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Infants learn enduring functions of novel tools from action demonstrations

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Abstract

According to recent theoretical proposals, one function of infant goal attribution is to support early social learning of artifact functions from instrumental actions, and one function of infant sensitivity to communication is to support early acquisition of generic knowledge about enduring, kind-relevant properties of the referents. The present study tested two hypotheses, derived from these proposals, about the conditions that facilitate the acquisition of enduring functions for novel tools in human infancy. Using a violation-of-expectation paradigm, we show that 13.5-months-old infants encode arbitrary end-states of action-sequences in relation to the novel tools employed to bring them about. These mappings are not formed if the same end states of action sequences cannot be interpreted as action goals. Moreover, the tool-goal mappings acquired from infant-directed communicative demonstrations are more resilient to counter-evidence than those acquired from non-infant-directed presentations, and thus show similarities to generic rather than episodic representations. These findings suggest that the acquisition of tool functions in infancy is guided by both teleological action interpretation mechanisms and the expectation that communicative demonstrations reveal enduring dispositional properties of tools.

Keywords

infancy; tool function; goal; teleological action interpretation; communication; ostension

Introduction

The material culture of *Homo sapiens* displays robustness and complexity unmatched in the animal kingdom. Our environment is populated with artifacts, and our goals are routinely attained with the help of different kinds of tools, which were designed and manufactured in order to facilitate bringing these goals about. Human adults conceptualize tools through their functions, i.e., they tend to think about kinds of tools as being “for” achieving particular goals. Function is an enduring *dispositional* property of a tool to bring about a particular goal when used in an instrumental action. Consequently, function is an unobservable abstract feature, whose relations to the available structural and behavioral information (e.g., observable physical features of a tool or its manners of use) are often cognitively opaque

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(Csibra & Gergely, 2006). Human children deal remarkably well with the considerable and unique challenge of acquiring knowledge of tool-kinds in terms of their functions, and numerous attempts have been made recently to study experimentally the early developmental roots of these achievements in infancy (Baumgartner & Oakes, 2011; Brugger, Lariviere, Mumme, & Bushnell, 2007; Futó, Téglás, Csibra, & Gergely, 2010; Hunnius & Bekkering, 2010; Sommerville, Hildebrand, & Crane, 2008; Träuble & Pauen, 2007).

Learning tool functions is not just a necessary step in development of full-fledged adult-like tool-use, but also a key to categorization of artifacts (Kelemen & Carey, 2007) — i.e., of a substantial portion of the human environment — as well as a key to online prediction of instrumental actions with tools and of their outcomes (Csibra & Gergely, 2007; Hunnius & Bekkering, 2010; Paulus, Hunnius, & Bekkering, 2011) – i.e. of a substantial portion of human everyday activities. Thus, we can expect that the development of functional knowledge of tools is not necessarily tied to slowly emerging competencies to use tools (Greif & Needham, 2011; McCarty, Clifton & Collard, 2001), and may have very early developmental bases. In adults, (i) function underlies categorization of tools (i.e. any given tool belongs to a kind in virtue of function, which is a property of both the individual tool and of its kind), (ii) tool-function mappings are exclusive (i.e., typically a tool has a single kind-defining function even though its physical structure affords attaining various goals), and (iii) tool-function mappings are enduring (i.e., a tool maintains its kind-defining function when broken, not in use, or when temporarily put to a different idiosyncratic use). Recent studies have demonstrated attention to functional information for categorization of tools in 12-months-old infants (Träuble & Pauen, 2007), expectation of exclusive mappings between artifacts and their hidden dispositional properties in infants perhaps as young as 10-month-old (Futó et al, 2010, but see Casler, 2014; Defeyter & German, 2003), and beginnings of endurance of function-tool mappings in 24-month-olds (Casler & Kelemen, 2007).

In order to learn the function of a tool one can try finding out what it was *made for*. Even young preschoolers appreciate the importance of intended function when making functional judgments (Kelemen, 1999; Defeyter, Hearing, & German, 2009). However, since both designers and users of tools typically aim at maximizing efficiency of instrumental actions, function can be often reliably established by considering what the tool is *good for* (i.e., it can be inferred from the causal-mechanical affordances of the tool), or by observing what it is *used for* (i.e., it can be inferred from the goal of an observed instrumental action with the tool; Csibra & Gergely, 2007). The latter route to function-tool mappings is of particular interest here for three reasons: (i) It relies on the mechanisms of action understanding, which can support goal-attribution (and consequently ascription of the function to the tool) despite the cognitive opacity of the causal relations that underlie the workings of the tool and its manner of use. (ii) Given human infants' proficiency with goal-attribution, learning what the tool is for by observing what it is used for may be a cognitive strategy available to human children already in infancy. (iii) Identifying the goal of an instrumental tool-use demonstrated in a communicative context, may allow infants to infer not only the

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idiosyncratic purpose that the individual tool serves on a particular occasion but its enduring function, which for adults defines the tool-kind (Hernik & Csibra, 2009).

The series of experiments presented in this paper explores the conditions that facilitate the acquisition of enduring functions for novel tools in human infancy. Specifically, this research is motivated by the theoretical proposal that learning tool kinds and their functions is facilitated by two sets of cognitive skills: (i) the propensity for teleological action interpretation, and (ii) the ability to acquire generic information from ostensive communicative demonstrations. In the following sections we discuss these theoretical claims in detail.

Early teleological action interpretation and tool-function acquisition

Throughout this paper we use the term "goal" to refer to a state of reality that is interpreted as an explanatory factor for the action that has brought it about. Such interpretations of action-outcome relations are called "teleological" (Csibra & Gergely, 2013).

From very early on, human infants are prone to interpret observed behaviors as goal-directed actions. This claim is supported by at least 3 types of evidence. (i) Infants expect observed consecutive actions to be similar with respect to their end-states (e.g. a hand ending its movement by making contact with a particular toy, or an agent stopping at a particular object) rather than with respect to their spatiotemporal characteristics (e.g. a hand or an agent moving towards the object along a particular motion-path; Woodward, 1998). (ii) This preferential encoding of the end-states over the spatiotemporal characteristics of the means actions depends critically on the behavioral and contextual characteristics of the actions leading to these end-states, such as efficiency (Hernik & Southgate, 2012, Biro, Verschoor, & Coenen, 2011), selectivity (Luo & Baillargeon, 2005; Hernik & Southgate, 2012) and purposefulness (Woodward 1999). (iii) Infants expect observed consecutive actions towards the same end-state to change according to the changing environment and to be the most efficient given the current situational constraints, rather than to preserve familiar spatiotemporal characteristics (Csibra & Gergely, 2013). Recent studies suggest that infants are sensitive to such efficiency considerations within the first months of their lives (Csibra 2008; Skerry, Carey, & Spelke 2013). It is worth pointing out that many recent studies on goal-attribution in infancy employ fixed-length familiarization paradigms, in which goals are operationalized as visually perceivable end-states of action sequences (e.g. Biro et al, 2011; Hernik & Southgate 2012, Luo, 2011; Luo & Baillargeon, 2005)

One potential function of the early emerging capacity for teleological action interpretation may be to support early social learning of tool functions despite cognitive opacity of tools (Casler & Kelemen, 2005, Csibra & Gergely, 2007; Hernik & Csibra, 2009). If human infants' representations of tools, like those of human adults, are indeed function-centered and facilitated by teleological interpretation of instrumental actions with tools, we should expect infants to readily form specific mappings between novel tools and goals, even if they cannot be supported by additional elements of tool knowledge (e.g., the understanding how the tool's structure and manner of use contribute causally to the goal). When tools of two different kinds are operated by means of the same actions (e.g., a flashlight and a remote control — both hand-held and operated by button-pushing), teleological interpretation of these actions should nevertheless enable ascribing two different functions to the tools, provided that there are two distinct states of environment (e.g., the light being emitted vs. the TV being on), which can be attributed as goals to the respective actions with these tools.

Notably, it is not yet established whether infants are capable of such achievements. Firstly, sensitivity to function in infants is typically operationalized by assessing whether they encode distinctive correlates of tool function: tool parts, means actions or spatial locations of the goal (Hunnius & Bekkering, 2010; Paulus et al., 2011; Träuble & Pauen, 2007). Secondly, even though recent findings are taken to suggest that, by the end of their first year of life, infants can form distinctive tool-outcome mappings, the underlying mechanism is unclear. Baumgartner and Oakes (2011) habituated infants to videos showing two different toys acted upon in the same way (e.g., rolled across a screen) while two different sounds (e.g., click or whistle) were emitted. Subsequently, 12-month-olds, but not younger infants, reacted with increased looking to test events in which the toy-sound pairings were switched, thus displaying evidence of learning separate correlations between the two toys and the corresponding sounds (see also Perone & Oakes, 2006, review in Oakes & Madole, 2008). While these results are consistent with our hypothesis, they do not clarify whether the toy-sound mappings relied on teleological action representation or were produced by processes of audio-visual integration and detection of co-occurrences within such bimodal events (for discussions, see Horst, Oakes, & Madole, 2005; Oakes & Madole, 2008)[1]. Extensive habituation procedure and reliance on auditory stimuli makes it difficult to compare Baumgartner & Oakes' (2011) study with the literature on teleological action interpretation.

We designed a violation-of-expectations procedure based on visual stimuli, which allowed us to target infants' mappings of tools and states of the environment. In Experiment 1, we asked whether infants would form representations linking two different novel tools with the end-states of action sequences in which they were used. Notably, the stimuli were design in order to ensure that the separate mappings could not be supported by attention to distinctive correlates of function. The tools were operated in the same manner and applied to the same location. They had no distinctive parts active when the goal was produced and the physical features provided no obvious cues to functions. Experiment 2 was designed to rule out that end-states of actions enter these representations in virtue of mere visual co-occurrence with the tools.

Acquiring generic tool representations from communicative demonstrations

Numerous recent empirical findings suggest that communicative demonstrations facilitate the encoding of kind-relevant and generalizable information in human children and infants (Träuble & Bätz, 2014; Egyed, Király, & Gergely, 2013; Király, Csibra, & Gergely, 2013; Butler & Markman, 2012, 2014; Futó at al., 2010; Yoon et al., 2008). According to the theoretical proposal of natural pedagogy (Csibra & Gergely, 2006, 2009, 2011; Csibra & Shamsudheen, in press) ostensive communication has this effect by eliciting a referential

[1]According to Oakes and colleagues (Perone, Madole, & Oakes, 2011), temporal contiguity of sound and action on the toy might lead infants to interpret the former as the causal consequence of the latter, but this conjecture was not tested experimentally (e.g., by manipulating temporal order of the action and the sound). This issue is not crucial for the approach advocated by Madole, Oakes and colleagues (Madole & Cohen, 1995; Oakes & Madole, 2008), which focuses on detecting co-occurrences of features (in this case, appearances and sounds) as a general mechanism underlying function acquisition in infancy.

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expectation (i.e., an expectation that the demonstration has a content; Deligianni, Senju, Gergely, & Csibra, 2011; Senju & Csibra, 2009) and a 'genericity bias' towards the content of the demonstration. In case of tool-function demonstrations genericity bias may lead to an expectation that the predicate (i.e., the demonstrated function) applies not only to the current episode but rather that it represents a permanent property of the object, and that it applies not only to the particular object used in the demonstration, but also to the kind that this object represents. On this account, communicative demonstrations of tool use are interpreted by the addressee as providing explicit information about the enduring, culturally sanctioned function of the object kind represented by the particular tool used in the demonstration. In contrast, while the mere observation of the outcome of a goal-directed tool-use may provide a cue about the potential function of the tool, the observed action could well be an idiosyncratic use of the object, whose outcome is not be to learned or generalized to other objects or to other occasions.

We addressed the role of the communicative context in forming the tool-goal mappings in Experiments 3 and 4, and tested the hypothesis that communicative demonstrations enable acquisition of generic functional representation of novel tools.

Experiment 1

We designed a violation-of-expectations procedure in which infants first watched two novel tools (a 'banana-peeler' and a 'banana-healer'), the use of which led to two different end-states. For an adult viewer, one of the tools seemed to transform an unpeeled banana into a peeled one, the other seemed to transform a peeled banana into an unpeeled one. In order to assess whether infants learned the mappings between each novel tool and its respective end-state, we measured 13.5-month-old[2] infants' looking-times to test events in which the use of the banana-peeler and the banana-healer led to end-states that were either congruent or incongruent with these mappings.

Since previous studies demonstrated that the presence of ostensive communicative signals, such as infant-directed speech, facilitates the encoding of kind-relevant information about artifacts (Futó at al., 2010; see also Yoon, et al., 2008), we also embedded our function demonstrations within the context of infant-directed communication.

Methods

Participants—Sixteen 13.5-months-old infants (13 months and 3 days – 14 months 2 days; mean = 13 months and 19 days) constituted the final sample. Two additional babies were excluded and replaced because they did not finish the procedure due to fussiness. All

[2]This age group was chosen for two reasons. First, it has been argued that 14-month-olds acquire function-form mappings through detecting arbitrary visual correlations (Madole & Cohen, 1995), making this age-group particularly suitable for testing the role of teleological action interpretation in functional encoding of tools. Second, in a pilot study with sixteen 12-month-olds (11 months and 21 days – 12 months and 16 days; mean = 12 months), which used a procedure and design identical to Experiment 1 (except that each participants saw only 2, rather than 4 test trials), we found no statistically significant effect of Test Event, $\text{mean}_{\text{Congruent}} = 19.49$ s, $\text{SD}_{\text{Congruent}} = 15.77$, $\text{mean}_{\text{Incongruent}} = 14.59$ s, $\text{SD}_{\text{Incongruent}} = 14.59$, $t(15) = 1.51$, $p = 0.15$, 2-tailed. We did find, however, a significant correlation between the index of looking to incongruent rather than congruent event ($\text{looking-time}_{\text{Inongruent}}$ - $\text{looking-time}_{\text{Congruent}}$) and age, $r_{\text{Pearson}}(16) = .66$. $p = .005$, which suggested to us that the task may be more suitable for babies older than 12 months.

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included babies looked at the outcome phase of each test trial for a minimum of 2 s (see Procedure and stimuli section for details).

Procedure and stimuli—Infants sat on their parents' lap approximately 100 cm away from the presentation screen, and watched a series of 8 familiarization trials followed by 4 test trials. An attention-getting animation was presented before 1st, 3rd, 5th and 7th familiarization trial and before each test trial. After the 8th familiarization trial, a message on the screen reminded the parents to keep their eyes closed till the end of the session.

Stimuli were movies pre-edited in Final Cut Express 4.0 software and presented on a 40 inch (102 cm) plasma screen, so that all depicted objects were shown in their real-life size (see Figure 1). Each familiarization movie (11.75 s total running time) started by fading-in from black (0.5 s) into a picture of a black-clothed table against a black background, with a banana mounted in the middle and a pair of novel objects (each approximately 15 cm wide and 26 cm high; henceforth: a Blue and a Pink tool) standing on each side, 48 cm apart. Both tools were in fact flower-pots turned upside down: a blue ceramic one and a pink plastic one with a semi-translucent plastic vertical tube attached on top. After a pause of 2.75 s, a pair of female human hands entered the scene from one of the sides. The hands lifted the nearest tool, covered the banana with it (2 s), then rotated the tool around its vertical axis by 45 degrees and back (1.25 s). Finally, the hands lifted the tool up from the banana, put it back to its original position at the side of the screen and exited (2.75 s). The outcome stayed on the screen for 2 s, and then the picture faded-out to black (0.5 s).

Crucially, if at the start of the familiarization movie the banana was unpeeled, then it was revealed as peeled when the tool was lifted back from it (banana-peeling movies), and if the banana was initially peeled, it was revealed unpeeled (banana-healing movies). Each infant saw 4 peeling and 4 healing movies on alternate familiarization trials, with one of the tools (either the Pink or the Blue, fully counterbalanced across participants) being consistently used as the banana-peeler and the other used as the banana-healer.

The 1st familiarization movie always showed the tool on the left being operated, but whether this tool was Pink or Blue was fully counterbalanced across participants. The 1st, 2nd, 5th and 6th familiarization trials showed the tools in the same spatial arrangement (e.g. the Pink on the left and Blue on the right) and the 3rd, 4th, 7th and 8th in the opposite spatial arrangement.

Each familiarization movie had the same pre-recorded audio track in which a female voice speaking Hungarian with infant-directed prosody (Cooper & Aslin, 1990). The voice said "Hello baby, hello. I will show you something. Look See? Done." The voice belonged to the research assistant who talked to the mother and the baby for about 5-10 minutes prior to the experiment, then walked with them to the testing room, seated them in front of the presentation screen and hid behind the curtain at the back of the presentation screen. Thus, the audio track carried three potential indicators (the prosody, the verbal content, and the speaker with a recent history of interactions) that could lead infants to interpret the familiarization movies as communicative demonstrations ostensively directed to them.

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During the test trials, infants saw movies just like those presented during familiarization, except that they were silent and began with the banana already covered and the hands starting to twist the tool around it. Thus, the initial state of the banana (peeled or unpeeled) was not known. Each test trial consisted of an *action phase* and an *outcome phase*. The action phase was always 2800 ms long and lasted from the beginning of the movie till the first movie-frame in which the banana was fully visible. The rest of the trial was the outcome phase, during which the hands brought the tool back to its side and exited, and the still frame of the two tools and the banana stayed on the screen until the infant looked away from the screen for more than 2 s, according to the on-line coding, or until 60 s elapsed from the beginning of the trial.

For each infant, on two test trials (either 1st and 4th or 2nd and 3rd, fully counterbalanced across the participants) the outcomes were congruent with the tool-end-state mapping presented in familiarization (i.e., the banana revealed from under the banana-peeler was peeled or the one revealed from under the banana-healer was unpeeled), and on the remaining two trials they were incongruent (i.e., the banana revealed from under the banana-healer was peeled or the one revealed from under the banana-peeler was unpeeled). The Pink tool was used on the 1st and 3rd trial and the Blue tool was used on the 2nd and 4th trial. Thus, within each pair of test trials (1st vs. 2nd), each tool was shown producing the same outcome, but one trial was always congruent and one was incongruent. For all infants, all test trials showed the same spatial arrangements of the tools: the Blue tool was operated from the left side and the Pink tool from the right.

Measure and coding—Infants were video recorded at 25 frames per second for off-line coding by the first author. The main measure of interest was infants' looking-time towards the screen during test trials from the beginning of the outcome phase till the infant looked away from the screen for more than 2 s (according to the off-line coding), or till the end of the test trial. Test trials from 6 infants (37% of all test trials) were coded off-line by a second coder, blinded to the conditions. The inter-coder agreement was excellent (r = .99, average absolute difference per trial = 112 ms). For all parametric tests, the looking times to the outcome phases of test trials were log-transformed in order to approximate a normal distribution.

Results and Discussion

Infants attended very well to the familiarization events. On average, a familiarization trial was watched for 97% of its duration (minimum = 71%, maximum = 100%), with 83% of all familiarization trials watched for at least 95% of their duration. All infants looked towards the screen 100% of the time during the action phases of test trials.

Average looking times to the test events are depicted on Figure 2. An initial 2 (Test Pair: 1st vs. 2nd) × 2 (Test Event: Congruent vs. Incongruent) ANOVA on log-transformed looking-times to the outcome-phases of test trials showed only a main effect of Test Event, $F(1,15) = 6.72$, $p = .02$, $\eta_p^2 = .31$. There was no significant effect of Test Pair, $F(1,15) = 1.09$, $p = .31$, nor interaction, $F(1,15) = 0.15$, $p = .70$. Thus, for the subsequent analyses looking times to congruent and incongruent test events were collapsed across the test pairs.

In a series of 2×2 ANOVAs, no significant main effects or interactions with Test Events (Congruent vs. Incongruent) were found for the controlled factors: Order of Test Outcomes (Peeled-first vs. Healed-first) and Order of Test Events (Congruent-1st-and-4th vs. Congruent-2nd-and-3rd). There was a main effect of Function Assignment (Pink Peeler-Blue Healer vs. Pink Healer-Blue Peeler), with the test trials looked at longer in case of the Pink Peeler-Blue Healer assignment, $F(1,14) = 10.34$, $p = .006$, $\eta_p^2 = .42$. However, there was no significant interaction between Function Assignment and Test Event either, $F(1,14) = 0.19$, $p = .67$.

Planned comparisons using 2-tailed t-test and nonparametric Wilcoxon Signed Ranks test confirmed that infants looked longer at the Incongruent than at the Congruent test events, $t(15) = 2.65$, $p = .018$, Cohen's $d = 0.66$, Wilcoxon $Z = -2.4$, exact $p = .014$ (Figure 2). This pattern of looking-times was shown by 12 of the 16 babies. These results are consistent with the hypothesis that human infants can readily learn mappings between novel tools and the goals that these tools help bringing about when employed in instrumental actions, even if the behavioral means by which these actions are performed are identical and the causal relations between the goal and the tool's structure and manner of use are cognitively opaque. Some elements of the action-sequence were causally transparent to 13.5-months-old infants (e.g., transporting the tool from one location to another, covering and uncovering a banana, twisting the object around its axis and back), and could be evaluated as the most efficient means to the corresponding sub-goals. Since the causal relations between these means actions in the sequence and the end-state (e.g., a peeled banana) were cognitively opaque to the infants, their efficiency with respect to this stipulated goal could not be evaluated either as the most efficient or as violating efficiency. Consequently, causal opacity of the two novel tools did not prevent infants from interpreting the end-states as goals of the tool-using actions, and mapping them on the tools as their functions.

In line with numerous recent reports on early goal-attribution (Biro, et al., 2011; Hernik & Southgate 2012; Luo, 2011; Luo & Baillargeon, 2005), the results demonstrate that the establishment of the mappings between visually perceivable tools and goal-states does not require extensive habituation but can be induced by only 4 pairs of demonstrations in 13.5–month-olds.

Experiment 2

The aim of this experiment was to test an alternative account of the results from Experiment 1. It is conceivable that the representations linking each tool with the respective final state of the banana were the results of mere visual association. Even much younger infants have been shown to extract statistical regularities from sequences of visual stimuli (Kirkham, Slemmer, & Johnson, 2002), and — as already pointed out in the introduction — according to some authors, statistical learning of the correlations between the tools and the states of environment co-occurring with their use (like outcomes) is one of the mechanisms of the early function acquisition (Baumgartner & Oakes, 2011; Oakes & Madole, 2008; Madole & Cohen, 1995).

In order to test whether infants in Experiment 1 formed the representations linking the two tools with the two states of the banana in virtue of their co-occurrence, in Experiment 2 we provided infants with familiarization movies which differed from those used in Experiment 1 in just one respect: The final state of the banana in each movie was always identical to its initial state (i.e., an initially unpeeled banana remained unpeeled, and the peeled one remained peeled, when revealed from under the tool). Since for each child the correlation between each tool and the final state of the banana was exactly the same as in Experiment 1, the association account predicts that infants in Experiment 2 should be just as likely as those in Experiment 1 to map the final banana-states on the corresponding tools, and consequently would again display increased looking to test trials incongruent with such mappings. Arguably, Experiment 2 should provide even better conditions for visual associations than Experiment 1, since in each trial a given tool was ever operated in the visual proximity of only one banana-state (as opposed to two different states per each trial in Experiment 1), and the time-window for forming a visual association between them was longer.

Notably, this prediction is inconsistent with the teleo-functional account of infant tool understanding advocated in this paper. According to this account, infants in Experiment 1 encoded the banana's final state (peeled or unpeeled) in relation to the tool – at least in part – *because* they interpreted it as the goal of the respective instrumental action with the tool. An end-state of the action sequence, which is identical to the initial-state (before the action is commenced), is not likely to be interpreted by infants as the goal of that action, because the action cannot be readily interpreted as the most efficient way to achieve it. Thus, the teleo-functional account predicts that the very same final states of the banana, which in Experiment 1 had been encoded in relation to the respective tools, would be less likely to enter such mappings in Experiment 2, where they are poor candidates for action goals.

Methods

Participants—Sixteen 13.5-months-old infants (13 months and 0 days – 13 months and 25 days; mean = 13 months and 17 days) constituted the final sample. Four additional babies were excluded and replaced because of experimenter's error, parental interference, looking at a test trial for less then 2 s, or fussiness resulting in not finishing the procedure.

Procedure, stimuli and coding—The experimental and coding procedures, as well as the stimuli were identical to those of Experiment 1, except that in the familiarization movies the initial state of the banana (from the beginning of the movie, until the banana was covered with a tool) was always the same as its final state (since when the banana was revealed from under the tool, until the end of the movie). As in Experiment 1, 37% of test trials were double-coded, resulting in an excellent inter-coder agreement (r = .99, average absolute difference per trial = 110 ms).

Results and Discussion

The overall level of infants' attention to familiarization trials was almost identical to that of Experiment 1. On average, a familiarization trial was watched for 97% of its duration (minimum = 71%, maximum = 100%), with 82% of all familiarization trials watched for at

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least 95% of their duration. All infants looked towards the screen 100% of the time during the action phases of test trials.

Mean looking times to the test trials are depicted on Figure 2. An initial 2 (Test Pair: 1st vs. 2nd) × 2 (Test Event: Congruent vs. Incongruent) ANOVA on log-transformed looking-times showed no significant effects or interaction, all Fs(1,15) < .72, all ps > .41, thus for the subsequent analyses looking times to congruent and incongruent test events were collapsed across the test pairs. In a series of 2×2 ANOVAs, we found no significant main effects of, or interactions with Test Events (Congruent vs. Incongruent), for the controlled factors: Order of Test Outcomes (Peeled-first vs. Healed-first), Order of Test Events (Congruent-1st-and-4th vs. Congruent-2nd-and-3rd) and Function Assignment (Pink Peeler, Blue Healer vs. Pink Healer, Blue Peeler), all Fs(1,14) < 0.64, all ps > .44.

Planned comparisons using 2-tailed t-test and nonparametric Wilcoxon Signed Ranks test did not find significant differences in looking-times between Congruent and Incongruent test events, $t(15) = 1.30$, $p = .21$, Wilcoxon $Z = -1.34$, exact $p = .19$ (Figure 2). Only six of the 16 babies looked longer at the incongruent events and 10 showed the opposite pattern.

A 2 (Test Events: Congruent vs. Incongruent) × 2 (Experiment: 1 vs. 2) ANOVA found no significant main effects of either Test Events, $F(1,30) = 0.003$, $p = .95$, or Experiment, $F(1,30) = 0.58$, $p = .45$. Importantly, it did find a significant interaction, $F(1,30) = 5.60$, $p = .024$, $\eta_p^2 = .16$, suggesting that infants in Experiment 2 responded to the test events differently from those in Experiment 1. Specifically, unlike infants in Experiment 1, infants in Experiment 2 did not show longer looking to test events incongruent with the pairings of tool and end-state presented in familiarization (Figure 2). These results suggest that the encoding of tools in relation to end-states in Experiment 1 cannot be accounted for by visual association. Moreover, they are consistent with the notion that interpreting the end-state as the goal, towards which the action-sequence was the efficient means, was critical for forming the mappings observed in Experiment 1.

It is worth pointing out that a recent study found that encoding of the end-state of an action sequence critically depended on efficiency in an age-group similar to that of our infant participants (Biro et al, 2011). Notably, similar to our experiments, Biro and colleagues (2001) used a fixed-length familiarization procedure, in which a human hand acted on a small set of objects. They showed 12-months-olds four familiarization trials in which a hand entered the stage, lifted the lid of a container and ended the action-sequence by grasping a toy. If opening the container during these familiarization events was efficiently related to the outcome of grasping the toy (because the toy was inside the container), infants showed evidence of encoding the identity of the toy involved in this end-state (i.e., they reacted with increased looking to the test event in which the hand grasped a new toy, rather than the old toy that it used to grasped during the familiarization). However, if during the familiarization the toy was always outside the container and thus lifting the lid was not efficiently related to the grasping outcome, then during test infants looked equally long at the hand grasping the old toy or the new one, thus showing no evidence of encoding the details of the outcome witnessed during the familiarization. In another condition reported by Biro and colleagues (2011), infants who watched the hand enter the stage and grasp the solitary toy (there was no container in this condition) also failed to evidence encoding the details of this outcome, and this result is consistent with numerous reports of null-effects in the so-called one-target versions of the Woodward paradigm (Hernik & Southgate 2012, Luo 2011, Luo & Baillargeon, 2005).

The pattern of results reported by Biro et al. (2011) strongly suggests that even for 1-year-olds encoding the details of action end-states may not be an easy feat but rather it depends on whether the end-states can be readily interpreted as action goals (see also, Woodward & Sommerville, 2000; Wynn, 2008). The success of our infant participants in forming tool-end-state mappings in Experiment 1 and the failure to evidence them in Experiment 2 is also consistent with this interpretation: the end-states, which could be interpreted as the goals of efficient actions, were encoded in relation to the respective tools, but those that could not be interpreted as goals – were not[3].

The remaining two experiments in the paper address the role of the communicative context in forming tool-goal mappings.

Experiment 3

In line with the theoretical proposal that communication facilitates the encoding of kind-relevant information (Csibra & Gergely, 2006, 2009, 2011), in Experiment 1 and 2, the instrumental actions with the novel tools were presented during familiarization in the context of communication directed to infant participants. Specifically, the audio track of the familiarization movies involved three sources of information (the prosody, the verbal content and the speaker), which could indicate to the infants that they were being communicatively addressed.

In order to test whether infant-directed communication indeed supported formation of the tool-goal mappings in Experiment 1, we changed the audio track accompanying the familiarization movies by removing the three sources of information that infants in Experiment 1 might have relied on to infer ostensive communication directed to them. Specifically, instead of a familiar speaker addressing them in infant-directed speech, infants heard an unfamiliar speaker, who addressed some undetermined audience using adult-directed intonation. We expected that infants exposed to such familiarization should be less likely to treat the instrumental actions with novel tools as communicative demonstrations, and consequently less likely to learn the end-states of the observed actions as the functions of the two novel tools.

Methods

Participants—Sixteen 13.5-months-old infants (13 months and 3 days – 13 months and 30 days; mean = 13 months and 18 days) constituted the final sample. Ten additional babies were excluded and replaced because of equipment failure (1), parental interference (2),

[3]Note that our manipulation prevented infants from interpreting the *end-states* of the action sequences as action goals (and our procedure tested specifically whether infants encoded *these end-states* in relation to the respective tools). This manipulation did not prevent infants from interpreting the no-change-of-state actions in Experiment 2 as directed at some other goals. The hypotheses tested in this paper do not specify what such goals could be and our task was not designed to test for such attributions.

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looking at a test trial for less then 2 s (3), indiscriminate looking at the screen for the full length of 2 test trials (1), or fussiness resulting in crying or not finishing the procedure (3).

Procedure, stimuli and coding—The experimental and coding procedures, as well as the stimuli were identical to those of Experiment 1, except for the audio track that accompanied each familiarization movie. Specifically, the female voice was speaking with a "flat" prosody typical of adult-directed speech. She was saying in Hungarian "Good day, ladies and gentlemen. There is something here. Useful. Very well. Done." The voice belonged to a lab-member with whom infants had no experience prior to testing. As in Experiments 1 and 2, 37% of test trials were double-coded, resulting in an excellent inter-coder agreement (r = .99, average absolute difference per trial = 80 ms).

Results and Discussion

As in Experiments 1 & 2, attention to familiarization events was at ceiling level. On average a familiarization trial was watched for 98% of its duration (minimum = 62%, maximum = 100%), with an excellent 91% of all familiarization trials watched for at least 95% of their duration. All infants looked towards the screen 100% of the time during the action phases of test trials.

An initial 2 (Test Pair: 1st vs. 2nd) × 2 (Test Event: Congruent vs. Incongruent) ANOVA on log-transformed looking-times to test-event outcomes showed no significant effects of Test Pair, $F(1,15) = 2.86$, $p = .11$, or Test Event, $F(1,15) = 0.95$, $p = .35$, but it did show a significant interaction between the two, $F(1,15) = 7.27$, $p = .017$, $\eta_p^2 = .33$. The significant interaction was further investigated using 2-tailed t-tests and Wilcoxon Signed Ranks tests, which revealed that infants looked significantly longer at the Incongruent Event than at the Congruent Event during the first test pair, $t(15) = 2.33$, $p = .034$, Cohen's $d = 0.58$, Wilcoxon $Z = -2.28$, exact $p = .021$, whereas during the second test pair they tended to look longer at the Congruent Event, even though this difference was not statistically significant, $t(15) = 2.10$, $p = .052$, Cohen's $d = 0.53$, Wilcoxon $Z = -1.76$, exact $p = .08$ (Figure 3). During the first test pair 12 of the 16 babies looked longer at the Incongruent Event, but during the second test pair only five did, Fisher's exact $p = .032$.

There were no significant effect of Order of Test Outcomes (Peeled-first vs. Healed-first) and Function Assignment (Pink Peeler-Blue Healer vs. Pink Healer-Blue Peeler), nor interactions of each of these factors with Test Event nor Test Pair, all Fs(1,14) < 0.67, all ps > .43. There was no significant interaction between the Test Event and Order of Test Events in the first pair of test trials, $F(1,14) = 2.10$, $p = .17$, but there was one in the 2nd pair, $F(1,14) = 5.90$, $p = .029$, $\eta_p^2 = .30$, which further suggests that, unlike in Experiments 1 and 2, in Experiment 3 infants' processing of the test events differed between the two test pairs[4].

A 2 (Experiment: 1 vs. 3) × 2 (Test Event: Congruent vs. Incongruent) × 2 (Test Pair: 1st vs. 2nd) ANOVA showed a significant effect of Test Event, $F(1,30) = 6.30$, $p = .018$, $\eta_p^2 = .17$,

[4]The interaction was due to the fact that infants looked at the Congruent Event in the 2nd pair longer than the Incongruent Event only when it was presented first, $t(7) = 4.53$, $p = .003$, Cohen's $d = 1.6$, Wilcoxon $Z = -2.52$, $p = .008$, 2-tailed, but not when it was presented second, $t(7) = 0.003$, $p = .99$, Wilcoxon $Z = -0.42$, $p = .74$. These post hoc effects are difficult to interpret due to a very small sample size.

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as well as a significant three-way interaction, $F(1,30) = 4.50$, $p = .042$, $\eta_p^2 = .13$. No other effects or interactions were significant. In response to the comment by an anonymous reviewer, we also analyzed infants' looking to the first pair of test events across Experiments 1 and 3, in order to test whether the non-ostensive presentation in Experiment 3 might have facilitated the tool-goal mappings. Such result would not be predicted by our account. In a 2 (Experiment: 1 vs. 3) × 2 (Test Event: Congruent vs. Incongruent) ANOVA, we found only a significant main effect of Test Event, $F(1,30) = 5.86$, $p = .022$, $\eta_p^2 = .16$, but no effect of Experiment an no interaction (*p*s > .32). Thus, in infants' looking to the first pair of test events, we found no evidence to suggest facilitation of tool-function mappings by the non-ostensive context.

Altogether, the results of Experiment 3 suggest that removing possible indicators of ostensive infant-directed communication had a subtle, yet telling, effect on infants' learning of novel tool functions. On the one hand, infants who watched the instrumental actions, which were not ostensively demonstrated to them, nevertheless encoded the goal-outcomes achieved with the two novel tools, and consequently reacted with increased looking to the test trials incongruent with these mappings. On the other hand, this evidence of learning new tool functions was short-lived and restricted solely to the first pair of test events. This pattern of looking-behavior across the test-pairs — significantly different from that of infants in Experiment 1 — suggests that the tool representations which infants acquired by watching non-ostensively presented instrumental actions with these tools were different from those acquired by watching ostensive infant-directed demonstrations.

Notably, representations acquired in the non-ostensive context failed to support infants' expectations after the first test-pair, i.e., after the first counterevidence to the newly acquired tool-function mappings was encountered by observing the first incongruent test trial. Recently, in a study on inductive inference and exploratory behavior in 4-year-olds, Butler and Markman (2012) reported a similar "vulnerability" to counterevidence when functional information about novel artifacts was acquired from non-ostensive action presentations. A different exploration-pattern, suggestive of a relative "resilience" to counterevidence, was observed in preschoolers for whom the very same functional property was demonstrated communicatively. According to Butler and Markman (2012), resilience in the face of mounting negative evidence is a signature characteristic of generic encoding, where information extracted from a communicative demonstration, which employs particulars, is attributed not just to those particulars themselves but also to the representation of the kind to which they belong.

One aspect of generic encoding is that it is readily expected to be relevant beyond the current demonstration and generalizable to new contexts, new instances and new items of the same kind. Another aspect is that it is not readily falsified by counterexamples (Leslie, 2007). Gergely & Jacob (2012) argued that similar "resilience" patterns are found in infants learning a novel means-action (Gergely, Bekkering & Király 2002; Király, Csibra & Gergely, 2013) or a hiding location of a target-object (Topál, Gergely, Miklósi, Erdőhegyi, & Csibra, 2008; Topál, Gergely, Erdőhegyi, Csibra, & Miklósi, 2009) from ostensive communicative demonstrations. Consistent with these interpretations, the significant difference in the resilience of infants' expectations between the current Experiments 1 and 3

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may also reflect a difference between generic vs. non-generic encoding of the novel tools' functions. It suggests that only functions presented to the babies through communicative demonstrations might have been represented as enduring tool properties (Experiment 1), not just as idiosyncratic transient properties manifested in a particular demonstration episode (Experiment 3).

Experiment 4

The aim of Experiment 4 was to test directly the prediction — stemming from the post-hoc interpretation of the results of Experiment 3 — that infants' representations of novel tool functions can be resilient or vulnerable to counterevidence depending on whether they were acquired from infant-directed communicative demonstrations or not. In Experiment 4 infants first watched familiarization events with the banana-peeler and the banana-healer, which were presented in the context of either infant-directed, or adult-directed communication (just like in the familiarizations of Experiments 1 and 3, respectively). But this time, before their representations were assessed at test, infants were confronted with counterevidence to the tool-goal mappings, which they had been exposed to during familiarization. On a single trial just before the test, one of the tools produced the outcome opposite to what it had produced during familiarization (e.g., the banana-peeler was shown to heal the banana, or the banana-healer was shown to peel the banana).

We expected that the infants who were exposed to infant-directed ostensive demonstrations would maintain clear expectations about tool-goal correspondences, as their generic representations of the two tools and their enduring functions should remain unchallenged by the single counterexample. Consequently, they should react with longer looking to test trials in which these mapping were violated, like infants in Experiment 1 did. On the other hand, if infants who acquired their tool-outcome mappings from the familiarization without infant-directed communication encoded them as transient idiosyncratic episodes, they should show a significantly different looking-pattern at test. In fact, since in Experiment 4 the counter-example preceded the test trials, these infants may not show at test any clear expectations about the tool-goal correspondences at all, similar to the infants in Experiment 3, who failed to show them after the exposure to counterevidence in the initial test trials.

Methods

Participants—Thirty two 13.5-months-old infants constituted the final sample divided into two conditions: infant-directed ostension (13 months and 9 days – 13 months and 30 days; mean = 13 months and 20 days) and no infant-directed ostension (13 months and 0 days – 13 months and 29 days; mean = 13 months and 14 days). Fifteen additional babies were excluded and replaced because of parent's interference (1), experimenter's error (2), looking to an outcome phase for less than 2 seconds (4), looking away during the action phase of the test (3), fussiness resulting in crying or not finishing the procedure (4) and coding difficulties (1).

Procedure, stimuli and coding—The experimental and coding procedures, as well as the stimuli the infant-directed ostension and no infant-directed ostension conditions were identical to those of Experiments 1 and 3 respectively, except that there was a single additional familiarization trial (counterevidence trial) presented between the familiarization and test, preceded by the attention-getting animation (a black-and-white checkerboard shaking periodically at the center of the screen while a gentle bell-sound was emitted). The counter-evidence trial showed the tools in the same spatial arrangement as the 8th familiarization trial and the 1st test trial. The tool used on that trial always produced the outcome opposite to what it used to produce during the preceding familiarization trials. For half of the babies the counterevidence trial showed the use of the same tool that was used in the last familiarization trial, for the other half it showed the other tool. The counterevidence trial was silent. As in earlier experiments, 37% of test trials were double-coded, resulting in an excellent inter-coder agreement (r = .99, average absolute difference per trial = 103 ms).

Results and Discussion

Overt attention to familiarization events and to the counter-evidence trials was at ceiling level. On average a familiarization trial was watched for 97% of its duration (minimum = 31%, maximum = 100%), with 71% of all familiarization trials watched for at least 95% of their duration. The counterevidence trial was attended on average for 97% of its duration with 78% of trials watched for at least 95% of their duration. There was no significant difference in how much overt attention infants in the two conditions paid to the familiarization events ($U_{\text{Mann-Whitney}}$ = 113.5, p = .58) and to the counter-evidence trial ($U_{\text{Mann-Whitney}}$ = 117, p = .53). All infants in the final sample looked towards the screen 100% of the time during the action phases of test trials.

An initial 2 (Test Pair: 1st vs. 2nd) × 2 (Test Event: Congruent vs. Incongruent) × 2 (Condition: Ostension vs. No-ostension) ANOVA on log-transformed looking-times to test-event outcomes showed no significant effect of Test Pair, $F(1,30) = 2.50$. $p = .12$, or any significant interactions between Test Pair and other factors (highest $F(1,30) = 1.58$, $p = .22$), therefore for further analyses the data were averaged across the test pairs. In a series of ANOVAs, no interactions with Test Event were found for the remaining controlled factors: Order of Test Outcomes (Peeled-first vs. Healed-first) and Function Assignment (Pink Peeler-Blue Healer vs. Pink Healer-Blue Peeler).

In a 2 (Test Event: Congruent vs. Incongruent) × 2 (Condition: Ostension vs. No-ostension) ANOVA we found a marginally significant effect of Condition (infants in the No-ostension group tended to look longer), $F(1,30) = 3.72$, $p = .063$, $\eta_p^2 = .11$, and a significant interaction, $F(1,30) = 6.49$, $p = .016$, $\eta_p^2 = .18$. Comparison of the individual patterns of looking confirmed that infants across the two conditions reacted to the test events differently (Figure 4). While 11 out of 16 babies in the Ostension condition looked longer at the incongruent rather than the congruent test trials, in the No-ostension condition only 4 out of the 16 babies did, Fisher's exact $p = .032$, 2-tailed.

Planned pairwise contrasts showed that in the Ostension condition babies looked significantly longer at the incongruent rather then the congruent test events, $F(1,30) = 4.52$, $p = .042$, $\eta_p^2 = .13$, whereas in the No-ostension condition they looked longer at the congruent events, however, this tendency was not statistically significant, $F(1,30) = 2.18$, $p = .15$. Comparisons using non-parametric Wilcoxon tests on looking times showed a smiliar pattern, but the effect in the Ostension condition did not reach statistical significance,

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Ostension: $Z_{\text{Wilcoxon}} = -1.7$, $p = .093$; No-ostension: $Z_{\text{Wilcoxon}} = 1.03$, $p = .32$. These results replicate the findings of Experiments 1 and 3 and allow us to conclude that infant-directed ostension facilitated the encoding of the tool-goal mappings as enduring aspects of tool representation. Resilience to counterevidence suggests that these mappings had a format of generic, rather than episodic representations, which could provide the basis for ascribing kind-defining functions to the novel tools.

General Discussion

The capacity for goal attribution is a remarkable, well documented feature of action understanding in infancy, often thought to be a cornerstone of early social cognition (Gergely & Csibra, 2003; Luo, 2011; Woodward 1998), which may support acquisition of tool functions (Casler & Kelemen, 2005; Csibra & Gergely, 2007; Hernik & Csibra, 2009). Sensitivity to ostensive communication is another hallmark of human infancy (Csibra & Gergely, 2006), which recently has been proposed to support social transfer of generic knowledge about kinds (Csibra & Gergely, 2006, 2009, 2011). The results of our experiments suggest that infants may indeed rely on the mechanisms of goal-attribution to start encoding functions of tools and that when such representations are acquired in a communicative context, the functions can be encoded as enduring generic tool properties.

Our findings indicate that at the beginning of the second year of life human infants readily represent novel objects in relation to the end-states of actions in which these objects are employed (Experiments 1, 3 & 4). Notably, several aspects of these findings are consistent with the proposal that the underlying mechanism is guided by teleological action interpretation. Specifically, (i) in line with the findings that infants attend specifically to the end-states over the spatiotemporal characteristics of the means actions (Woodward, 1998), in our experiment participants formed separate tool-end-state mappings despite the two means actions were identical. (ii) In line with numerous empirical findings documenting that infants' encoding of the end-states of action-sequences as goals critically depends on whether the actions can be readily interpreted as efficient means to the end-states (Biro et al. 2011; Hernik & Southgate, 2012), the results of Experiment 2 confirmed that the tool-end-state mappings indeed depended on whether the actions with the tools could be interpreted as efficient ways of bringing out the end-states. (iii) In line with recent reports of early goal-attribution (Biro, et al., 2011; Hernik & Southgate 2012; Luo 2011; Luo & Baillargeon 2005), in our experiment infants formed representations of action end-states on the bases of relatively few examples. (iv) In line with the proposal that by inferring what the tool is for (the function) from what it is used for (the action goal) infants may overcome a challenge of acquiring functional tool knowledge despite the physical opacity of tools (Csibra & Gergely, 2007), our infant participants attributed functions to tools despite the lack of knowledge of how the different end-states are causally related to the physical attributes of the novel tools and to the manners of use.

Our results also add to a growing body of evidence supporting the proposal that communication contributes to vertical social transmission of generic knowledge, at the receptive end of which human infants are prepared to be from early on in ontogeny (Futó et al., 2010; Király et al. 2013; Egyed, Király & Gergely, 2013; Yoon et al. 2008, Topál et al., 2008, 2009; Senju & Csibra; 2008; Butler & Markman, 2012). Infants' tool-function mappings acquired from communicative demonstrations (Experiments 1 and 4) exhibited endurance in the face of counterevidence, which was lacking if the mappings were acquired from demonstrations that were not infant-directed (Experiments 3 and 4). At the same time, the null-results of Experiment 2 are consistent with recent findings that suggest that acquisition of the opaque arbitrary means communicated to infants is constrained by their pre-existing expectations about what constitutes a well formed goal-directed action (Király et al., 2013). When end-state of the observed novel arbitrary action with a tool was identical to the state of the environment prior to the action (and thus could not be interpreted as the action goal), infants are not likely to imitate the action (Király et al., 2013), or show evidence of a mapping between the tool and the end-state (current Experiment 2), even if the action was presented in an ostensive communicative context.

Tools constitute an extensive domain in the proximal environment of a human child. Learning about tools requires the coordination of several learning mechanisms, and contribution from various (physical, motor, social) domains of cognition. The aim of our studies was to test the predictions derived from the hypotheses, according to which goal attribution supports function acquisition and sensitivity to communication supports representation of enduring functions in infancy. Our studies found positive evidence for such generic teleo-functional understanding of tools in 13-months-old infants. It remains an open question whether successfully formed toy-sound mappings, found in slightly younger babies using a habituation-switch paradigm (Baumgartner & Oakes, 2011), should be accounted for by the same mechanism, or they result from general statistical learning mechanisms that do not appeal to the notion of ‘goal’. It is also an open question whether and how association-based mechanisms may interact with teleological interpretation mechanisms in supporting the acquisition of tool-knowledge. Further research may elucidate the relations between forming the tool-goal mapping and encoding particular means-action that bring the goal about with the help of the tool, as well as the role that contrasting the two end-states on alternate trials might have had in forming the tool-goal mappings in the current studies. Convergent evidence for the role of communicative goal-directed demonstrations in forming such mappings is also needed, especially if it relies on experimental manipulations of goal-directedness and ostension different from those employed in the current experiments. Since both the capacity for goal attribution and the sensitivity to ostensive communicative signals are available to infants much earlier in their first year of life (Csibra, 2008; Luo 2011; Csibra & Gergely, 2006), future studies should investigate the age at which the development of generic tool knowledge starts being effectively supported by functional understanding.

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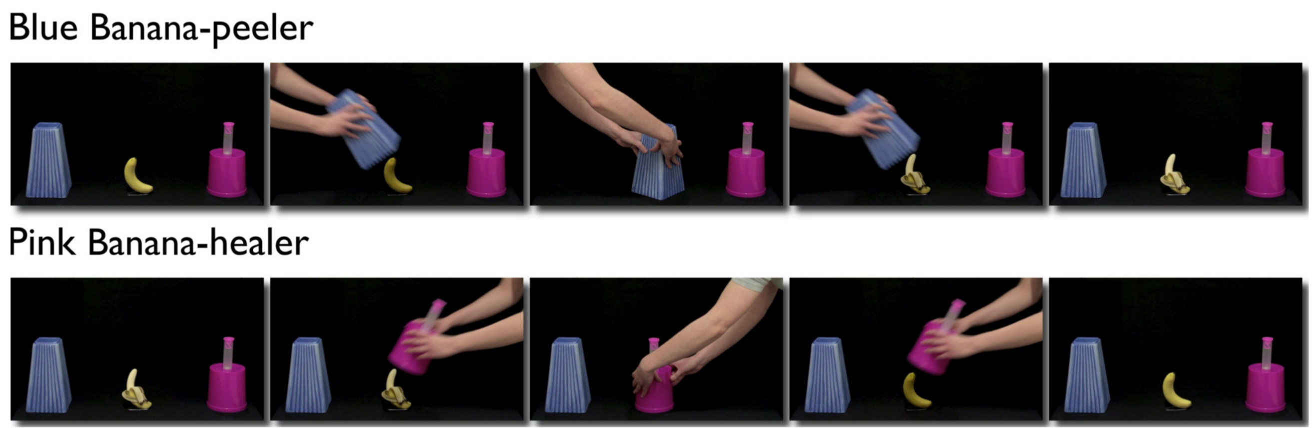

Figure 1.
Still frames from two exemplars of familiarization movies used in Experiments 1, 3 & 4. In Experiment 2, the final state of the banana in each familiarization movie was identical to its initial state. The movies used in test trials never showed the initial state of the banana.

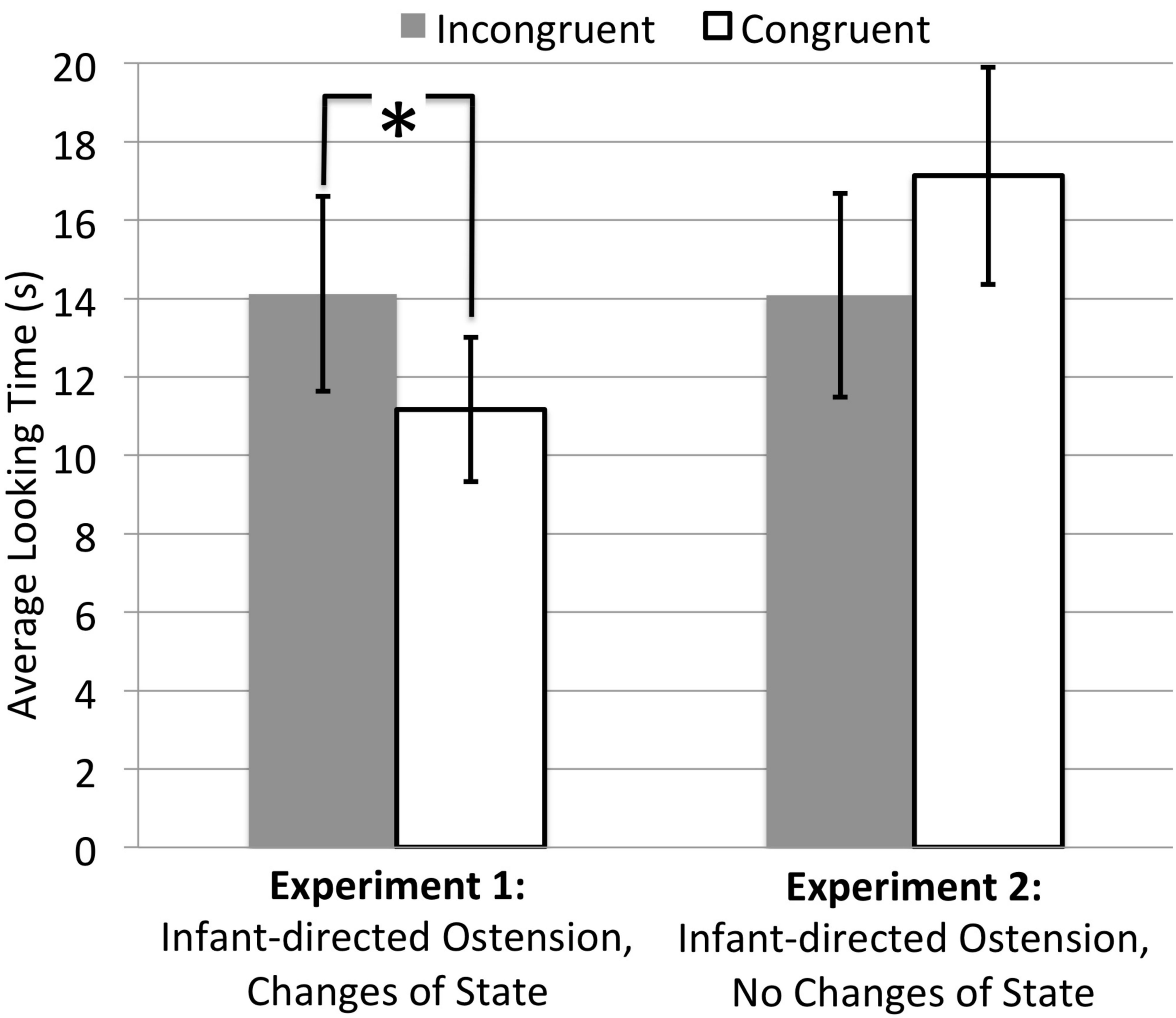

Figure 2.

Average looking times to the test trials in Experiments 1 and 2. Error bars mark standard errors of means. Asterisk marks a statistically significant difference. Note that the raw looking times were log-transformed before parametric tests.

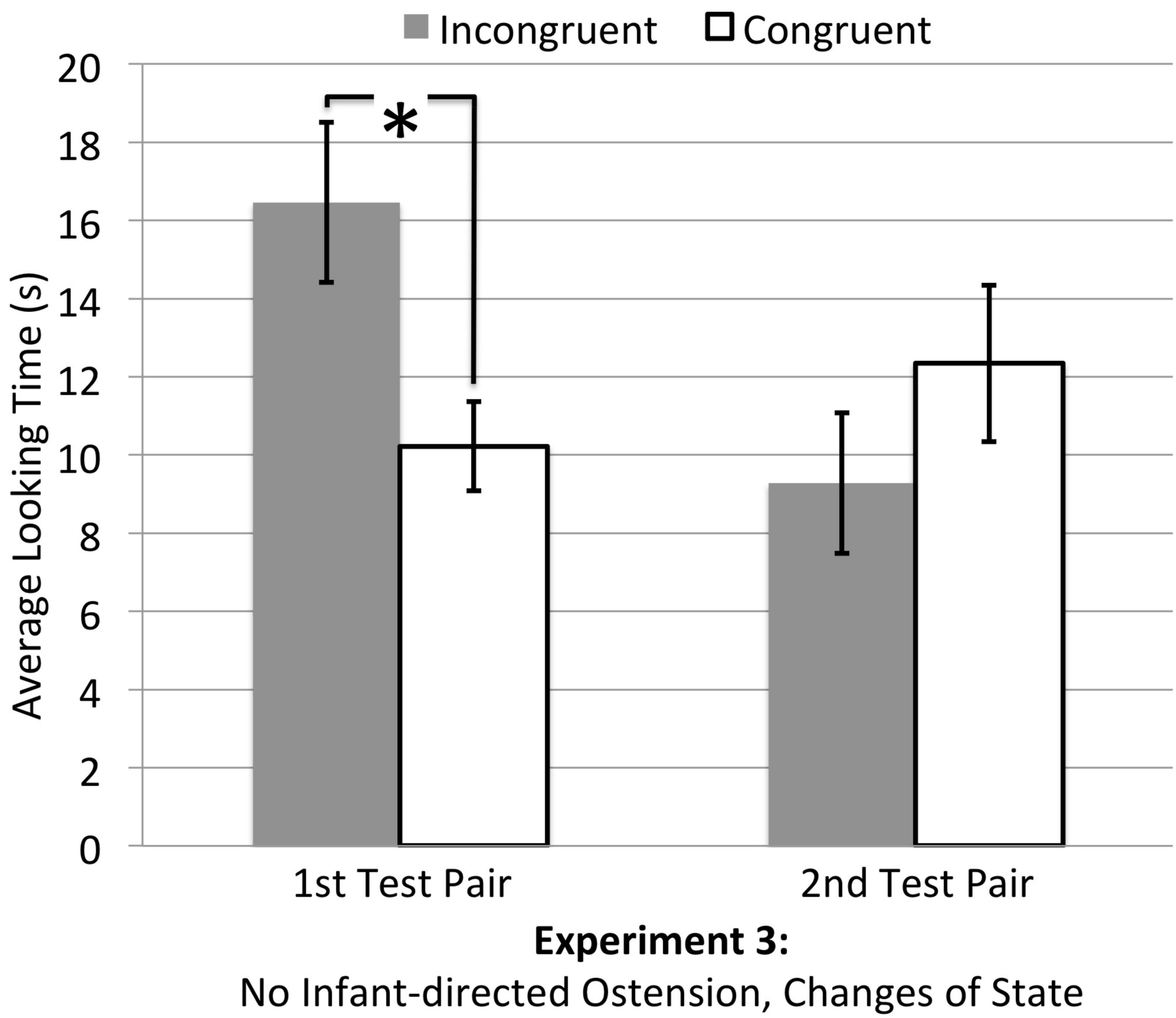

Figure 3.
Looking times to test trials in Experiment 3. Error bars mark standard errors of means. Asterisk marks a statistically significant difference. Note that the raw looking times were log-transformed before parametric tests.

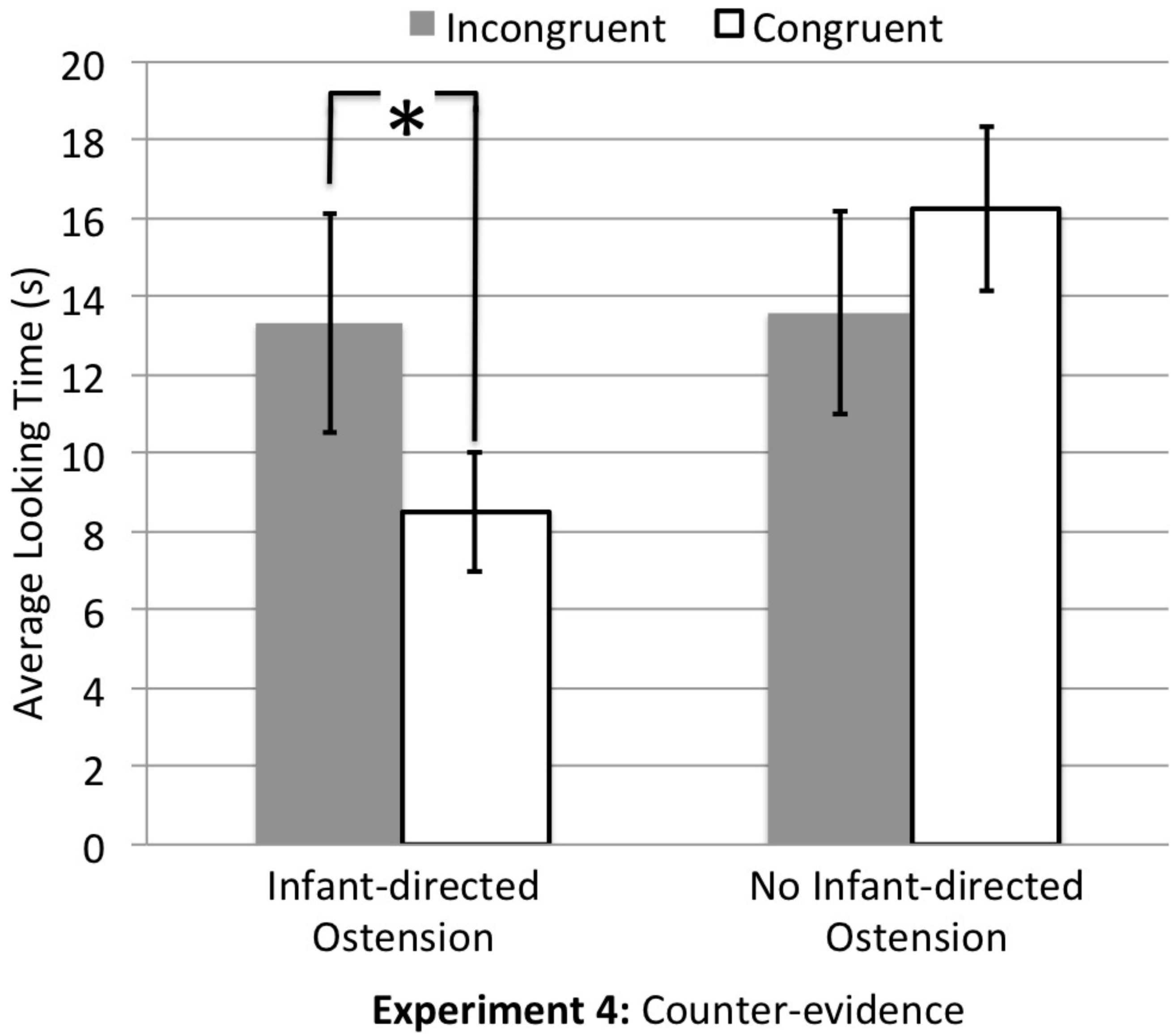

Figure 4.
Looking times to test trials in Experiment 4. Error bars mark standard errors of means. Asterisk marks a statistically significant difference. Note that the raw looking times were log-transformed before parametric tests.