therefore predicts that effects of a given constraint should be apparent from the earliest moments of processing. The observed lag in effect onsets, with the different-speaker advantage emerging about 500 ms later than the same-speaker advantage, is inconsistent with this prediction.

Perhaps a constraint-based theorist could argue that the two effects are in fact simultaneous, and that the time-lag is a statistical artifact resulting from the

different-speaker advantage having a smaller effect size or greater variance than the same-speaker advantage. It is indeed possible for two simultaneous effects to appear to emerge (statistically) at different points in time if one of the effects is smaller and/or noisier than the other. But Figure 3 does not support this interpretation. During the 200–500 ms interval in which the same-speaker effect is increasing while the different-speaker remains near zero, the confidence band for the latter is actually *narrower* than that for the former, the opposite of what would be required to explain the different effect profiles in terms of differences in variance. Moreover, the peak effect size is similar for both effects, with both approaching .10, the same-speaker effect at 450 ms and the different speaker effect at 1000 ms. In short, the time lag is unlikely to be explained away as a statistical artifact, and seems to reflect true underlying differences in processing.

An additional difficulty for the constraint-based view is in explaining the transitory nature of the same-speaker advantage for maintained precedents. The effect increases to reach its peak at 450 ms, after which it begins a steady decline back to zero. Under constraint-based models, which assume an evidence accumulation process, the probability of gazing at a given referential alternative at a given moment should reflect the totality of evidence accumulated for that alternative up to that moment. In the context of the referential communication experiments reviewed above, evidence accumulation for the target is monotonic—the listener receives increasing evidence over time for the target. So why is the peak effect of the social and linguistic information not sustained over the full interval, as seems to be the case for the two other principal effects? It is as if the speaker information affects eye movements without contributing to the evidence accumulation process per se.

In contrast to constraint-based views, we propose that the variety of patterns exhibited by the three principal effects suggests they are supported by different underlying cognitive mechanisms. We begin our discussion of these mechanisms by considering the same-speaker advantage for maintained precedents. Our proposal is that this effect does

not feed into the interpretation process proper, but reflects a kind of implicit episodic priming. By episodic priming, we mean the phenomenon by which a cluster of associations stored in episodic memory associated with a stimulus are activated in a manner independent of conscious awareness (Tulving & Schacter, 1990). For instance, hearing a word repeated by the same speaker versus a different speaker can influence recognition memory for a word, implicating that listeners maintain detailed traces of the perceptual event associated with hearing a spoken word (Church & Schacter, 1994; Goldinger, 1996; Mullennix, Pisoni, & Martin, 1989). In the case of referential language, it seems likely that such episodic traces will additionally contain information about the referent of the expression, such that hearing an expression will more strongly activate the referent associated with the expression on previous trials when it is spoken in the same voice.

Episodic priming provides a better explanation than common ground for the transitory nature of the same-speaker advantage that was discussed above. Priming can lie outside of awareness and might be the result of a memory system that is functionally independent of the systems involved in linguistic interpretation. The explanation in terms of episodic priming is also supported by the observation that the same-speaker advantage seems impervious to cognitive load. A number of studies that suggest that accessing and using common ground requires effortful attention (Brown-Schmidt, 2009b; Nilsen & Graham, 2009; Rossnagel, 2000). Experiment 2 of Kronmüller and Barr (2007) tested the effects of load on the use of precedents, with half of the participants performed the task under cognitive load. Barr (2008a)'s reanalysis of this experiment found an increasing slope from 300–450 ms for the same speaker condition, with no evidence that the effect was modulated by cognitive load. In contrast, the load manipulation strongly influenced the different-speaker advantage for broken precedents.

Finally, support from the episodic priming view comes from a study by Barr et al. (2014) on proper names. Unlike most laboratory studies on precedent use, in which referential associations are established and tested within an experimental session, Barr et

al. (2014) took advantage of the pre-existing referential associations naturally existing between undergraduate friends, in terms of which students from their same year of study they mutually knew, and what proper names they used to refer to them (e.g., Kevin, Claire, etc.). In a referential task, listeners' eyes were tracked as they viewed pictures of mutually known and non-mutually known students and heard a speaker name a target person. From 528–796 ms, listeners were more likely to look at the target if the name was spoken by the friend versus an unfamiliar person (a lab assistant). However, this was the case whenever the friend articulated the name, even if the friend was not the source of the message (i.e., the friend was relaying a message designed by the assistant). In this latter case, the friend was not in common ground with the listener, which implies that the target person was activated through episodic associations. The effect is similar to the same-speaker repetition benefit observed in the current investigation, in that it is short lived, and has a similar timing.

Could it perhaps be argued that episodic priming is one mechanism through which common ground effects are realized? The question is relevant because some theorists have moved to conflate common ground with ordinary memory (Gerrig & McKoon, 1998; Horton & Gerrig, 2005; Brennan & Hanna, 2009). For instance, Gerrig and McKoon (1998) proposed that "common ground is merely memory processes acting on representations" (p. 82); similarly, Brennan and Hanna (2009) claimed that common ground is "just like any other information in memory" (p. 275). No one of course denies that common ground depends on memory, but such a conflation could only promote further terminological and theoretical confusion (Lee, 2001). What is critical about common ground is that it is mutually known or mutually believed to be shared (Clark & Marshall, 1981). When listeners witness a speaker choosing to refer to a piece of folded paper as "the tent", they will store an episode in memory that links together the speaker, the expression (in that speaker's voice), and the referent. However, it is only in the case in which the listener knows that speaker knows that the listener has observed the labeling event, that the

episode becomes part of the common ground; if there is no co-presence—e.g., if listeners secretly observed the labeling episode over a video link—then they have no basis for believing that the speaker knows that they know. Speaker effects in precedent use may be independent of whether these higher-level links in the inference chain have been completed, and as such, may reflect basic memory operations or egocentric heuristics rather than common ground (Shintel & Keysar, 2007). A final argument against conflating common ground with ordinary memory is that memory associations and common ground can point in different directions during language use, for instance in cases of direct quotation or simultaneous translation. In such cases, what is relevant for interpretation is the common ground with the designer of the message rather than the information that happens to be associated with the person who is delivering the message (Barr et al., 2014).

Contrasting strongly with the temporal profile of the same-speaker advantage is the different-speaker advantage for broken precedents. This effect appears much later, well after the speaker effect for maintained precedents or the main effect of precedent. This late emergence is consistent with the recovery-from-preemption proposal (Kronmüller & Barr, 2007), who argued that it reflects the use of common ground to recover from a partner-independent preemption effect. Additionally, the fact that they found the effect to be attenuated by cognitive load provides further support for this hypothesis, given the evidence from other studies (cited above) that reasoning about common ground is effortful. The idea that common ground is involved is also supported by the fact that the effect seems strongest on the first few trials involving a broken precedent (Brennan & Hanna, 2009; Matthews et al., 2010), which could reflect listeners gradually suspending their assumption that the speaker will be cooperative. In contrast, no such dissipation over trials has been reported for the speaker effect for maintained precedents. Finally, studies have found that unlike for maintained precedents, for broken precedents whether or not the precedent is in common ground affects the rate of target selection, with listeners more likely to select the target in the different speaker condition (Kronmüller & Barr, 2007).

However, this final point comes with the caveat that the identity of the target is usually less ambiguous in the case where a precedent is maintained than when it is broken.

Further support for the recovery-from-preemption proposal comes from a recent neuroimaging study in which listeners' brains were scanned using magentoencephalography (MEG) while they heard speakers break precedents (Bögels et al., in press). Areas of the brain associated with mentalizing (e.g., ventromedial prefrontal cortex, right temporo-parietal junction) seemed to be activated "on demand" to restore coherence after a pragmatic violation, instead of being activated in advance to calibrate listeners' expectations to their common ground with the current speaker.

Finally, late effects of common ground were also detected in a recent study that looked at the use of precedents in the interpretation of negated referring expressions of the form *not the key* (Kronmüller et al., under review). Listeners heard such expressions under one of two conditions, while viewing a display containing three unconventional objects, two of which were potential referents for the negated expression (forming the complement set). In the *common ground* condition, the current speaker had previously referred to one of these two objects (the *critical* object) using a positive referring expression (e.g., "the spaceship"), thus affording the inference that the speaker must be referring to other of the two objects, which had not yet been named (the *unmentioned* object). In the *privileged ground* condition, the critical object had also been named, but by another speaker, and so the labeling event was not in common ground with the current speaker; thus, the speaker could plausibly be using negation to refer to either of the two referents. To the extent that listeners use precedents based o common ground, the preference to look at the unmentioned object over the critical object should be stronger in the common ground condition. However, the difference between these two conditions did not emerge until around 1100 ms after speech onset, with both conditions showing a similar preference for the unmentioned object prior to this moment.

Researchers studying precedent use in comprehension have almost single-mindedly

focused their efforts on documenting the existence and timing of speaker-specific effects in comprehension, and on developing explanations for the observed patterns. This is unfortunate since (as Figure 3 indicates) speaker-specific factors explain only a small part of the total effect of precedents on comprehension. Listeners expect to hear descriptions conforming to established precedents mostly due to factors that are unrelated to common ground, but the field is currently lacking a good explanation for why this should be the case. It is possible that the speaker-independent share of the maintained precedent effect could reflect priming due to the established symbolic association. Likewise, the fact that listeners seem to experience difficulty mapping new expressions to referents that were previously described differently could reflect difficulties reconceptualizing objects (as suggested by Kronmüller et al., under review). Alternatively, it may be the case that listeners are following a simple heuristic according to which old expressions are mapped to old referents and new expressions to new referents.

An interesting possibility that has not been sufficiently explored is that speaker-independent precedent effects reflect listeners' use of a model of the behavior of a *generic* speaker. It is possible that listeners are interpreting the speech from the perspective of an idealized, cooperative speaker who happens to share the listener's knowledge. In other words, listeners might seek to explain the speaker's behavior by considering what it would mean if *they themselves* had performed the speech act. It seems necessary to assume that some sort of speaker modeling is going on to explain why listeners might be surprised when a speaker breaks a precedent. If speaker were purely egocentric, and did not engage in any speaker modeling, then they should not care when a speaker breaks a precedent; they should just choose the referent that best matches the description. The fact that the bring a speaker's previous utterances to bear suggest that some sort of speaker modeling is going on, but we suggest that this modeling is extremely minimal.

In sum, although the debate in research on dialogue has centered around the question of whether partner-specific effects exist, any theory will capture very little of the

phenomenon unless it grapples with the towering effects related to speaker-independent processing. Although speaker-specificity does exist, the effects do not drive the main effect of precedent. Theories based on common ground alone have weak explanatory power, especially if, as we have argued, the early same-speaker advantage for maintained precedents reflects episodic priming rather than common ground.

Explaining inconsistency of the studies

To orient future research on this topic, it is informative to consider the possible reasons why some studies have succeeded while others failed to detect partner specific effects. To begin, various factors may have conspired against the detection of the speaker-specific repetition benefit. First, the effect is short lived, making it difficult to detect using fixation latency measures, such as those used in Barr and Keysar (2002) and Metzing and Brennan (2003). Second, in some cases, the effect is sometimes masked by anticipatory baseline effects in the opposite direction (Barr, 2008a). In Experiment 2 of Kronmüller and Barr (2007), listeners who heard a different speaker looked more at the target prior to the onset of the referring expression. A plausible reason for this advantage was that listeners expected speakers to refer to something that was new for themselves (the speakers), and because of the structure of the experiment, the target was only speaker-new in the different-speaker condition. Without any correction for this anticipatory effect, the positive slope for the same speaker had to fight against a baseline effect in the opposite direction to be detected the detection threshold. Third, whether or not an effect is detected using a bin-by-bin analysis will depend on where the bins fall relative to the temporal profile of the effect, and studies vary in where they place their bins. Finally, and most obviously, is power: experimenters increase power by increasing sample size but also by controlling variability. For example, unlike other studies, Brown-Schmidt (2009a) used a consistent linguistic template (adjective+noun) for all referring expressions in the test trials. Other studies did not control the form of the referring expressions, possibly

smearing the effect over time, making it harder to detect within a single window.

Regarding the speaker effect for broken precedents, the power to detect the effect for broken precedents may be *inversely* related to sample size, to the extent that speakers who repeatedly break precedents increasingly undermine listeners' assumptions that speakers will be consistent in their future referring behavior. Supporting this, Matthews et al. (2010) found that children initially showed extreme surprise when speakers broke their own precedents, but this effect dropped substantially after only a few trials. If listeners quickly adapt their expectations, increasing the number of broken precedent trials within an experiment may not increase power, but may in fact work against it, since a smaller proportion of trials will be the ones showing the effect.

The social context of the experiment might also explain the disparate results with respect to the speaker effect for broken precedents. In experiments reporting the effect, listeners were led to believe that speakers were naïve participants (Kronmüller & Barr, 2007; Metzing & Brennan, 2003); in other experiments not reporting such effects, the speakers were presented to the participants as experimenters (Brown-Schmidt, 2009a). It seems likely that listeners relax their assumptions about linguistic cooperativity when listening to experimenters than when listening to people they assume to be naïve participants. Under this explanation, listeners would be less surprised by referential inconsistency from an experimenter than from someone assumed to be a naïve participant, minimizing differences between the speakers in the former case.

Finally, discrepancies in the detection of the speaker effect for broken precedents might have a methodological basis. In visual world studies, the target will be selected at different points across trials and across listeners. It does not seem reasonable to include data beyond this selection point, as the interpretation process has ended, and including such data would only add noise that would make it more difficult to detect effects on the trials that have not yet terminated. There are different ways to handle this issue, and these different techniques each may differently impact the detection of later effects. Some

researchers discard all post-selection data, replacing the data frames with surrogate looks to the target object (or whatever object the listener ultimately selected) (e.g., Kronmüller and Barr (2007)). In other words, the missing data is treated as though the listener remained fixated on the target (or other selected object). This approach is usually evident when looking at the data graphs, as the target curve will gradually increase until it asymptotes to 1 (or the probability of choosing the target). The experiments by Brown-Schmidt (2009a) do not appear to have this cumulative character; rather, the curves seem to reach a peak and then drop off, suggesting that a different approach was used. It is beyond the scope of this paper to argue for any one particular approach. We limit ourselves to pointing out that it is not clear whether dropouts were handled equivalently across all three studies, as authors often do not report how they were handled.

Implications for future studies

To close, we consider some general implications of the current investigation for visual-world research on communication in dialogue. First, and perhaps most importantly, theorizing in the study of language use in dialog needs to attend more to relative effect sizes instead of focusing solely on the highest-order effect to reach significance. For example, consider a hypothetical study that finds that over the first 400 ms of processing, the effect of precedent on target advantage scores is reliably larger in the same speaker condition than in the different speaker condition. Now consider the possibility that the individual effects of precedent in the same and different speaker conditions are .25 and .20. Under a purely partner-specific account, where comprehension is completely restricted to common ground, the effect in the different speaker should be zero—however, it is 80% of the size of the effect in the same-speaker condition! Studies that find interactions have generally levied the evidence in favor of partner-specific accounts while ignoring main effects, despite the fact that in terms of effect size, the main effects overshadow the interaction effects by many magnitudes. Getting partner effects to reach significance does

not imply that comprehension is not largely egocentric. Detecting a significant interaction effect may complicate the interpretation of a main effect; however, it does not license one to completely ignore the main effect unless the main effect is entirely driven by the interaction, which is clearly not the case in experiments on precedent use.

Second, attempts to account for discrepant findings across studies have tended toward unidimensional explanations with a selective reading of the literature, while study methodologies and findings differ over a wide variety of dimensions. Some researchers have suggested that speaker-specific effects are only likely to be detected in fully interactive contexts (Brennan & Hanna, 2009; Brown-Schmidt, 2009a; Brown-Schmidt & Hanna, 2011). But such explanations ignore differences in methodology and analysis across experiments, and moreover, only hold up under a selective reading of the literature. For instance, Brennan and Hanna (2009) suggest that their reanalysis shows detects earlier effects of common ground than in Kronmüller and Barr (2007), pointing out that the former used live speakers and the latter used prerecorded speakers. However, Brown-Schmidt (2009a), who used live speakers, failed to detect the different-speaker advantage for broken precedents, except in a post-hoc analysis of one experiment, while Kronmüller and Barr (2007) detected such effects across two non-interactive experiments. Similarly selectivity is apparent in the suggestion by Brown-Schmidt (2009a) that Kronmüller and Barr (2007) failed to find speaker effects for maintained precedents due to lack of interactivity, when the interactive experiments of Brennan and Hanna (2009) and Barr and Keysar (2002) also failed to find the effect. Furthermore, later experiments by Horton and Slaten (2012) detected the same speaker advantage despite being completely noninteractive. Finally, Shintel and Keysar (2007) directly manipulated interactivity, and found no difference in precedent use. In short, interactivity provides a poor explanation for the variation in outcomes of the experiments on precedent use. Progress will require considering a broader range of methodological and analytical differences such as those that we signaled in Table 1, as well as efforts to explanation the totality of findings, rather than

selective interpretation based on unidimensional differences across experiments.

Finally, we hope to have convinced readers that the combination of data across visual world studies holds great promise for improving knowledge accumulation in the study of dialogue. To support future meta-analyses, researchers using the visual world paradigm should include probability curves in reports of their findings, rather than just providing aggregate scores over specific time windows. Ideally, researchers should make their data available in public repositories not only to support later meta-analysis, but so that their findings can be independently verified. This would enable the field to advance at a greater pace toward a consensus regarding the nature of discourse effects in spoken language comprehension.

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