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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 current study tested two hypotheses, derived from these proposals, about the conditions that facilitate the acquisition of enduring functions for novel tools during human infancy. Using a violation-of-expectation paradigm, we show that 13.5-month-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 counterevidence than those acquired from non-infant-directed presentations and, thus, show similarities to generic representations rather than episodic ones. These findings suggest that the acquisition of tool functions during infancy is guided by both teleological action interpretation mechanisms and the expectation that communicative demonstrations reveal enduring dispositional properties of tools.

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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; that is, 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 (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 during 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) - that is, 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) – that is, 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, (a) 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 its kind), (b) 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 (c) 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-month-old infants (Träuble & Pauen, 2007), expectation of exclusive mappings between artifacts and their hidden dispositional properties in infants perhaps as young as 10 months (Futó et al., 2010; but see Casler, 2014, and Defeyter & German, 2003), and beginnings of endurance of function-tool mappings in 24-month-olds (Casler & Kelemen, 2007).

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 (Defeyter, Hearing, & German, 2009; Kelemen, 1999). However, because both designers and users of tools typically aim at maximizing efficiency of instrumental actions, function often can be 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. First, 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. Second, 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 during infancy. Third, identifying the goal of an instrumental tool use demonstrated in a communicative context may allow infants to infer not only the idiosyncratic purpose that the individual tool serves on a particular occasion but also its enduring function, which for adults defines the tool kind (Hernik & Csibra, 2009).

The series of experiments presented in this article explore the conditions that facilitate the acquisition of enduring functions for novel tools during 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: (a) the propensity for teleological action interpretation and (b) 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 article, 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 three types of evidence. First, 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, an agent stopping at a particular object) rather than with respect to their spatiotemporal characteristics (e.g., a hand or an agent moving toward an object along a particular motion path) (Woodward, 1998). Second, 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). Third, infants expect observed consecutive actions toward 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 during 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., 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 handheld 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 television 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. First, sensitivity to function in infants is typically operationalized by assessing whether they encode distinctive correlates of tool function – tool parts, means actions, and spatial locations of the goal (Hunnius & Bekkering, 2010; Paulus et al., 2011; Träuble & Pauen, 2007). Second, even though recent findings are taken to suggest that, by the end of their 1st 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 on in the same way (e.g., rolled across a screen) while two different sounds (e.g., click and 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, thereby displaying evidence of learning separate correlations between the two toys and the corresponding sounds (see also Perone & Oakes, 2006; see review in Oakes & Madole, 2008). Although 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). Extensive habituation procedure and reliance on auditory

¹ According to Perone, Madole, and 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 during infancy.

stimuli make it difficult to compare Baumgartner and Oakes's (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 designed 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 (Butler & Markman, 2012, 2014; Egyed, Király, & Gergely, 2013; Futó et al., 2010; Király, Csibra, & Gergely, 2013; Träuble & Bätz, 2014; Yoon, Johnson, & Csibra, 2008). According to the theoretical proposal of natural pedagogy (Csibra & Gergely, 2006, 2009, 2011; Csibra & Shamsudheen, 2015), ostensive communication has this effect by eliciting a referential expectation (i.e., an expectation that the demonstration has a content) (Deligianni, Seniu, Gergely, & Csibra, 2011; Seniu & Csibra, 2008) and a "genericity bias" toward the content of the demonstration. In the case of tool function demonstrations, genericity bias may lead to an expectation that the predicate (i.e., the demonstrated function) not only applies to the current episode but also represents a permanent property of the object and that the predicate 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, although 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, and the other tool seemed to transform a peeled banana into an unpeeled one. To assess whether infants learned the mapping between each novel tool and its respective end state, we measured 13.5-month-old² 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.

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 16 12-month-olds (M = 12 months 0 days, range = 11 months 21 days to 12 months 16 days), which used a procedure and design identical to Experiment 1 (except that each participant saw only 2 test trials rather than 4), we found no statistically significant effect of test event ($M_{\text{Congruent}} = 19.49 \text{ s}$, $SD_{\text{Congruent}} = 15.77$, $M_{\text{Incongruent}} = 14.59 \text{ s}$, $SD_{\text{Incongruent}} = 14.59$), t(15) = 1.51, p = .15, two-tailed. However, we did find a significant correlation between the index of looking to incongruent events rather than congruent ones (looking time_{Inongruent} – looking time_{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.

Because 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 et al., 2010; see also Yoon et al., 2008), we also embedded our function demonstrations within the context of infant-directed communication.

Method

Participants

The final sample consisted of 16 13.5-month-old infants (M = 13 months 19 days, range = 13 months 3 days to 14 months 2 days). An additional 2 babies were excluded and replaced because they did not finish the procedure due to fussiness. All 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' laps 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 (a black-and-white checkerboard shaking periodically at the center of the screen while a gentle bell sound was emitted) was presented before the 1st, 3rd, 5th, and 7th familiarization trials and before each test trial. After the 8th familiarization trial, a message on the screen reminded parents to keep their eyes closed until 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 Fig. 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 \sim 15 cm wide and \sim 26 cm high, henceforth blue and pink tools) 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), and 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, 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 as unpeeled (banana-healing movies). Each infant saw four peeling movies and four healing movies on alternate familiarization trials, with one of the tools (either pink or blue, fully counterbalanced across participants) being consistently used as the banana-peeler and the other being used as the banana-healer.

The first 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., pink on the left and blue on the right), and the 3rd, 4th, 7th, and 8th familiarization trials showed the tools in the opposite spatial arrangement.

Each familiarization movie had the same prerecorded audio track in which a female voice spoke 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 baby for approximately 5 to 10 min prior to the experiment, 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.

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

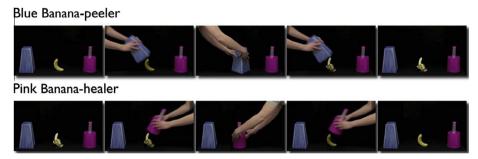


Fig. 1. Still frames from two exemplars of familiarization movies used in Experiments 1, 3, and 4. In Experiment 2, the final state of the banana in each familiarization movie was identical to its initial state. The movies used in test trials the banana.

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 until 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 online coding, or until 60 s elapsed from the beginning of the trial.

For each infant, on 2 test trials (either 1st and 4th or 2nd and 3rd, fully counterbalanced across participants) the outcomes were congruent with the tool–end-state mapping presented during familiarization (i.e., the banana revealed from under the banana-peeler was peeled or the banana revealed from under the banana-healer was unpeeled), and on the remaining 2 test trials they were incongruent (i.e., the banana revealed from under the banana-healer was peeled or the banana 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 1 trial was always congruent and 1 was incongruent. For all infants, all test trials showed the same spatial arrangements of the tools, namely that the blue tool was operated from the left side and the pink tool was operated from the right side.

Measure and coding

Infants were video-recorded at 25 frames per second for offline coding by the first author. The main measure of interest was infants' looking times toward the screen during test trials from the beginning of the outcome phase until infants looked away from the screen for more than 2 s (according to the offline coding) or until the end of the test trial. Test trials from 6 infants (37% of all test trials) were coded offline by a second coder blinded to the conditions. The intercoder 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 being watched for at least 95% of their duration. All infants looked toward the screen 100% of the time during the action phase of test trials.

Average looking times to the test events are depicted on Fig. 2. An initial 2 (Test Pair: 1st vs. 2nd) \times 2 (Test Event: congruent vs. incongruent) analysis of variance (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, or 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

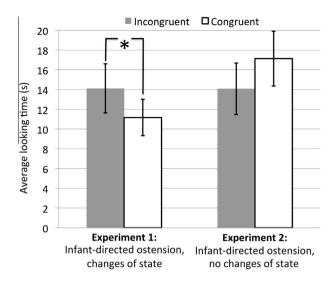


Fig. 2. Average looking times to the test event outcomes in Experiments 1 and 2. Error bars mark standard errors of means. The asterisk marks a statistically significant difference. Note that the raw looking times were log-transformed before parametric tests.

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 being looked at longer in the 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 two-tailed t test and nonparametric Wilcoxon signed ranks test confirmed that infants looked longer at the incongruent test events than at the congruent ones, t(15) = 2.65, p = .018, Cohen's d = 0.66, Wilcoxon Z = -2.4, exact p = .014 (Fig. 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 to bring 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-month-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 subgoals. Because 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 goal states and tools does not require extensive habituation but rather can be induced by only four 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

according to some authors (as already pointed out in the Introduction), 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 early function acquisition (Baumgartner & Oakes, 2011; Madole & Cohen, 1995: Oakes & Madole, 2008).

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 that 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 a peeled banana remained peeled, when revealed from under the tool). Because 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 because in each trial a given tool was 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 teleofunctional account of infant tool understanding advocated in this article. According to this account, infants in Experiment 1 encoded the banana's final state (peeled vs. 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 teleofunctional 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.

Method

Participants

The final sample consisted of 16 13.5-month-old infants (M = 13 months 17 days, range = 13 months 0 days to 13 months 25 days). An additional 4 babies were excluded and replaced because of experimenter's error (n = 1), parental interference (n = 1), looking at a test trial outcome for less than 2 s (n = 1), or fussiness resulting in not finishing the procedure (n = 1).

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 (from 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 excellent intercoder 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 being watched for at least 95% of their duration. All infants looked toward the screen 100% of the time during the action phase of test trials.

Mean looking times to the test trial outcomes are depicted on Fig. 2. An initial 2 (Test Pair: 1st vs. 2nd) \times 2 (Test Event: congruent vs. incongruent) ANOVA on log-transformed looking times showed no significant effects or interaction, all Fs(1,15) < .72, all Fs(1,15) < .73, al

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 two-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 (Fig. 2). Only 6 of the 16 babies looked longer at the incongruent events, and 10 showed the opposite pattern.

A 2 (Test Events: congruent vs. incongruent) \times 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 during familiarization (Fig. 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, toward 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 et al. (2011) used a fixed-length familiarization procedure in which a human hand acted on a small set of objects. They showed 12-month-olds 4 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 the hand had grasped during familiarization). However, if during 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 and the hand grasping the new one, thereby showing no evidence of encoding the details of the outcome witnessed during familiarization. In another condition reported by Biro and colleagues, infants who watched the hand enter the stage and grasp the solitary toy (there was no container in this condition) also failed to display evidence of 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 might not be an easy feat but rather depends on whether the end states can be readily interpreted as action goals (see also Woodward & Sommerville, 2000, and Wynn, 2008). The success of our infant participants in forming tool-end-state mappings in Experiment 1 and the failure to display evidence of them in Experiment 2 are 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 study addressed 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 Experiments 1 and 2 the actions with the novel

³ Note that our manipulation prevented infants from interpreting the *end states* of the action sequences as action goals (and our procedure specifically tested 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 study do not specify what such goals could be, and our task was not designed to test for such attributions.

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 infants that they were being communicatively addressed.

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.

Method

Participants

The final sample consisted of 16 13.5-month-old infants (M = 13 months 18 days, range = 13 months 3 days to 13 months 30 days). An additional 10 babies were excluded and replaced because of equipment failure (n = 1), parental interference (n = 2), looking at a test trial outcome for less than 2 s (n = 3), indiscriminate looking at the screen for the full length of 2 test trials (n = 1), or fussiness resulting in crying or not finishing the procedure (n = 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 spoke with a "flat" prosody typical of adult-directed speech. She said 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 excellent intercoder agreement (r = .99, average absolute difference per trial = 80 ms).

Results and discussion

As in Experiments 1 and 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 being watched for at least 95% of their duration. All infants looked toward the screen 100% of the time during the action phase of test trials.

An initial 2 (Test Pair: 1st vs. 2nd) \times 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 two-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 (Fig. 3). During the first test pair 12 of the 16 babies looked longer at the incongruent event, but during the second test pair only 5 did (Fisher's exact p = .032).

There were no significant effects of order of test outcomes (peeled first vs. healed first) and function assignment (pink peeler–blue healer vs. pink healer–blue peeler), nor were there interactions of either of these factors with test event or test pair, all Fs(1,14) < 0.67, all ps > .43. There was no significant interaction between 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 second pair, F(1,14) = 5.90, p = .029, $\eta_p^2 = .30$, which further suggests

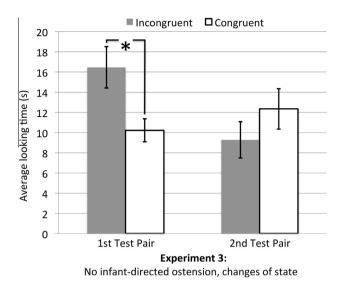


Fig. 3. Looking times to test trial outcomes in Experiment 3. Error bars mark standard errors of means. The asterisk marks a statistically significant difference. Note that the raw looking times were log-transformed before parametric tests.

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, $\eta_p^2 = .17$, 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 a 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 and no interaction (ps > .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 that 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, that is, 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

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, two-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.

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. 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 and Jacob (2012) argued that similar "resilience" patterns are found in infants learning a novel means action (Gergely, Bekkering, & Király, 2002; Király et al., 2013) or a hiding location of a target object (Topál, Gergely, Miklósi, Erdohegyi, & Csibra, 2008; Topál, Gergely, Erdohegyi, 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 may also reflect a difference between generic and 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 directly test 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 banana-healer, and these were presented in the context of either infant-or adult-directed communication (just like during 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 (either the banana-peeler was shown to heal the banana, or the banana-healer was shown to peel the banana).

We expected that infants who were exposed to infant-directed ostensive demonstrations would maintain clear expectations about tool–goal correspondences because 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 mappings were violated, just like infants in Experiment 1 did. On the other hand, if infants who acquired their tool–outcome mappings from familiarization without infant-directed communication encoded them as transient idiosyncratic episodes, they should show a significantly different looking pattern at test. In fact, because in Experiment 4 the counterexample preceded the test trials, these infants might 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 during the initial test trials.

Method

Participants

The final sample consisted of 32 13.5-month-old infants divided into two conditions: infant-directed ostension (M = 13 months 20 days, range = 13 months 9 days to 13 months 30 days) and no-infant-directed ostension (M = 13 months 14 days, range = 13 months 0 days to 13 months 29 days). An additional 15 babies were excluded and replaced because of parent's interference (n = 1),

experimenter's error (n = 2), looking to an outcome phase for less than $2 ext{ s} (n = 4)$, looking away during the action phase of the test (n = 3), fussiness resulting in crying or not finishing the procedure (n = 4), or coding difficulties (n = 1).

Procedure, stimuli, and coding

The experimental and coding procedures, as well as the stimuli, for 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 trials, preceded by the attention-getting animation. The counterevidence 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, and 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 intercoder agreement (r = .99, average absolute difference per trial = 103 ms).

Results and discussion

Overt attention to familiarization events and to the counterevidence 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 being watched for at least 95% of their duration. The counterevidence trial was attended on average for 97% of its duration, with 78% of trials being 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 counterevidence trial ($U_{\text{Mann-Whitney}} = 117$, p = .53). All infants in the final sample looked toward the screen 100% of the time during the action phase of test trials.

An initial 2 (Test Pair: 1st vs. 2nd) \times 2 (Test Event: congruent vs. incongruent) \times 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) \times 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 (Fig. 4). Whereas 11 of 16 babies in the ostension condition looked longer at the incongruent test trials than at the congruent ones, only 4 of 16 babies in the no-ostension condition did (Fisher's exact p = .032, two-tailed).

Planned pairwise contrasts showed that in the ostension condition babies looked significantly longer at the incongruent test events than at the congruent ones, 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 nonparametric Wilcoxon tests on looking times showed a similar pattern, but the effect in the ostension condition did not reach statistical significance (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 representations, which could provide the basis for ascribing kind-defining functions to the novel tools.

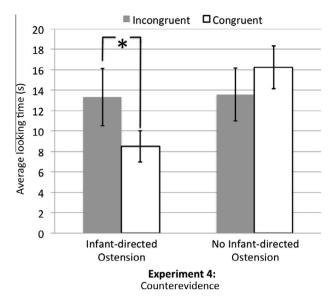


Fig. 4. Looking times to test trial outcomes in Experiment 4. Error bars mark standard errors of means. The asterisk marks a statistically significant difference. Note that the raw looking times were log-transformed before parametric tests.

General discussion

The capacity for goal attribution is a remarkable, well-documented feature of action understanding during 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) that 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 2nd 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, and 4). Notably, several aspects of these findings are consistent with the proposal that the underlying mechanism is guided by teleological action interpretation. First, 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 study participants formed separate tool-end-state mappings despite the two means actions being identical. Second, 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 about the end states. Third, in line with recent reports of early goal attribution (Biro et al., 2011; Hernik & Southgate, 2012; Luo, 2011; Luo & Baillargeon, 2005), in our study infants formed representations of action end states on the bases of relatively few examples. Fourth, 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 (Butler & Markman, 2012; Egyed et al., 2013; Futó et al., 2010; Király et al., 2013; Senju & Csibra, 2008; Topál et al., 2008, 2009; Yoon et al., 2008). 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 suggesting that acquisition of the opaque arbitrary means communicated to infants is constrained by their preexisting expectations about what constitutes a well-formed goal-directed action (Király et al., 2013). When the end state of the observed novel arbitrary action with an object was identical to the state of the environment prior to the action (and, thus, could not be interpreted as the action goal), infants were not likely to imitate the action (Király et al., 2013), or to 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 the contribution from various domains of cognition (e.g., physical, motor, social). The aim of our experiments 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 during infancy. Our experiments found positive evidence for such generic teleofunctional understanding of tools in 13-month-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 whether 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 experiments. 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. Because both the capacity for goal attribution and the sensitivity to ostensive communicative signals are available to infants much earlier in their 1st year of life (Csibra, 2008; Csibra & Gergely, 2006; Luo, 2011), 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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