

How Do Communicative Cues Shape the Way That Dogs (*Canis familiaris*) Encode Objects?

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Our human capacity to efficiently learn from other individuals is unparalleled in any nonhuman species. Some scholars argue that our propensity to learn socially is supported by an early-emerging expectation that communicative cues will convey generic information (Csibra & Gergely, 2011). In the current 2 studies, we examine whether this expectation about generic information is unique to humans by testing a species that readily attends to human cues—dogs. Specifically, we adapted a violation of expectation paradigm previously used with human infants to examine whether communicative cues lead dogs to selectively encode generic, kind-relevant information about objects (e.g., shape). Prior work has demonstrated that human infants are more likely to notice unexpected changes in kind-relevant information in communicative contexts (i.e., when an agent points to the object; Yoon et al., 2008). In contrast, across 2 studies ($N = 136$), dogs were no more likely to notice kind-relevant changes in communicative contexts than noncommunicative contexts. These findings suggest that although dogs attend to human communicative cues, such cues do not shape the way that dogs encode objects. More broadly, this finding lends support to the claim that our early-emerging generic expectation crucially supports our human capacity to efficiently learn from one another.

Keywords: pedagogy, dogs, generics, violation of expectation

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Humans have evolved an incredible ability to transmit complex information between individuals and across generations. Although a wide range of species across the animal kingdom teach and learn from one another (Caro & Hauser, 1992; Kline, 2015; Skerry et al., 2013; Thornton & Raihani, 2008), human teaching and learning is more complex than that of other species. In addition to learning how to navigate our surrounding physical environments (e.g., where to find food and what to eat; Birch & Doub, 2014; Higgs, 2015; Liberman et al., 2016; Wertz & Wynn, 2014), humans must also learn to interact with complex tools, rituals, and customs unique to our cultural environments (Csibra & Gergely, 2011; Legare & Nielsen, 2015; Tomasello, 1999).

Which components of human cognition support our ability to learn this complex cultural information? Although many elements of human cognition must come together to support this ability, one feature that makes the process of learning cultural information highly efficient, and possibly unique (Csibra & Gergely, 2011), is our ability to learn from the intentional instruction of others. Direct teaching helps the youngest members of our society to quickly and accurately extract information about the world that might otherwise be unattainable through either observational learning or direct manipulations on the physical world (Csibra & Gergely, 2011; Shafto et al., 2012).

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Although the specific cognitive capacities underlying our ability to learn from intentional instruction are still under debate, there is growing evidence that infants (and older learners) learn from others via a system of *natural pedagogy* (Csibra & Gergely, 2009; Csibra & Gergely, 2011; Csibra & Shamsudheen, 2015). This system of natural pedagogy is characterized by a rich set of expectations regarding both *how* and *what* others will teach. With respect to *how* others teach, human infants seem to expect that adults will use certain communicative cues—specifically ostensive cues (e.g., eye contact and infant-directed speech)—to signal their intention to teach information (Csibra, 2010; Csibra & Gergely, 2009; Senju & Csibra, 2008). Infants—even newborns (Farroni et al., 2002; Vouloumanos & Werker, 2007)—are highly attuned to these ostensive cues in early development (Behne et al., 2005; Senju & Csibra, 2008; Topál et al., 2014; Vouloumanos et al., 2014).

Although this sensitivity to ostensive communicative cues is an integral part of learning from intentional instruction, this capacity alone cannot explain humans' unique ability to transmit complex cultural knowledge. Other species—most notably domesticated dogs (*Canis familiaris*)—show a heightened sensitivity to human ostensive cues (Hare & Tomasello, 2005; Hare et al., 2002; Johnston et al., 2015; Lakatos et al., 2009; Miklósi & Soproni, 2006; Téglás et al., 2012; Topál et al., 2014). Specifically, dogs can develop a heightened sensitivity to the same ostensive cues that human infants follow when raised in an environment rich in human interaction (e.g., high-pitched infant-directed speech and eye contact; Kaminski et al., 2012; Téglás et al., 2012; Topál et al., 2009, 2014). These findings suggest that a sensitivity to ostensive cues is not sufficient to support uniquely human cultural learning.

But human infants show more than just a mere sensitivity to ostensive cues. In addition to their expectations about *how* others will teach them (i.e., via ostensive cues), infants also have expectations about *what* these ostensive cues will convey (Csibra & Gergely, 2009; Csibra & Gergely, 2011; Csibra & Shamsudheen, 2015). At the most basic level, infants seem to expect that ostensive communication will be *referential* (i.e., referring to something external to the communicator; Csibra, 2010; Csibra & Shamsudheen, 2015). This expectation has been demonstrated in several studies showing that young infants expect referential signals (e.g., eye gaze, pointing, etc.) to refer to things in the world, but only if these referential signals are preceded by ostensive cues. For instance, 4-month-old infants successfully use a person's gaze to look more quickly at an object that appears on a screen, but do so only if the gaze is preceded by direct eye contact (Farroni et al., 2003). Likewise, 8-month-olds will preferentially look at an object indicated by someone's gaze, but only if the gaze is preceded by eye contact, infant-directed speech (Senju & Csibra, 2008), or contingent responsivity (Deligianni et al., 2011). Thus, by 8 months of age, infants seem to expect that ostensive cues will provide referential information about the world.

But is the expectation that ostensive cues will be referential unique to humans? Recent work with dogs suggests not. Similar to human infants (Deligianni et al., 2011; Farroni et al., 2003; Senju & Csibra, 2008), dogs are more likely to attend to referential cues when they are prefaced by communicative cues such as eye contact or high-pitched speech (Duranton et al., 2017; Miklósi et al., 1998; Soproni et al., 2001; Téglás et al., 2012). For example, dogs will only look preferentially at an object indicated by a human's

gaze if the gaze is preceded by ostensive cues (i.e., eye contact and high-pitched speech), rather than nonostensive attention-getters (i.e., salient moving images and adult-directed speech; Téglás et al., 2012). Similarly, there is evidence that dogs search more often in a location indicated by an experimenter's pointing if the experimenter first establishes eye contact before pointing (Kaminski et al., 2012). Finally, dogs are more likely to track the direction of a human's gaze if the human's gaze is preceded by ostensive cues *and* directed at a referent (e.g., a potential treat hiding location), rather than into distant space (Duranton et al., 2017). Thus dogs, similar to human infants, selectively follow referential signals when they are preceded by ostensive cues, and they track the direction of these cues more successfully when they are targeted at a referent. Together, these findings suggest that dogs expect human ostensive cues will provide referential information in much the same way as human infants.

Overall, then, it seems that dogs learn from human instruction in much the same way as human infants. Similar to human infants, dogs expect human instruction will be conveyed through ostensive cues (Kaminski et al., 2012; Téglás et al., 2012; Topál et al., 2009; 2014) and provide referential information (Duranton et al., 2017; Kaminski et al., 2012; Soproni et al., 2001; Téglás et al., 2012). However, some scholars suggest that human infants have one additional expectation about ostensive communication that may not be shared with dogs: the expectation that ostensive communication will provide *generic* information that generalizes across members of a particular kind (Csibra, 2010; Csibra & Gergely, 2011; Csibra & Shamsudheen, 2015; Gergely & Csibra, 2013). Kinds are categories with rich inductive potential (Xu, 2005) that share important underlying properties (Gelman, 2003). For instance, “balls,” “ropes,” and “bowls” represent three distinct *kinds* of objects that share underlying properties (roundness for balls, tensile strength for ropes, and containment for bowls). Importantly, these shared properties provide the basis for generalization across diverse members of the same kind (e.g., that balls roll, ropes pull, and bowls hold things). The proponents of natural pedagogy propose that infants expect ostensive communication to convey generic knowledge that not only applies to the specific object being communicated about, but to all members of that object's kind (Csibra & Gergely, 2011; Csibra & Shamsudheen, 2015). Note that this expectation makes learning more efficient because it allows learners to leverage the information they learn in one context and apply it to novel situations. By expecting ostensive communication to provide generic knowledge, learners can drastically reduce the required amount of episodic learning.

Although it is difficult to directly test whether infants expect ostensive communication to generalize across kinds, there is some evidence that human learners pay more attention to kind-relevant features of objects (e.g., function, shape) than low-level perceptual features (e.g., color, location) when addressed with ostensive communication (Futó et al., 2010; Träuble & Bätz, 2014; Yoon et al., 2008). This pattern of performance suggests that ostensive communication draws infants' attention to features of an object that are more diagnostic of an object's kind, thus providing a better basis for generalization. For example, although 9- and 10-month-olds typically encode spatiotemporal information about objects (Xu, 2002; Xu & Carey, 1996), they begin to encode information about kind-relevant properties, such as function (Futó et al., 2010) or shape (Yoon et al., 2008;

for additional evidence, see [Chen et al., 2011](#); cited in [Gergely & Csibra, 2013](#)) when ostensively cued.

Additional evidence that humans expect ostensively communicated information to be generic comes from work with older infants and preschoolers showing that kind-relevant information gathered from ostensive communication is resilient in the face of counterevidence. In one study, 13 five-month-olds were ostensively shown that a tool had a particular function (e.g., peeling bananas) and then immediately afterward shown (nonostensive) counterevidence that the tool was used for a different function (e.g., wrapping up bananas; [Hernik & Csibra, 2015](#); Experiment 4). Although infants had evidence that the tool was used for both functions, they looked longer on subsequent trials in which the tool was used in line with the nonostensive function; importantly, this pattern was not observed when the demonstrator used the tool without ostensive communication; [Hernik & Csibra, 2015](#); Experiment 4). Similar work with preschool children suggests that ostensive communication leads preschoolers to categorize objects based on kind-relevant features; when learning about novel types of objects from ostensive communication, preschoolers more frequently persist in trying to make an object perform a category-defining function when it is broken ([Butler & Markman, 2012](#)) and categorize objects based on their function rather than salient perceptual features ([Butler & Markman, 2014](#); for similar evidence with 12-month-old infants, see [Kovács et al., 2011](#); cited in [Gergely & Csibra, 2013](#)). Thus, by preschool, children seem to actively attend to kind-relevant features when learning about objects via ostensive communication.

But is this focus on generic information unique to human social learning or do dogs share this tendency as well? Initial work suggests that dogs may not share the expectation that ostensive cues will communicate generic information ([Fugazza et al., 2016](#); [Tauzin et al., 2015](#); [Topál et al., 2009](#)). One study that directly compared dogs and human infants found that while human infants seemed to expect ostensive cues would provide generic information, dogs seemed to expect ostensive cues to provide imperative commands ([Topál et al., 2009](#)). This study used an A-not-B task in which a demonstrator initially ostensively demonstrated that an object was in Location A. Following these ostensive A trials, the object was visibly hidden in Location B. [Topál and colleagues \(2009\)](#) then tested whether subjects continue to look in the ostensively cued Location A or whether they switched to the correct Location B and crucially whether subjects' behavior was influenced by the presence of the initial demonstrator. Human infants continued to search in the ostensively cued Location A regardless of whether the initial demonstrator was present or not, suggesting that they both generalized the ostensively communicated information and that they continued to generalize in this way regardless of whether the initial demonstrator was present or not. In contrast, dogs only searched in the ostensively cued Location A if the initial demonstrator was present. In this way, dogs did not demonstrate a human-like expectation that ostensive information will generalize. Instead, they seemed to treat the ostensive cues like a command, which they only needed to follow if the original person who conveyed the command was present. When the commander left the room, dogs searched randomly. Together, these findings suggest that dogs may see ostensive communication as a command and more broadly that they do not share a human-like expectation that ostensive communication will generalize.

Unfortunately, it is possible that the specific method used in this study masked dogs' *expectations* about human communication. Prior work has demonstrated that dogs often respond to human ostensive cues as though they are commands ([Topál et al., 2009](#)). Because of this automatic response to a human's commands, dogs tested in this A-not-B study may appear as though they fail to share human infants' generic expectations about ostensive cues due to a performance issue rather than a competence issue. To get around this, we examined whether dogs would show evidence of a generic expectation for ostensive communication using a nonbehavioral method. Specifically, we tested dogs in a looking time methodology developed to test human infants' expectations that ostensive communication will convey generic information ([Yoon et al., 2008](#)).

In their prior study, [Yoon and colleagues \(2008\)](#) presented 9-month-old infants with an event in which an experimenter cued an object using either ostensive (e.g., pointing and infant-directed speech) or nonostensive cues (e.g., reaching). The researchers then tested the features infants remembered about this object by changing either the objects' identity (i.e., it changed to a new kind of object) or its location. When infants were cued to an object by nonostensive cues, they were more sensitive to location changes than to identity changes ([Yoon et al., 2008](#)). In contrast, when infants were cued to an object by ostensive cues, they were more sensitive to kind changes than location changes, suggesting that ostensive cues increase infants' attention to kind-relevant features over location (a finding that is observed in adults as well; [Mamo et al., 2014](#)). Together, these results suggest that ostensive contexts cause infants to pay more attention to kind-relevant and generalizable information, thus demonstrating an expectation that ostensive communication will convey generalizable information.

However, these results should be interpreted with some caution, given that a group of researchers recently failed to replicate the original study by [Yoon et al \(2008\)](#); [Silverstein et al., 2019](#)). In two experiments, [Silverstein et al. \(2019\)](#) used a method similar to that of [Yoon et al. \(2008\)](#) study with a few exceptions. Most notably, the researchers controlled for the amount of time the ostensive and nonostensive cues were presented, which was not done in the original [Yoon et al. \(2008\)](#) study in which ostensive cues were presented for a longer amount of time. [Silverstein et al. \(2019\)](#) failed to find any evidence that infants were more sensitive to kind changes in the ostensive condition and location changes in the nonostensive condition. Instead, they found that infants were more likely to notice kind changes across both ostensive and nonostensive contexts. However, even this result only occurred in their first study on one measure of looking time (i.e., duration of first look) but not the other (i.e., total looking time). Consequently, this recent failed replication has cast doubt on whether infants truly demonstrate an expectation that ostensive communication conveys generic information.

To investigate whether dogs demonstrate an expectation that ostensive communication conveys generic information, we adapted [Yoon and colleagues' \(2008\)](#) infant looking time method for use with dogs. To date, a number of studies have successfully used looking time methods to test dogs ([Adachi et al., 2007](#); [Marshall-Pescini et al., 2014](#); [Pattison et al., 2010](#); [Pattison et al., 2013](#); [West & Young, 2002](#)). For example, looking time methods have revealed that dogs are able to discriminate objects based on color and size ([Pattison et al., 2013](#)), able to perceive human actions

as goal-directed (Marshall-Pescini et al., 2014), and have expectations about numerical quantities (West & Young, 2002).

In two experiments, we investigated whether dogs would exhibit longer looking durations at kind-relevant changes (e.g., object changes) than location changes in ostensive contexts versus nonostensive contexts. If dogs, similar to human infants, show a modulation in their looking times based on ostensive cues, such performance would suggest that they share a human-like expectation that ostensive communication will convey generic information. More broadly, this pattern of results would suggest that humans are *not* unique in expecting ostensive communication to generalize. In contrast, if dogs fail to show modulation in looking time based on ostensive cues, this would suggest that—in contrast to human infants—dogs do not expect ostensive communication to convey generic information (Topál et al., 2009). More broadly, this pattern of results would provide initial evidence suggesting that humans may be unique in our tendency to expect ostensive cues to generalize, further supporting the claim that this generic expectation may uniquely support our complex human culture (Csibra & Gergely, 2011).

Experiment 1

Subjects

We tested 88 pet dogs (*Canis familiaris*; 41 males; $M_{\text{age}} = 4.96$; $SD_{\text{Age}} = 2.68$) of varying breeds (see Table S1 in the online supplemental materials). Eighteen additional dogs were tested but excluded due to being too difficult to code ($n = 9$; because of dark fur or fur in front of their eyes), being unwilling to look at the study presentation (six), experimenter error (two), or owner interference (one). All dogs were pets whose owners volunteered for participation by entering their dogs' information in an online database. Subjects were required to show no aggressive tendencies, be up-to-date on all their vaccinations, and be older than 4 months of age. Before participation, all dogs visited the center at least once before testing to become familiar with the center.

Method

Materials and Setup

Testing was conducted in a large 3.5 m \times 3.15 m room in our center. The testing materials consisted of an occluder 35.5 cm \times 28.4 cm, a tray, and four toys. Our toys included a colorful, rubber spikey toy, a plush blue bear wearing a pink neck kerchief, a red and white rope toy with two red balls attached, and a colorful plush star-shaped toy.

Dogs were tested in the presence of two people: an owner and an experimenter. For the duration of the experiment, dogs sat at their owner's feet, approximately 1.66 m from the experimenter. Dogs' owners were instructed to keep their eyes closed for the duration of the experiment while holding their dog in place with a leash. Dogs were filmed using an overhead camera and a tripod camera. The footage from the tripod camera was used to code dogs' looking time.

Design and Procedure

All subjects participated in six trials: three familiarization trials and three test trials. The familiarization trials and test trials alternated

such that each subject received a familiarization trial followed by a test trial, followed by another familiarization trial, and so on.

Familiarization Trials. On the familiarization trials, subjects were divided into two conditions: the *ostensive condition* and the *nonostensive condition*. We used the same ostensive and nonostensive cues that Yoon and colleagues (2008) used with infants. Specifically, in the ostensive condition, the experimenter lifted the occluder, pushed the tray out with the toy on it and then made eye contact with the dog, calling its name, and pointing at the object. The ostensive familiarization trials lasted approximately 9 s. In the nonostensive trials, the experimenter *avoided* eye contact with the dog, reached toward the toy, and made an "Ooh!" sound to get the dog's attention. The nonostensive familiarization trials lasted approximately 9 s. These attention-getting cues were repeated twice in both conditions. See Video S1 in the online supplemental materials for a video clip of the method. We used the same condition for all three familiarization trials for any given subject; if a subject started off with a familiarization trial where the experimenter pointed (e.g., in the ostensive condition) at a blue bear toy on the right side of the tray, then that was the familiarization trial they saw throughout the experiment.

Test Trials. All subjects participated in three test trials: a *no change* trial in which the same toy stayed on the same side after the familiarization trial (Figure 1c), a *location change* trial in which the identity of the toy remained the same, but it moved to the opposite side of the tray after the familiarization trial (see Figure 1d), and a *kind change* trial in which the identity of the toy changed to its paired toy after the familiarization trial but remained on the same side (Figure 1e). At the beginning of each test trial, the experimenter lifted the occluder and pushed the tray out, all while staying fully hidden behind the occluder (Figure 1b). Once the tray was in place, the experimenter began a 15-s countdown to ensure dogs had sufficient time to look at the test trial display. To keep the intertrial duration and sound cues uniform regardless of condition, the experimenter always moved a toy behind the occluder before putting the toy in its final placement on the tray.

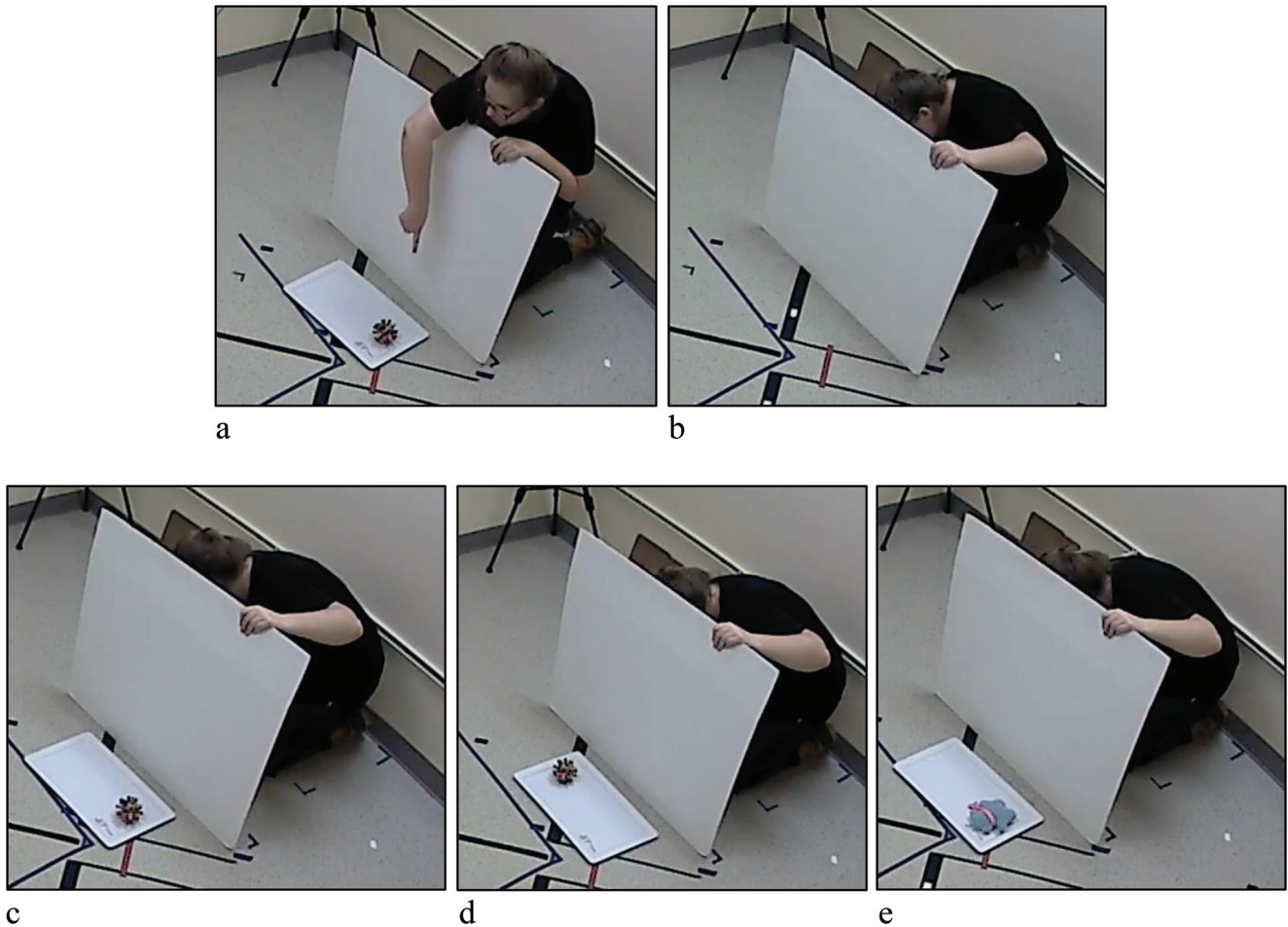
We counterbalanced (a) the order of the three test trials, (b) the toy used in the familiarization trials, and (c) the side the toy appeared on in the familiarization trials across dogs.

Coding and Analysis

Looking time for all trials were coded by two coders (Michael J. Bogese and Alyssa M. Arre), each of whom were blind to condition and trial type while coding. A dog was coded as looking at the display if their eye gaze was directed toward any part of the tray on which the two toys were placed. Out of the 88 dogs which we included in the final analysis, an additional eight trials were excluded from analysis because the dog's eyes were not fully visible (i.e., due to the dog sitting behind the owner's legs, having fur in front of its eyes, etc.). Reliability was high for the remaining trials, $r = 92\%$. All analyses were conducted with both coders' values and were the same regardless of which set of coding used. All analyses and figures report data from Michael J. Bogese's coding.

Statistical analyses were conducted using R statistical software (Version 3.6.1, R Foundation for Statistical Computing, Vienna, Austria). Looking time was analyzed using a linear mixed model (LMM), as the transformed response variable had a normal error

Figure 1
Experimental Setup for Experiment 1



Note. The experimenter points during a familiarization trial in the ostensive condition (a). Then the experimenter pulls the object and tray behind the occluder (b) before bringing the tray back out for the test trials. There were three types of test trials in Experiment 1: no change to the object or location (c), location change where the object swapped sides (d), and kind change where the object was replaced with a different object on the same side (e). See the online article for the color version of this figure.

distribution. Predictors of interest were condition (ostensive or nonostensive), trial type (no change, kind change, or location change), trial number, and age. To control for repeated measures, subject identity was included as a random effect. All mixed models were run using R package “lme4” (Bates et al., 2012).

In mixed model analyses, we first examined a null model, which included only subject identity. We then compared the null models with full models that included all predictor variables and their interactions. Model comparisons were conducted with likelihood ratio tests. Based on the initial results of the LMM we then conducted follow-up *t*-tests comparing looking time in (a) kind trials to no change trials and (b) location trials to no change trials.

Results

Our model for looking time revealed an effect of trial number (Likelihood Ratio Test [LRT]: $\chi^2 = 6.84, p = .009, R^2 = .02$) and trial type (LRT: $\chi^2 = 16.00, p < .001, R^2 = .04$). No other factors or

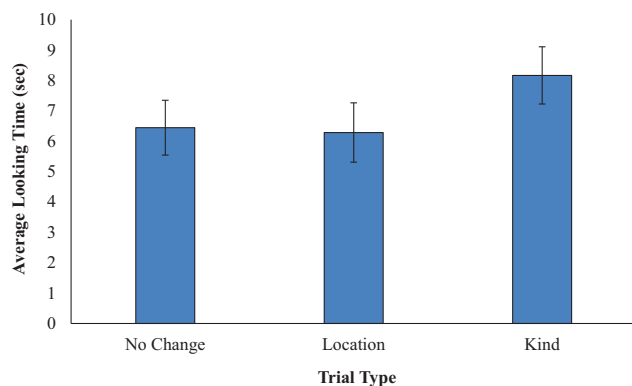
interactions—including those pertaining to the effect of ostension—were significant predictors (LRT: $ps > .11$). Thus, unlike human infants (Yoon et al., 2008), dogs showed no difference in their looking time based on ostensive cues.

As shown in Figure 2, dogs looked longer at kind change test trials ($M = 8.17, SD = 4.42$) than at no change test trials ($M = 6.45, SD = 4.15$), $t(82) = 3.46, p < .001, d = .38$, but we did not observe any statistical difference in looking between location change ($M = 6.29, SD = 4.53$) and no change trials, $t(80) = .54, p = .591, d = .06$. As shown in Figure 3, the effect of trial number was characterized by an overall decrease in looking time across trials.

Discussion

We found that dogs looked longer on kind change trials compared to no change trials, but looked at location change and no change trials at similar levels. Moreover, we found that dogs' looking time was not modulated by ostensive cues. In contrast to

Figure 2
Average Looking Time (in Seconds) Across Trial Types for Experiment 1



Note. Error bars indicate 95% confidence intervals. See the online article for the color version of this figure.

infants tested in a previous study (Yoon et al., 2008; but see Silverstein et al., 2019), dogs noticed kind changes in both ostensive and nonostensive contexts and failed to notice location changes in either context. Thus, dogs' ability to encode kind-relevant information may not be modulated by ostensive cues in the same way that infants' ability is. More broadly, this result provides additional evidence that dogs do not specifically expect ostensively communicated information to generalize in the same way as infants.

That said, it is possible that dogs detected kind-relevant changes in both the ostensive and nonostensive conditions in Experiment 1 because they interpreted the cues we used in the nonostensive condition as being ostensive. Although we used the same cues in Experiment 1 that Yoon and colleagues (2008) used in their study with infants, it is possible that dogs interpreted the reaching hand and/or the nonostensive attention getting sound (i.e., "Oooh") as being ostensive, especially given that the nonostensive "Oooh" was delivered in a high-pitched voice. To address this potential issue, we ran a second experiment in which we accentuated the distinction between the ostensive and nonostensive cues. In particular, we made the nonostensive condition more starkly nonostensive by having the demonstrator use (a) a wooden stick rather than a reaching hand to point at the object during the familiarization trials and (b) neutral tongue clicking, instead of a high-pitched "Oooh," as an auditory cue. If dogs continue to detect kind changes at the same rate for the ostensive and nonostensive conditions in Experiment 2, then this would provide even stronger evidence that ostensive cues do not modulate dogs' tendency to encode kind-relevant information about objects.

Moreover, given that dogs failed to notice the location change in either the ostensive or nonostensive conditions, we also made a few adjustments in Experiment 2 to make the location change more salient. Specifically, we worried that dogs may have failed to notice the location changes because they were not truly impossible. Note that in Yoon and colleagues' (2008) infant study, the location change appeared to happen "magically" in plain sight; specifically, an object seemed to disappear from one box and reappear into another. This change appeared impossible because the infants were able to see both boxes during the entirety of the experiment, so there would be no way for the object to move from one box to the other without the

infants witnessing it. In contrast, in our study, the object was pulled behind an occluder in between trials, making it entirely possible that the experimenter moved the object when it was out of the dog's sight. To address this concern in Experiment 2, we changed the apparatus so that location changes would appear impossible to dogs, in the same way they were in Yoon and colleagues (2008).

Experiment 2

Subjects

We tested a new group of 48 dogs (24 males; $M_{\text{age}} = 6.49$; $SD_{\text{Age}} = 3.53$; see Table S1 in the online supplemental materials) that had not participated in Experiment 1. Two additional dogs were tested but excluded due to the dog being unwilling to look at the study presentation (one) or owner interference (one). All dogs were recruited from the same database described in Experiment 1.

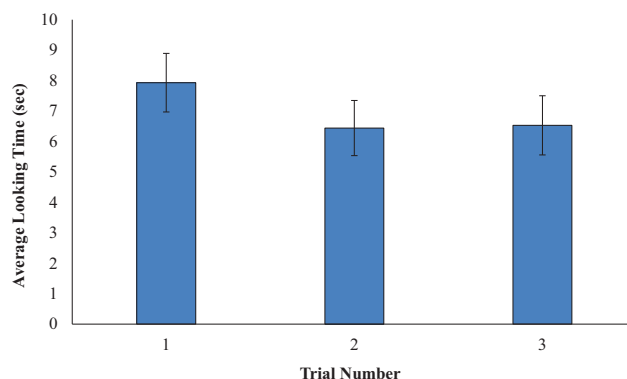
Method

Materials and Setup

Testing for Experiment 2 was conducted in a small room 3.8 m \times 2 m in our center. We built a new looking time apparatus that consisted of a back occluder 35.5 cm \times 28.4 cm, a stage 35.5 cm \times 13.7 cm, and two black boxes on either side of the stage 12.7 cm \times 12.7 cm. See Figure 4. The boxes were separated from each other by 12.7 cm to make it clear it would be impossible for an object to move from one box to the other without passing visibly through the 12.7 cm open space between the two boxes. For Experiment 2, we used the rope toy and plush star-shaped toy pairing from Experiment 1 (see Figures 4c and 4e). We chose to use this particular pairing for Experiment 2 because it was the most visually distinct pair.

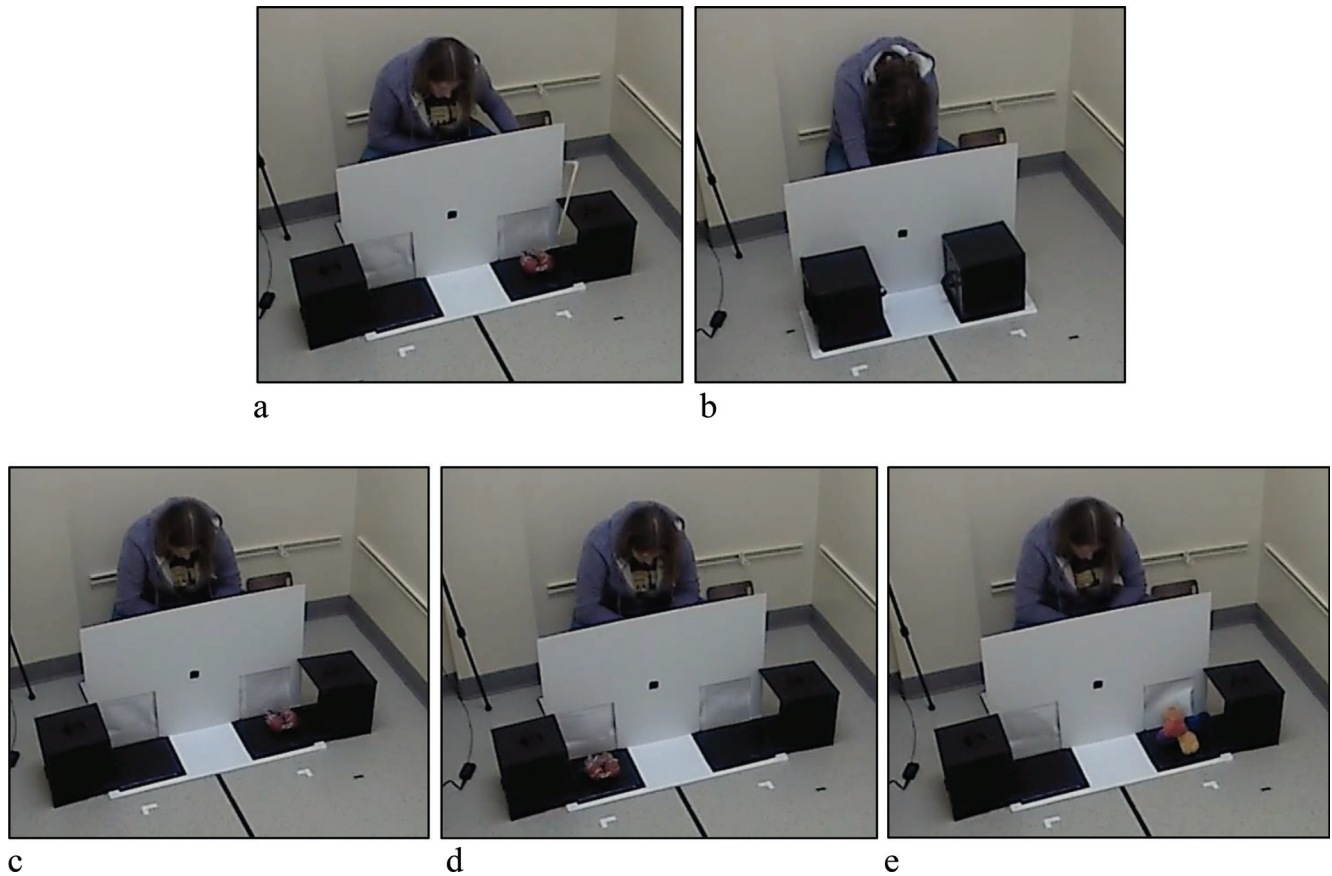
As in Experiment 1, dogs were tested in the presence of two people: their owner and an experimenter. During the duration of the experiment, dogs sat at their owner's feet approximately 2 m from the experimenter. As in Experiment 1, dogs' owners were

Figure 3
Average Looking Time (in Seconds) Across Trial Number for Experiment 1



Note. Error bars indicate 95% confidence intervals. See the online article for the color version of this figure.

Figure 4
Experimental Setup for Experiment 2



Note. The experimenter uses a stick to point at the object in a familiarization trial in the nonostensive condition (a). Then the experimenter closes the boxes in between the familiarization trials and the test trial (b) before reopening the boxes for the test trials. There were three types of test trials in Experiment 1: no change to the object or location (c), location change where the object appeared in the other box (d), and kind change where the object was replaced with a different object in the same box (e). See the online article for the color version of this figure.

instructed to keep their eyes closed for the duration of the experiment while holding their dog in place with a leash.

We filmed the dogs' behavior from three separate locations. The back occluder on the apparatus had a small hole cut in the middle of it so that a GoPro camera could film dogs' faces at high resolution. In addition to the GoPro camera, we also coded dogs' behavior using an overhead camera and a third tripod camera behind the experimenter. When possible, the GoPro footage was the primary footage the coders used to code the dogs' looking time. In rare instances of GoPro camera malfunction, the tripod angle was used to code the dogs' looking time.

Design and Procedure

The design and procedure of Experiment 2 were the same as that of Experiment 1 with two notable exceptions. First, on location change test trials, the experimenter would swap the toy from one box to the other through a false back on each of the boxes (covered in white cloth; Figure 4a). Specifically, the experimenter would open the black boxes to begin the familiarization trial (Figure 4a) and then close the boxes at the end of the

familiarization trial (Figure 4b). Although the boxes were closed in front, the experimenter would pull the toy from the cloth backside of one box and put it in the cloth backside of the other box. As in Experiment 1, we made sure the intertrial duration and auditory cues were kept identical across trials. To do this, the experimenter always stuck her hands in the backside of both boxes and pulled out the toy before either replacing it or changing it. This ensured any subtle movements of the box or auditory cues were the same across all trial types.

The second difference between Experiments 1 and 2 was in the nonostensive demonstration. In Experiment 2, we made the nonostensive condition more clearly nonostensive. Specifically, rather than the experimenter reaching toward the object and saying "Ooh!" in a high-pitched voice, the experimenter looked down at her feet and used a wooden stick to point at the object while making a neutral clicking noise with her tongue (see Figure 4a and Video S1 in the online supplemental materials). The ostensive familiarization trials took approximately 9 s, and the nonostensive familiarization trials took approximately 16 s.

Moreover, to improve the codability of the videos, the experimenter oriented the camera at the end of the experiment by getting the dog to look at each of the boxes and the experimenter's face. This was done to make it clearer where the edges of the apparatus were for the coders.

Coding and Analyses

As in Experiment 1, looking time was coded by two coders (Michael J. Bogese and Alyssa M. Arre) who were each blind to condition and trial type. A dog was coded as looking at the display if their eye gaze was directed toward any part of the apparatus. Three trials were excluded from analysis due to experimenter error ($n = 2$) or because the dog's eyes were not fully visible ($n = 1$). Reliability was high for the remaining trials, $r = 93\%$. Statistical analyses were conducted in the same way as in experiment 1. Based on the initial results of the LMM we then conducted follow-up t -tests comparing looking time in (a) kind trials to no change trials and (b) location trials to no change trials. Moreover, based on the initial results of the LMM, we also compared looking time between the ostensive and nonostensive conditions for each of the three trials. All analyses were conducted with both coders' values and were the same for both sets of codes, except where indicated. All analyses and figures report data from Michael J. Bogese's coding.

Results

Our model for looking time revealed that subjects' looking time was predicted by trial type (LRT: $\chi^2 = 8.48$, $p = .014$, $R^2 = .02$) and an interaction between condition and trial number¹ (LRT: $\chi^2 = 6.98$, $p = .008$, $R^2 = .01$). Moreover, there was a three-way interaction between age, trial type, and trial number (LRT: $\chi^2 = 6.21$, $p = .045$, $R^2 = .07$) as well as a trend toward an interaction between age and condition (LRT: $\chi^2 = 3.66$, $p = .056$, $R^2 = .11$)². Although the three-way interaction is hard to interpret, the trending interaction between age and condition showed that older dogs looked longer in the nonostensive condition than in the ostensive condition (see Figure S1 in the online supplemental materials). No other factors or interactions were significant predictors (LRT: $ps > .30$).

As shown in Figure 5, dogs looked longer when there were kind changes ($M = 7.20$, $SD = 4.73$) than when there was no change ($M = 5.58$, $SD = 4.78$), $t(45) = 2.91$, $p = .005$, $d = .43$, but showed no statistical difference in looking between location ($M = 5.86$, $SD = 4.45$) and no changes, $t(44) = 1.02$, $p = .311$, $d = .15$.

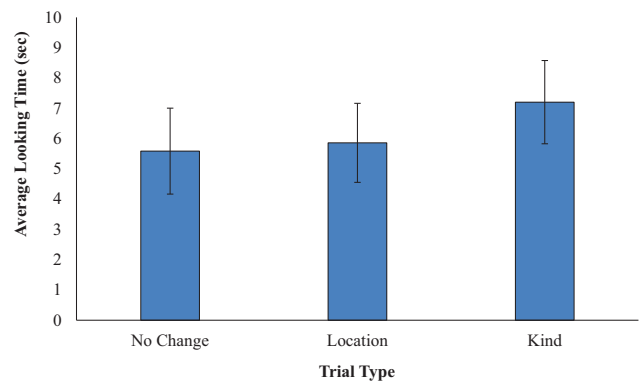
To unpack the interaction between condition and trial number, we compared looking time between conditions for each trial. We used a Bonferroni correction (adjusted $p = .017$) to correct for multiple comparisons. On Trial 1, dogs looked longer in the nonostensive condition than on the ostensive condition, $t(41.60) = 2.82$, $p = .007$, $d = .84$.³ There were no differences between condition on Trials 2 and 3 ($ts < 1.40$, $ps > .17$). See Figure 6.

Discussion

To better understand the results of Experiment 1, we made two main methodological changes in Experiment 2. Specifically, we made the nonostensive cues more drastically nonostensive and made the location changes impossible. Nonetheless, just as in Experiment 1, dogs tested in Experiment 2 looked significantly longer at kind changes despite failing to notice location changes in either the ostensive or nonostensive conditions.

Figure 5

Average Looking Time (in Seconds) Across Trial Types for Experiment 2



Note. Error bars indicate 95% confidence intervals. See the online article for the color version of this figure.

Together, these findings suggest that ostensive cues did not modulate dogs' ability to detect either kind or location changes. In contrast, dogs seem to notice kind changes in both ostensive and nonostensive contexts and fail to notice location changes in either context. These findings are in contrast to those of human infants who are more likely to notice kind changes and less likely to notice location changes in ostensive contexts. Thus, it seems that dogs are not more likely to encode kind-relevant, generalizable features of objects in ostensive contexts, providing further evidence that dogs do not expect ostensively communicated information to generalize in the same way as human infants.

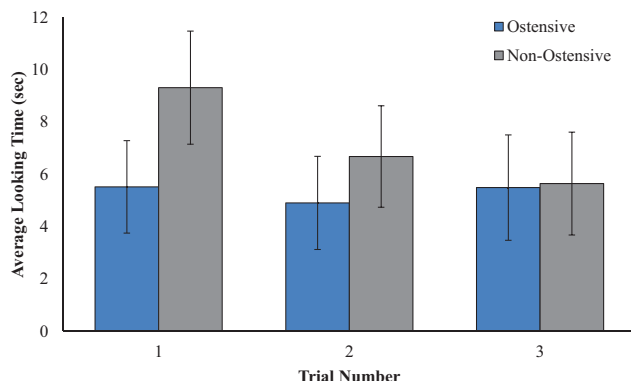
The one factor that ostensive cues did appear to impact was dogs' looking time across trials. Specifically, dogs looked longer on Trial 1 in the nonostensive condition than the ostensive condition. Given that we did not observe a similar interaction between ostensive condition and trial number in Experiment 1, dogs may have looked longer on Trial 1 in the nonostensive condition of Experiment 2 because they were focused on the stick used in the presentation. However, it is important not to read too much into this interaction given that it was only significant in one coder's coding. In the other coder's coding, there was simply a main effect of trial number, which did not interact with condition. Together, these results clearly suggest that ostensive cues did not impact dogs' ability to detect kind or location changes; the only outcome ostensive cues may have impacted was dogs' interest in the displays across trials.

¹ Only coder MB's coding values revealed a significant interaction between condition and trial number. Coder AA's coding revealed a main effect of trial number (LRT: $\chi^2 = 7.61$, $P = .006$, $R^2 = .02$), and no main effect or interaction involving condition (LRT: $Ps > .15$).

² Only coder MB's coding values revealed a significant three-way interaction and a trend towards an interaction between age and condition. Coder AA's coding revealed a trend towards a main effect of age (LRT: $\chi^2 = 3.28$, $P = .098$, $R^2 = .04$), and no interactions involving age (LRT: $Ps > .12$).

³ This effect was only marginal for the second coder ($p = .070$).

Figure 6
Average Looking Time (in Seconds) Across Trial Number and Condition for Experiment 2



Note. Error bars indicate 95% confidence intervals. See the online article for the color version of this figure.

General Discussion

Across two studies, dogs noticed kind-relevant changes despite failing to notice location changes. This pattern of results was true regardless of whether dogs learned about objects ostensively or nonostensively. These findings contrast with those of human infants who selectively noticed kind-relevant changes in ostensive contexts, suggesting that these infants expected ostensively communicated information to generalize (Yoon et al., 2008; but see Silverstein et al., 2019). The findings of the current looking time studies add to existing behavioral work with dogs (Topál et al., 2009), suggesting that dogs do not expect ostensive communication to generalize in the same way as human infants.

Given that dogs—a species that is highly sensitive to human ostensive cues (Hare & Tomasello, 2005; Téglás et al., 2012; Topál et al., 2014)—do not expect ostensive communication to generalize in the same way as human infants in prior work (Yoon et al., 2008), what does this say about the origins of human pedagogy? We believe this pattern of performance provides initial evidence—pending results from other species—for the claim that our tendency to expect ostensive communication to generalize may be unique to humans (Csibra & Gergely, 2011) and that such an expectation may support our uniquely complex culture by increasing the efficiency of human instruction. By expecting ostensively communicated information to generalize, human learners are able to learn a vast amount of information in one learning episode (e.g., all hammers hit nails), rather than needing to learn each piece of information individually on a trial-by-trial basis.

That said, it is crucial to emphasize that these conclusions about human pedagogy remain tentative for now. As described earlier, a group of researchers (Silverstein et al., 2019) failed to replicate the original finding that human infants attend to kind changes more often when objects are cued ostensively (Yoon et al., 2008). Interestingly Silverstein and colleagues (2019) found some evidence that infants—like dogs in our studies—were more likely to notice kind changes than location changes across both ostensive and nonostensive contexts. Although it is unclear why dogs and infants noticed only kind changes and not location changes, it is possible that dogs and infants wanted to interact with the objects (in the case of

dogs) or witness the experimenter interact with the objects (in the case of infants) and thus were more focused on *what* the object was at the time of presentation, rather than precisely *where* the object was. More work could examine this kind over location preference by using objects that are less exciting, and that dogs and infants would be less motivated to manipulate.

Although the current work provides converging evidence with prior behavioral work with dogs (Topál et al., 2009), suggesting that dogs do not expect ostensively communicated information to generalize, future work should investigate this question further. It would be interesting in future studies, for example, to calibrate dogs' expectations across less salient kind changes. It is possible that the dogs tested in the current studies did not show an enhanced ability to detect kind-relevant changes in the ostensive condition because they were already at ceiling at detecting kind-relevant changes even in the nonostensive condition. To address this concern, future work could make kind-relevant changes more difficult for dogs to detect by using elements of previous work with adult humans (Marno et al., 2014). In particular, future work with dogs could (a) increase the number of objects in the display during demonstration and/or (b) make the changes between object kinds more subtle. When similar methodological changes were made in studies with adult humans, participants showed an increase in their memory for kind-relevant changes in ostensive communicative contexts (Marno et al., 2014). Moreover, future work could conduct pretests with dogs to determine their preference for the toys used in the demonstrations. One weakness of the current study is that we were unable to conduct such a pretest, and thus future work could better ensure that objects are matched for preference.

In addition, future work could also investigate whether more discriminable location changes result in dogs becoming more reliant on ostensive cues. It is possible that dogs in the current studies demonstrated a floor effect in noticing location changes, such that the location changes were too difficult for them to detect in any context. Although we worked to make the location changes more drastic in Experiment 2, future studies could make these changes even *more* dramatic (e.g., appearing on one side of the room vs. another). If location changes were made easy enough for dogs to detect in nonostensive contexts, this would provide a good opportunity to investigate whether dogs would get *worse* at detecting location changes in ostensive contexts, similar to infants in prior work (Yoon et al., 2008). If this future work continued to demonstrate no difference in dogs' ability to detect location changes in ostensive versus nonostensive contexts this would provide even stronger evidence that humans are unique in their expectation that ostensive communication will generalize.

In addition, future work could further accentuate the distinction between the ostensive and nonostensive conditions. Experiment 1 closely replicated the ostensive and nonostensive conditions of the previous work with infants (Yoon et al., 2008) and found no effect of ostension. Moreover, Experiment 2 observed the same null effect even after making this distinction more drastic by using a wooden stick and no social cues. It is possible, though, that dogs require an even stronger distinction given their high sensitivity to human ostensive cues. In particular, dogs may have interpreted both the “Oooh” sounds in the nonostensive condition in Experiment 1 and the clicking sounds made by the experimenter in Experiment 2 as somewhat ostensive. In other words, because the nonostensive condition was social—there was a human present—dogs may have interpreted these

sounds as ostensive. Future work could investigate this further by comparing an ostensive condition to a fully nonsocial condition. However, we find it highly unlikely that this would impact the results given that dogs have distinguished between ostensive and nonostensive conditions in a wide range of prior work (e.g., Duranton et al., 2017; Kaminski et al., 2012; Téglás et al., 2012; Topál et al., 2009).

Finally, in addition to making the ostensive and nonostensive conditions more distinct, future work could take even further measures to ensure that the ostensive and nonostensive conditions are as closely matched as possible. In particular, additional studies could use videos of the demonstrations (as was done in the case of infants; Silverstein et al., 2019; Yoon et al., 2008). In the current study, we chose to use live actors because we thought this would be more likely to capture dogs' attention, but this led to some variation in the duration of familiarization trials in Experiment 2. Future studies using video presentations would allow for more consistent and closely-matched demonstration times during the familiarization trials. Moreover, future work could measure dogs' looking time during the familiarization trials to ensure dogs are looking at the familiarization demonstrations for similar durations between the two conditions. Given that we did not see any differences between the ostensive and nonostensive conditions, with the exception of looking time on the first trial in Experiment 2, we think it is unlikely these small differences between the ostensive and nonostensive conditions influenced our results.

Taken together, our findings build on those of previous behavioral studies (Topál et al., 2009) and suggest that dogs do not expect that information gained via ostensive communication will generalize across object kind. This provides additional evidence that humans may be unique in their expectation that ostensively communicated information will generalize. Considered more broadly, it is possible that this expectation that ostensive communication will generalize uniquely supports our complex human culture via highly efficient instruction (Csibra & Gergely, 2011). Not only does this work highlight a potentially unique aspect of human learning—the expectation that ostensive information will generalize—it also demonstrates more broadly how looking time research with dogs can help pinpoint unique aspects of human learning.

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