I Know Something you Don't Know: Twenty-month-olds' Tool use Learning From Exploration and Social Interaction

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CHAPTER 1

Introduction

If asked to think about the development of tool use, we might imagine an early hominid experimenting with a bone hammer à la Stanley Kubrick's Dawn of Man, or perhaps we picture little Lucienne Piaget learning to attain a distant ducky by tugging the cloth it lies upon. In both cases, the image is likely one of individual exploration. However, when we think of our own experiences learning to use tools – particularly complex tools like a car or a new cell phone – we're far more likely to recall not only our own independent exploration but also a fair amount of observing or receiving direct assistance from a more experienced tool user. This balance – or imbalance – between independent exploration and social learning pervades tool use in the real world and in research. Although we may think of tools like spoons or hammers to be far too simple to require this combined approach, for a human infant they are just as complex as a car or cell phone. The following experiment examines the question of how infants negotiate the trade-off between relying on their own object exploration abilities and imitating another's example when learning how to use new tools.

First, let us consider the role of independent object exploration in tool use learning. All of the key components of tool use relate to object exploration: In order to use a tool an individual must perceive the affordances of a tool object in relation to other objects in the world, plan a sequence of actions to utilize those affordances, and execute the sequence successfully. By around 6 months of age, infants are readily exploring objects manually, orally, and visually, and in doing so learning affordances and cause-effect relations between their own bodies and those objects as well as between multiple objects (Gibson, 1988, Gibson, 2000; Kahrs, Jung &

Lockman, 2012a, 2012b; Lockman, 2000). Soon after, infants show evidence of motor planning in their minute adjustments of hand shape and velocity based on the affordances of the object (Lockman, Ashmead & Bushnell, 1984; McCarty, Clifton, Ashmead, Lee & Goubet, 2001; Barrett, Traupman & Needham, 2008; von Hofsten & Rönnqvist, 1988) or future goals for the object (Claxton, Keen & McCarty, 2003). By the second year of life, infants have advanced object exploration skills (Ruff, 1984), and exploratory ability has been linked to problem-solving ability in tool use scenarios (Caruso, 1993). As infants gain more familiarity using tools, they learn to adjust their motor plans during execution, correcting awkward first grasps of objects or altering their initial inefficient trajectory with an object while en route to a goal (Connolly & Dalgleish, 1989; McCarty, Clifton & Collard, 1999; McCarty & Keen, 2005).

Although no account of this developmental trajectory of tool use has failed to comment on the importance of object exploration (or its more vague alias, "experience"), it is obvious that using exploration alone to learn about tools is inefficient. It would be surprising for an adult to spontaneously discover an efficient method for using a can opener, eyelash curler, or jigsaw through exploration, and even an infant independently discovering the myriad uses of a spoon seems far-fetched. Instead, adults (Dennett, 1987) and even school-age children (Kelemen, 1999) tend to employ a 'design stance' when reasoning about and using artifacts, privileging the intended functions of an object over possible other functions. The strength of this approach is improved efficiency, while a weakness is 'functional fixedness', or difficulty using artifacts with known functions in novel ways (Duncker, 1945). Although children 5 years of age and younger have been shown to be resistant to functional fixedness and more likely to use known artifacts in various or novel ways than older children (Defeyter & German, 2003; German & Defeyter, 2000), there is evidence that even toddlers' tool use is guided by their prior knowledge of tools.

For example, 13- to 18-month-old infants struggle to use a familiar tool in a novel way (Barrett, Davis & Needham, 2007), while 2-year-olds readily apply tool use knowledge gained from observing a demonstration or listening to another person's testimony about a tool's function to their behaviors (Casler & Kelemen, 2005, 2007).

With only this glimpse of the research, there are already indications that social learning of tool use is a double-edged sword. However, before diving into the rich literature on tool use learning through social interaction, it is useful to highlight and define two key terms that will reappear frequently: Imitation and social-pedagogical cues. In this dissertation, I will use the term imitation to refer to an organism copying the displayed behaviors of another organism, including object choice (e.g., selecting one toy over another), order of behaviors (e.g., using one toy before another), motor actions (e.g., holding or using an object in a particular way), and goals (e.g., directing actions or objects toward a particular purpose). Although researchers offer differing explanations for the origin of human imitation of conspecifics and a reliable age of onset (see Meltzoff, 2005 versus Ray & Heyes, 2011), by all accounts imitation appears early in human ontogeny and appears to serve both learning and social purposes, particularly in toddlerhood when language skills are still nascent (Nadel, 2002, 2006; Nielsen, Suddendorf & Dissanayake, 2006). Although behavior can be rated on a scale of imitativeness, the term imitation generally refers to behavior that is fairly loyal to the model and is thus differentiated from emulation, which involves a copying of a method (e.g., moving a tool in a particular way) or a means (e.g., attaining a toy) but not both (e.g., moving a tool in a particular way to attain a toy) (Nagell, Olguin & Tomasello, 1993).

As for social-pedagogical cues, these are more precise than social cues in that they go further than simply attempting to engage a social other and explicitly attempt to direct another's attention or behavior. Researchers have not compiled a definitive list of social-pedagogical cues, but the term often includes eye contact, contingent interaction, pointing, gaze direction at a target object, gaze switching between a target object and a potential learner, infant-directed speech or 'motherese,' and semi-directive or explicitly directive language that ranges from pithy ("Look, [*child's name*]!") to elaborate ("Now it's your turn."). By around their first birthdays infants show sensitivity to the presence or absence of social-pedagogical cues in learning contexts (Gergely, Egyed & Király, 2007), and some even go so far as to posit that this sensitivity arose in human evolution due in large part to the creation and dissemination of tools (Csibra & Gergely, 2006).

Imitation and social-pedagogical cues are indispensible in any examination of tool use development, and are closely intertwined. Occurrence and faithfulness of imitation is noticeably high around 12 to 36 months of age (Nielsen et al., 2006), a time when tool use is also rapidly developing (e.g., Connolly & Dalgleish, 1989). Infants in the second year of life readily imitate, or attempt to imitate, others' tool use behaviors (Barrett et al., 2007), and tend to be highly faithful tool use imitators (Barrett et al., 2007; Nagell et al., 1993). Similarly, demonstrations of tool use that incorporate social-pedagogical cues have been shown to positively influence infants' learning about tool use to a greater extent than demonstrations that are non-social or social but not explicitly pedagogical (Sage & Baldwin, 2011). To round out the relations between these ideas, the presence of social-pedagogical cues in tool use demonstrations appears to strongly affect toddlers' and preschoolers' imitation of adults' tool choices (Casler & Kelemen, 2007; DiYanni & Kelemen, 2008; DiYanni, Nini & Rheel, 2011) and tool use behaviors (Nielsen, 2006; Nielsen, Simcock & Jenkins, 2008).

With both the social learning and exploration research in mind, it would seem that exploration takes infants partway to learning about complex tools, and social learning takes them the rest of the way. Indeed, baseline data with 13- to 18-month-olds (Barrett et al., 2007) and 2-year-olds (Elsner & Schellhas, 2012) illustrate that toddlers often fail entirely to enact an efficient tool use method or even to use a tool instrumentally without having first observed an adult's imitable demonstration. However, information gleaned from social versus exploratory learning about tools may conflict, and this conflict has led to large differences in experimental results based on small differences in methodology. These differences form a puzzling knot that, with this dissertation, I hope to begin unraveling.

On one hand, we see imitation amending earlier, fruitless exploration, but preventing further, potentially fruitful exploration. For example, both 24-month-olds (Elsner & Schellhas, 2012) and 18-month-olds (Barrett et al., 2007) who fail to discover a functional tool use method while exploring on their own will go on to adopt tool use methods others demonstrate to them. These same infants will persist in using those methods on novel tasks even when simple exploration would reveal that the new task requires a different method for success (Barrett et al., 2007; Elsner & Schellhas, 2012). On the other hand, we see examples of fruitful exploration reducing imitation later, with both positive and negative consequences. It is noteworthy that each example involves an infant discovering a simple, successful tool use method before they witness a demonstration of a more complex or less effective tool use demonstration, which they then choose to ignore. For example, 18-month-olds who had previously discovered a simple solution to a task on their own (i.e., illuminating a touch light with one's hand) were less likely to imitate a more complex solution demonstrated by an adult (i.e., illuminating the same light with one's head) than infants who had had no opportunity to explore the task on their own prior to an adult's demonstration (Pinkham & Jaswal, 2011). Similarly, 24-month-olds who had observed a model reject an optimally functional tool in favor of a nonfunctional tool reliably chose to use the optimal tool later, although they were at chance in their choice of tool if the model had rejected an optimal tool in favor of a suboptimal but still functional tool (DiYanni & Kelemen, 2008).

Taken together, these findings demonstrate that infants use both their own knowledge gained via exploration and information gleaned from social-pedagogical interactions with others to guide their tool use choices and behaviors. However, it is unclear if infants reliably weigh information from each of these learning methods when making tool use decisions during the second year of life, and how specific their reasoning abilities on that point are at this age. Multiple studies have suggested that children 24 months of age and younger do not reliably demonstrate optimal rather than suboptimal tool use behaviors when learning about tools (Barrett et al., 2007; Nagell et al., 1993), even if they have seen demonstrations showing that a certain tool or tool use behavior is suboptimal or ineffective (DiYanni & Kelemen, 2008; Want & Harris, 2001). However, such studies have used strong social-pedagogical cues to encourage imitation when demonstrating suboptimal or incorrect tool use (DiYanni & Kelemen, 2008; Nagell et al., 1993) or when demonstrating tool use behaviors that will not transfer to novel scenarios (Barrett et al., 2007). In some particularly confounding cases, infants have been offered meager social-pedagogical cues discouraging imitation of poor tool use alongside equally meager cues encouraging imitation of successful tool use (Want & Harris, 2001). In all cases, infants are given no experience of exploring the tool use scenario successfully on their own prior to being tested on their tool use choices or behaviors.

Although these details may seem trivial, other research findings suggest they are in fact crucial to our interpretation of infants' tool use learning abilities. The presence of useful prior knowledge gained via exploration stands out as a particularly important point to clear up given that exploration is recognized as a key factor in infants' tool use development (e.g., Lockman, 2000), and given the compelling and contrary results of studies that have manipulated prior knowledge. For example, infants under 24 months of age who have experience accomplishing a novel tool use task (i.e., retrieving a distant toy with a stick-like tool) will successfully choose a functional tool from an array of differentially affordant tools later without assistance (Brown, 1990). Even 18-month-olds who have learned a simple solution via independent exploration first are far more likely to fail to imitate a more complex solution learned from a pedagogical interaction than infants who were not able to explore the task beforehand (Pinkham & Jaswal, 2011). Although these studies strongly suggest that infants will use prior knowledge gained via exploration in social-pedagogical tool learning scenarios if they are given the chance to do so, they are inconclusive given that the design of Brown (1990) involved some social-pedagogical demonstration of tool use during the exploration phase, and Pinkham and Jaswal (2011) did not involve tool use.

The necessity of further research is clear, and several design factors emerge as worthy of attention. First, there is the issue of age. Infants tend to be understudied in the tool use literature compared to preschoolers, but they have the social and exploratory skills to use tools and are learning to use them in the world outside the lab. This study therefore focuses on 20-month-olds. Second, in previous studies subjects are rarely given the opportunity to explore a tool use task prior to a demonstration, or if they are the tasks are too complex and unfamiliar for children to make much headway on their own. In this study, all infants are given the opportunity to explore the task before seeing it demonstrated, and the task and tool are manipulated to allow for variation in infants' discovery of an effective solution while exploring. Third, in the aforementioned studies infants are often shown confusing demonstrations. Children see both helpful and unhelpful tool use methods one after the other with little differentiation in social cues between them (e.g., Want & Harris, 2001), they see an experimenter praise a nonfunctional tool and reject a functional one after demonstrating both (e.g., DiYanni & Kelemen, 2008), or they are shown a method that is entirely inappropriate for an upcoming task (e.g., Elsner & Schellhas, 2012). In this study, each infant is shown only one demonstration that is clearly effective or ineffective, and is marked accordingly with social-pedagogical cues. With these changes, I hope to demonstrate that infants can successfully weigh tool use information gleaned from exploration and observation as they have been shown to in other domains (e.g., Pinkham & Jaswal, 2011), and will not simply imitate an experimenter or choose randomly between optimal and suboptimal tools (e.g., DiYanni & Kelemen, 2008).

The task utilized in this study – a commercially available pegboard toy that was paired with one of two different hammer tools – was selected because it fulfilled two criteria. First, it is likely that infants would have had some prior experience with the task prior to participating in the study. This was desirable as I wanted infants to be able to attempt and potentially discover a solution to the task without seeing a demonstration beforehand – the task could not be so obscure or unfamiliar that infants would not be able to conceive of the goal and a method to achieve it on their own. Second and more subtly, this task is associated with a specific standard tool and tool use method, namely a hard hammer tool and a hammering action that involves striking the peg forcefully multiple times using the head of the tool. Therefore, infants were very likely to attempt to use this standard approach when solving the task on their own. This was important because I manipulated the properties of the tool so that the standard approach would not always be effective.

Half of the infants in the study explored the task with a hard wooden hammer, while the other half explored the same task with a soft sponge hammer. The hard tool was highly effective when used with a standard hammering method, while the soft tool was considerably less effective when used with the same method. I predicted that this manipulation in tool properties would lead to differences in infants' effectiveness at solving the task during the explore phase. Infants who explored the hard tool and used the standard hammering method with which they were already familiar would be highly effective, while infants who explored the soft tool with the same method would not be. Following this varied experience, the experimenter then demonstrated the same task using a different tool. For infants who had explored the hard tool, the experimenter demonstrated hammering with the soft tool to poor effect. For infants who had explored the soft tool, she demonstrated hammering with the hard tool to great success. The success or lack of success in each demonstration was deliberately and repeatedly marked with social-pedagogical cues. Finally, infants were tested by being given the same task again, but with a choice of which tool to use.

For the explore phase, I expected that all infants would be highly likely to use a hammering method, which in turn would lead to infants being more likely to be effective on the task with the hard tool than with the soft tool during the explore phase. Their explore phase effectiveness as well as the social-pedagogical cues used during the demonstration should influence their imitation at test: I predicted that infants who had not been effective at explore (i.e., those who had the soft tool at that phase) would be likely to imitate the experimenter by using the tool from the effective demonstration, while infants who had been effective at explore (i.e., those who had the hard tool at that phase) would be unlikely to imitate the tool used in the ineffective demonstration.

Of course, infants' behaviors might vary based on other factors uncontrolled by this experiment. To capture those differences, I included questionnaires in my experimental design so that they could be included in analyses testing my predictions. First, I included the Early Childhood Behavior Questionnaire (ECBQ), a short parent-report temperament questionnaire devised specially for toddlers (Putnam, Gartstein & Rothbart, 2006). Each measure pertains to qualities that may affect tool use success: Impulsivity and activity level (surgency), frustration and fear (negative affect), and ability to control attention and inhibit prepotent responses (effortful control). To use a tool successfully, infants must learn the affordances of objects, conceive and apply a strategy that incorporates those affordances, and correct the strategy during execution if faced with failure. Each step of this process could be influenced by temperament factors: Infants with high surgency and low negative affect may more eagerly engage with the objects and so learn their affordances more easily, while infants with high surgency and low effortful control may be too impulsive to settle on a single strategy or see it through to completion, and those with high effortful control and low negative affect may possess the tenacity and calm to correct mistakes and accomplish the task.

In addition to the ECBQ, I also included a questionnaire pertaining to infants' prior experience with hammers. This questionnaire was devised in the lab to pertain to this project specifically and included questions about infants' prior experience with real and toy hammers, including toys like those used in the experiment. The questions covered a range of prior experiences, from seeing hammers but not playing or hitting anything with them, playing with hammers but not using them to hit anything, and experience using hammers to hit other objects.

CHAPTER 2

Method

Participants

Forty-six 19.5- to 21.5-month-old infants (21 female) participated in this study. Participants were split between two training conditions: hard tool explore/soft tool demo (23 infants, age: *M* = 20 months, 24 days; *SD* = 19 days; 11 females) and soft tool explore/hard tool demo (23 infants, age: *M* = 20 months, 17 days; *SD* = 25 days; 10 females). Of all the infants, 89% were identified as not Hispanic or Latino by their parents, 4% as Hispanic or Latino, and 7% preferred not to answer. Of all the infants, 85% were identified as White or Caucasian by their parents, 4% as mixed race, 4% as Asian, 4% as Black or African American, and 2% preferred not to answer.

Data from an additional 9 infants were collected and excluded due to infants' failure to participate in the test phase of the experiment.

Materials

Tools. There were two hammer tools that were different in shape and material but designed to be highly similar in all other respects. Both tools had plain wooden handles, measuring 16 cm on the hard tool and 15.5 cm on the soft tool. The hard tool (Figure 1) had a cylindrical gray wood head that was hard enough to hammer pegs measuring 6 cm long and 3.5 cm in circumference. The soft tool (Figure 2) had an asymmeterical gray sponge head that was too soft to hammer pegs, although it could be used successfully to push pegs, measuring 6.5 cm long, 4.5 cm in circumference on its small end, and 5.5 cm in circumference on its large end. Due to this difference in material, the soft hammer was marginally larger and more lightweight than the hard hammer. These differences were preserved as they were too slight to strongly affect the ability to grasp or use either tool, and they allowed the tools to be easily visually distinguishable.

Figure 1. The hard tool.

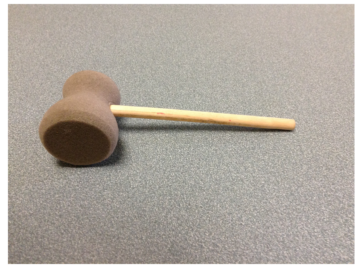

Figure 2. The soft tool.

Warm-up items. The warm-up phase utilized three commercially available plastic bath toys and a single rectangular bucket measuring 14 cm high and 11.5 cm across. A child could easily grasp each bath toy with one hand. The rectangular bucket was made of clear plastic and decorated with red electrical tape confetti. (A second, identical rectangular bucket was used as part of the test phase.)

Task. The task was a commercially available toy pegboard (Figure 3) measuring 27 cm long, 8 cm across, 12 cm high at each leg, and 5 cm high in the center. The pegboard featured eight holes and pegs, but for the purposes of this study all but one hole were covered in red electrical tape and only one red, round peg measuring 8 cm long and 2 cm in circumference at each end was used. This was done to simplify the task and reduce distractions.

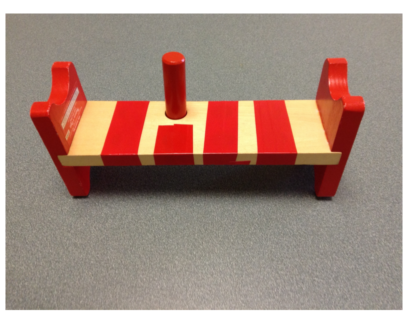

Figure 3. The pegboard task.

Procedure

An experimenter obtained informed consent from the parent in the waiting room of the lab before the experiment began. In addition to the consent documents, during this time parents completed the ECBQ and the hammer experience questionnaire. While the parent filled out these forms, the infant met the experimenter and warmed up with the lab staff by playing with toys in the waiting room. Before the experiment began, the parent was instructed not to grasp, use, or play with any objects if the infant offered one to them, or to direct or guide the infant's behavior in any way. Parents were permitted to respond otherwise to their infant's interactions with them as they normally would.

In the center of the testing room was a large, kidney-shaped table. The parent sat in a chair in the dip of the kidney-shaped table, and the infant was seated in the parent's lap. The experimenter was seated across from the parent and baby, with all the experimental stimuli concealed in shelves under the table. Video cameras stationed directly above, facing, to the left, and to the right of the infant recorded the entire experimental procedure for later coding.

Warm-up phase. In this phase, the experimenter placed one of the rectangular containers on the table within reach of the child. The experimenter then offered a bath toy to the child. When the child took the toy, the experimenter encouraged the child to put it in the bucket. After the child did so, the experimenter repeated this process with two other toys. The experimenter then poured all three toys back out onto the table, and encouraged the child to place them one by one back in the bucket. The process of pouring out the toys and having the child replace them was repeated, and then the toys and bucket were retrieved. This phase was included as earlier pilot data indicated that infants were often too shy to grasp, play, or use the tool or pegboard during the explore phase if the experiment began with the explore phase and omitted a warm-up.

Explore phase. The content of this phase varied according to the infant's condition. In the hard tool explore/soft tool demo condition, the experimenter put the hard tool and the pegboard with the peg placed in it on the table close to her, out of reach of the child. The tool was placed on the side of the pegboard closest to the child, and the peg was arranged so that its bottom end was flush with the bottom of the board, leaving the peg free to be hammered down but unlikely to fall out of the board. The experimenter then said to the child, "Can you hammer the peg?" and tapped the top of the peg lightly with her finger. The experimenter then pushed the pegboard and tool toward the child. The child was permitted 30 seconds of free play with the pegboard, peg, and tool. In rare cases that the child appeared distracted and did not immediately engage with the toys, the experimenter would lightly tap the peg again with her finger. After the 30 seconds were complete, the experimenter retrieved all of the toys and stowed them under the table out of sight of the infant.

The procedure during this phase in the soft tool explore/hard tool demo condition was identical, with the exception that the tool offered was the soft tool.

Demonstration phase. The content of this phase also varied depending on the infant's condition. In the hard tool explore/soft tool demo condition, the experimenter brought out the pegboard with the peg in place as in the explore phase, but with the soft tool positioned on the side of the pegboard closest to the child. The experimenter then made eye contact with the child while announcing, "I'm going to hammer the peg!" The experimenter grasped the handle of the soft tool and struck the peg with the head of the tool three times in quick succession. This action had no effect on the peg, and the experimenter reacted by furrowing her brow, frowning, and saying, "Hmm!" in a disappointed tone of voice. The experimenter then removed all items from the table and repeated the sequence of actions with commentary twice, for a total of three unsuccessful demonstrations. All items are then removed from the table and stowed out of sight.

The soft tool explore/hard tool demo condition was identical to the hard tool explore/soft tool demo condition in terms of the arrangement of objects and the basic sequence and number of events. However, in this condition the experimenter brought out the hard tool with the pegboard, and the head of the tool effectively hammered down the peg with each strike until the peg reached its lowest possible point after three strikes. After this effective display, the experimenter reacted by raising her eyebrows, smiling, and saying, “Hmm!” in a pleased tone of voice. When the items were removed from the table the experimenter returned the peg to its initial position while it was out of sight, and then repeated her actions for a total of three successful demonstrations. All items were then removed from the table and stowed out of sight.

Test phase. In this phase, the experimenter took out the two clear plastic buckets and placed them approximately two feet apart from each other on the table out of reach of the child. The experimenter then announced, “Look! This one goes here,” and placed the explore tool in one bucket, “and this one goes here!” and placed the test tool in the other bucket. The tools were placed handle-down in the buckets, allowing the head of each tool to protrude from the top of the container and thus ensuring that the tools were visibly distinct. Whether the explore tool was placed in the bucket to the infant’s left or right was counterbalanced between subjects.

The experimenter then announced, “Now it’s your turn!” and took out the pegboard with the peg in place ready to be hammered. She pushed the pegboard and peg into the child’s reach then directed her gaze back and forth between the buckets, asking, “Which one do you want to use?” If the child pointed or reached toward one of buckets at this point, the experimenter would push the bucket with the tool within the child’s reach, and the child was permitted to play freely with the tool, pegboard, and peg until they ceased acting upon the items or after 60 seconds, whichever came first. If the child reached or pointed toward the other bucket after having attained the first bucket, it was also pushed within the child's reach.

If the child failed to choose (e.g., pointed or reached toward both or neither bucket), the experimenter repeated her question but did not push either bucket closer to the child. Most children pointed or reached toward one bucket at this point and were then allowed to play freely until they ceased interacting with the items. However, in rare cases that a child failed to do so the experimenter would leave the buckets far from the child and continue repeat her question until the child pointed or reached toward one bucket or up to two more times (i.e., four repetitions total), whichever came first. If an infant still failed to choose at this point, the experimenter would then push both buckets within the child's reach simultaneously. All items were retrieved and stowed when the infant ceased to act upon them or after 60 seconds, whichever came first.

Measures

A total of eleven measures were taken: Six were taken from the temperament and hammer experience questionnaires, and four were coded from each infant's video. The five coded measures describe infants' actions during both the explore and test phases (action object, action type, action location, action duration, action effectiveness). A trained coder coded all of the measures from the prerecorded video of each infant using Datavyu coding software (Datavyu Team, 2014).

Early childhood behavior questionnaire. The ECBQ allows for the coding of three different measures of temperament: Surgency, negative affect, and effortful control. These three scores were calculated for each infant from their questionnaires according to the instructions laid down by Putnam and colleagues (2006). Surgency scores ranged from 3.80 to 6.50 (M = 5.34;

SD = 0.61 points), negative affect scores ranged from 2.30 to 4.50 (*M* = 3.17; *SD* = 0.64 points), and effortful control scores ranged from 3.30 to 6.20 (*M* = 4.40; *SD* = 0.56 points).

Prior experience with hammers. Each question on the prior experience with hammers questionnaire was assigned a range of point values based on the range of possible answers for that question. Each infant's prior experience score represents the percentage of points earned out of all possible points. All infants were reported to have some prior experience with hammers, and scores ranged from 18.00% to 100% (*M* = 60.30%; *SD* = 18.52 points).

Action object. This categorical measure indicated what object the infant used to perform a given action. There were five categories: Hard tool head, soft tool head, hard tool handle, soft tool handle, or the infant's own hand(s).

Action type. This measure indicated the nature of the action the infant performed using a tool head, handle, or hand. Two categories of action were of interest: Hammering and pushing. Hammering actions involved the infant rapidly raising and lowering the action object and striking it against another object, such as the peg or table. Pushing actions involved the infant pressing the action object against another object.

Action location. This measure indicated where the infant directed any given action. Two categories of location were recorded: Peg or elsewhere. Actions directed within 2 cm of the peg were coded as peg, further than 2 cm away from the peg but on the pegboard or against the table as elsewhere. Only actions directed at the peg were considered goal-directed and examined in this study. Follow-up research may more broadly examine the potential influence of off-task learning about tools on on-task behaviors with tools, but those questions are beyond the scope of the present project.

Action duration. The duration of each single action or bout of repeated actions was measured in milliseconds.

Action effectiveness. Actions were coded for their effectiveness in lowering the peg. There were two categories of effectiveness: Not effective and effective. Any action directed at the peg that resulted in the peg lowering was coded as effective, and any action directed at the peg that resulted in no change in the peg's height was coded as not effective.

Reliability

For reliability purposes, a second trained research assistant coded a subset of the full dataset (N=7) which contained 126 separate actions. Reliability between coders on the action duration measure was calculated using a Pearson's product-moment correlation. There was a strong positive relationship in coders' recorded action durations, $r(126) = .99$, $p <. 0001$. Reliability between coders on all other action measures was calculated using Cohen's kappa formula, and was found to be high for all measures: For action object $\kappa = .97$, $p < .05$; for action type $\kappa = .84$, $p < .05$; for action location $\kappa = .92$, $p < .05$; and for action effectiveness $\kappa = .82$, $p < .05$.

CHAPTER 3

Results

Over the course of gathering data and coding infants' behaviors, both a greater and lesser variety of behavior than had been anticipated emerged. In order to do justice to this as clearly as possible, the raw codes on infants' action object, type, location, duration, and effectiveness were used to derive more straightforward measures, often dichotomous measures, of infants' behavior during the explore and test phases. These measures were used for analyses instead of the raw action codes because they better express the variability or lack thereof in the various data.

Before presenting the analyses I conducted, I must define terms and explain the derived measures. In this dissertation the word "method" describes any action directed on the peg during any phase of the experiment. A rough examination of the raw action object, type, and location codes revealed that infants used a total of four distinct methods: Using the head of a tool to hammer, using the head of a tool to push, using the handle of a tool, or using one's hand. Incidence of the handle method was low (19.57% of all infants), was never the only or preferred method used by any infant, and was uniformly ineffective. For these reasons, I did not differentiate between hammering or pushing with the handle. Likewise the hand method invariably involved pushing, so no further differentiation was necessary. With these distinctions in mind, the terms "hammer" or "hammer method" will refer only to using a tool's head to hammer the peg, the terms "push" or "push method" will refer only to using a tool's head to push the peg, "handle" to any action on the peg using a tool handle, and "hand" to any action on the peg using the hand.

Descriptive Statistics

Method number. Method number represents the sum of all methods used by an infant during a given phase. It was calculated separately for explore and test. Explore method number ranged from 0 to 4 methods (M =1.32 methods; SD = 0.87 methods) and 86.96% of infants used one or more methods during the explore phase. Test method number ranged from 1 to 4 (M =2.11 methods; SD = 0.92 methods), and 100% of infants used one or more methods during the test phase.

Hammer method at explore. This dichotomous measure designated whether an infant used a hammering method on the peg during the explore phase. This measure was included for two reasons. First, this method was strongly cued. It is the conventional method for this task and it was verbally prompted at the onset of the explore phase, both of which would lead me to expect that its incidence would be quite high. This was the case, as 67.39% of infants used this method during the explore phase. Secondly and more importantly for the research question and hypotheses of this study, this method would lead to a difference in effectiveness between the two tools and therefore an important difference in learning during the explore phase. While pushing with a tool head, using a tool handle, or using one's hand could be done with more or less equal effectiveness regardless of the infant's choice of tool, hammering with the tool head was effective only with the hard tool.

Effectiveness. This dichotomous measure indicated whether or not an infant had been effective at least once during a given phase. This measure used the same two levels (not effective and effective) as the effectiveness raw code, but summarized over the entire phase rather than describing a single action. As expected and desired, effectiveness was variable but by no means at floor with this age group, as 58.70% of all infants were effective during the explore phase and 80.43% were effective during the test phase.

Tool imitation at test. This dichotomous measure indicated whether an infant imitated the experimenter's demonstrated tool at test. Imitation was defined as using the same tool as the experimenter first or exclusively during the test phase. Infants who never used the same tool as the experimenter or used it only after using the other tool were categorized as not imitating the experimenter's tool. The sample was roughly split in half in terms of which tool infants used first at test, with 47.83% using the hard tool first during the test phase and 52.17% using the soft tool first in the same phase.

Method imitation at test. This dichotomous measure indicated whether an infant imitated the experimenter's demonstrated method, i.e., hammering, at test. Imitation was defined as using the same method as the experimenter first or exclusively during the test phase. Infants who never used the same method as the experimenter or used it only after using another method were categorized as not imitating the experimenter's method. This measure was included because hammering is conventional, was strongly and repeatedly cued during both the exploration and demonstration phases, and as one would expect incidence of its use by infants was high. During the test phase 89.13% of all infants used a hammering method at least once, and 58.70% of all infants used it first during that phase.

Preferred tool at test. This dichotomous measure indicated whether an infant spent more of their time on the task at test using the hard or the soft tool. Infants had to use one tool exclusively on the task or for a greater length of time in milliseconds than they used the other tool on the task in order to be categorized as preferring that tool. All of the infants chose and used at least one tool during the test phase and 56.52% of all infants went on to choose a second tool as well, although only 39.96% of all infants used a second tool on the task during the course of the test phase. (The remaining infants who chose a second tool simply held or played with it off-task.) In the entire sample of 46 infants, only 1 infant showed no preference between the two tools at test. This infant spent an equal amount of time using each tool.

Analyses

Hammer method at explore. A binomial logistic regression was performed to ascertain the effect of explore tool, sex, age, the three ECBQ measures (surgency, effortful control, and negative affect) and prior experience with hammers on whether infants used the hammer method during the explore phase. There were two levels of the dependent variable: Not using a hammer and using a hammer method. Not using a hammer method was the reference category. The logistic regression model was not statistically significant, $X^2(7) = 6.88$, $p = .442$. The model explained 19.40% (Nagelkerke R^2) of the variance in using a hammer method first during the explore phase and correctly classified 71.70% of cases. Sensitivity was 93.50%, specificity was 26.70%, positive predictive value was 72.50% and negative predictive value was 66.67%. None of the predictor variables were statistically significant. The results of this analysis appear in Table 1.

Table 1. Binomial logistic regression predicting hammer method at explore

	B	SE	Wald	*df*	*p*	Odds Ratio	95% CI for Odds Ratio	
							Lower	Upper
Explore Tool	.486	.719	.456	1	.500	1.626	.397	6.659
Sex	-1.188	.792	2.247	1	.134	.305	.064	1.441
Age	-.003	.015	.028	1	.868	.998	.969	1.027
Surgency	-.939	.624	2.260	1	.133	.391	.115	1.330
Negative Affect	.165	.543	.093	1	.761	1.180	.407	3.422
Effortful Control	.233	.617	.142	1	.706	1.262	.376	4.232
Experience	.025	.022	1.327	1	.249	1.026	.982	1.071
Constant	4.781	10.562	.205	1	.651	119.245		

Effectiveness at explore. A binomial logistic regression was performed to ascertain the effect of explore tool, sex, age, the three ECBQ measures, prior experience with hammers, number of methods used at explore, and using a hammer method at explore on infant's effectiveness during the explore phase. There were two levels of the dependent variable of effectiveness at explore: Not effective and effective. Not effective was the reference category. The logistic regression model was statistically significant, $X^2(9) = 32.42$, $p < .001$. The model explained 71.50% (Nagelkerke R^2) of the variance in effectiveness during the explore phase and correctly classified 91.30% of cases. Sensitivity was 96.90%, specificity was 78.60%, positive predictive value was 91.18% and negative predictive value was 91.67%. Of the predictor variables, two variables were statistically significant: Prior experience with hammers and number of methods used at explore. Increasing prior experience with hammers and increasing the number of methods tried were each associated with an increased likelihood being effective during the explore phase. The complete results of this analysis appear in Table 2.

Table 2. Binomial logistic regression predicting effectiveness at explore

	B	SE	Wald	*df*	*p*	Odds Ratio	95% CI for Odds Ratio	
							Lower	Upper
Explore Tool	.050	1.220	.002	1	.967	1.052	.096	11.499
Sex	.908	1.371	.439	1	.508	2.480	.169	36.441
Age	-.019	.032	.360	1	.548	.981	.922	1.044
Surgency	-.064	1.213	.003	1	.958	.938	.087	10.108
Negative Affect	1.349	1.028	1.723	1	.189	3.852	.514	28.870
Effortful Control	-.829	.954	.755	1	.385	.437	.067	2.830
Experience	.103	.047	4.791	1	*.029	1.109	1.011	1.216
Explore Methods	4.700	1.934	5.909	1	*.015	109.982	2.486	4865.854
Explore Hammer	-.828	1.391	.355	1	.552	.437	.029	6.674
Constant	1.464	22.086	.004	1	.947	4.322		

Tool imitation at test. A binomial logistic regression was performed to ascertain the effect of demonstration tool, sex, age, the three ECBQ measures, prior experience with hammers, the number of methods used during the explore phase, using a hammering method during the explore phase, and effectiveness during the explore phase on infant's imitation of the experimenter's demonstrated tool at the test phase. There were two levels of the dependent variable: Did not imitate and imitated. Did not imitate was the reference category. The logistic regression model was marginally statistically significant, $X^2(10) = 18.11$, $p = .053$. The model explained 43.50% (Nagelkerke R^2) of the variance in tool imitation at test and correctly classified 71.70% of cases. Sensitivity was 68.00%, specificity was 76.20%, positive predictive value was 77.27% and negative predictive value was 66.67%. Of the predictor variables, only number of methods used at explore was statistically significant. Increasing the number of methods used during the explore phase was associated with an increased likelihood of imitating the experimenter's demonstrated tool at test. The complete results of this analysis appear in Table 3.

Table 3. Binomial logistic regression predicting tool imitation at test

	B	SE	Wald	*df*	*p*	Odds Ratio	95% CI for Odds Ratio	
							Lower	Upper
Demo Tool	.634	.823	.594	1	.441	1.885	.376	9.451
Sex	-1.469	.890	2.723	1	.099	.230	.040	1.318
Age	.010	.017	.328	1	.567	1.010	.977	1.043
Surgency	-.749	.669	1.258	1	.262	.473	.128	1.751
Negative Affect	1.472	.791	3.467	1	.063	4.360	.925	20.537
Effortful Control	1.519	.840	3.275	1	.070	4.569	.881	23.679
Experience	.023	.027	.701	1	.402	1.023	.970	1.080
Explore Methods	1.843	.827	4.972	1	*.026	6.316	1.250	31.920
Explore Hammer	-1.608	1.196	1.809	1	.179	.200	.019	2.086
Explore Effect.	-1.524	1.276	1.427	1	.232	.218	.018	2.655
Constant	-14.051	11.905	1.393	1	.238	.000		

Method imitation at test. A binomial logistic regression was performed to ascertain the effect of demonstration tool, sex, age, the three ECBQ measures, prior experience with hammers, the number of methods used during the explore phase, using a hammering method during the explore phase, and effectiveness during the explore phase on infant's imitation of the experimenter's demonstrated method (i.e., hammering) at the test phase. There were two levels of the dependent variable: Did not imitate and imitated. Did not imitate was the reference category. The logistic regression model was statistically significant, $X^2(10) = 19.57$, $p < .05$. The model explained 46.70% (Nagelkerke R^2) of the variance in method imitation at test and correctly classified 76.10% of cases. Sensitivity was 81.50%, specificity was 68.40%, positive predictive value was 78.57% and negative predictive value was 72.22%. Of the predictor variables, two were statistically significant: Using a hammer method during the explore phase and the number of methods used during the explore phase. Infants who used a hammer method during the explore phase were 24.18 times more likely to imitate the demonstrated method at test, while increasing the number of methods used during the explore phase was associated with a slight decrease in the likelihood that infants imitated the demonstrated method at test. The complete results of this analysis appear in Table 4.

Table 4. Binomial logistic regression predicting method imitation at test

	B	SE	Wald	*df*	*p*	Odds Ratio	95% CI for Odds Ratio	
							Lower	Upper
Demo Tool	-.296	.821	.130	1	.718	.743	.149	3.717
Sex	.073	.955	.006	1	.939	1.075	.165	6.995
Age	-.010	.017	.301	1	.583	.990	.957	1.025
Surgency	.762	.683	1.246	1	.264	2.143	.562	8.171
Negative Affect	-.093	.707	.017	1	.895	.911	.228	3.644
Effortful Control	1.793	1.065	2.834	1	.092	6.007	.745	48.440
Experience	.001	.024	.001	1	.974	1.001	.954	1.050
Explore Methods	-2.195	.843	6.785	1	*.009	.111	.021	.581
Explore Hammer	3.185	1.238	6.621	1	*.010	24.175	2.136	273.572
Explore Effect.	2.117	1.197	3.127	1	.077	8.310	.795	86.873
Constant	-5.721	11.121	.265	1	.607	.003		

Preferred tool at test. A binomial logistic regression was performed to ascertain the effect of demonstration tool, sex, age, the three ECBQ measures, prior experience with hammers, the number of methods used during the explore phase, using a hammering method during the explore phase, and effectiveness during the explore phase on infant's preferred tool during the test phase. There were two levels of the dependent variable: Preferred hard tool and preferred soft tool. (As mentioned previously, 1 infant did not prefer a tool at test, and was excluded from this analysis.) Preferred hard tool was the reference category. The logistic regression model was statistically significant, $X^2(10) = 26.75$, $p < .01$. The model explained 59.80% (Nagelkerke R^2) of the variance in preferring the hard tool at test and correctly classified 79.10% of cases. Sensitivity was 78.30%, specificity was 77.30%, positive predictive value was 78.26% and negative predictive value was 77.27%. Of the predictor variables, three were statistically significant: Demonstration tool, sex, and effectiveness at explore. Infants who saw the hard tool during the demonstration phase (and by extension used the soft tool during the exploration phase) were .09 times less likely to prefer the soft tool over the hard tool, while infants who were male were .04 times less likely to prefer the soft tool. Infants who were effective during the explore phase were .01 times less likely to prefer the soft tool. The complete results of this analysis appear in Table 5.

Table 5. Binomial logistic regression predicting preferred tool at test

	B	SE	Wald	*df*	*p*	Odds Ratio	95% CI for Odds Ratio	
							Lower	Upper
Demo Tool	-2.332	1.163	4.022	1	*.045	.097	.010	.949
Sex	-3.132	1.148	7.441	1	*.006	.044	.005	.414
Age	.021	.019	1.175	1	.278	1.021	.983	1.061
Surgency	.834	.847	.969	1	.325	2.303	.438	12.115
Negative Affect	1.221	.903	1.829	1	.176	3.390	.578	19.889
Effortful Control	-2.129	1.167	3.330	1	.068	.119	.012	1.171
Experience	.033	.031	1.115	1	.291	1.033	.972	1.098
Explore Methods	.603	.837	.519	1	.471	1.827	.354	9.420
Explore Hammer	2.163	1.454	2.211	1	.137	8.694	.503	150.402
Explore Effect.	-4.473	2.007	4.969	1	*.026	.011	.000	.583
Constant	-10.267	13.182	.607	1	.436	.000		

Effectiveness at test. A binomial logistic regression was performed to ascertain the effect of demonstration tool, sex, age, the three ECBQ measures, prior experience with hammers, the number of methods used during the explore phase, using a hammering method during the explore phase, and effectiveness during the explore phase on infant's effectiveness during the test phase. There were two levels of the dependent variable: Not effective and effective. Not effective was the reference category. The logistic regression model was not statistically significant, $X^2(10) = 15.34$, $p = .120$. The model explained 45.20% (Nagelkerke R^2) of the variance in effectiveness during the test phase and correctly classified 87.00% of cases. Sensitivity was 97.30%, specificity was 44.40%, positive predictive value was 87.80% and negative predictive value was 80.00%. Effectiveness during the explore phase was marginally significant; infants who had been effective during the explore phase were 35.48 times more likely to be effective during the test phase. The complete results of this analysis appear in Table 6.

Table 6. Binomial logistic regression predicting effectiveness at test

	B	SE	Wald	*df*	*p*	Odds Ratio	95% CI for Odds Ratio	
							Lower	Upper
Demo Tool	2.231	1.299	2.949	1	.086	9.312	.729	118.868
Sex	.731	1.083	.456	1	.500	2.077	.249	17.330
Age	.008	.023	.124	1	.724	1.008	.964	1.054
Surgency	-1.236	1.348	.840	1	.359	.291	.021	4.082
Negative Affect	-1.897	1.273	2.221	1	.136	.150	.012	1.818
Effortful Control	2.110	1.367	2.382	1	.123	8.250	.566	120.313
Experience	-.027	.036	.565	1	.452	.973	.907	1.044
Explore Methods	-2.723	2.149	1.606	1	.205	.066	.001	4.431
Explore Hammer	2.104	1.631	1.665	1	.197	8.200	.335	200.444
Explore Effect.	3.569	1.878	3.613	1	.057	35.483	.895	1406.917
Constant	-2.160	17.560	.015	1	.902	.115		

CHAPTER 4

Discussion

This study sought to reveal if and how infants in the second year of life weigh information about tool use gleaned from exploration and observation to make strong decisions about which tool to use on given task. I undertook this question in order to resolve the conflicting evidence on this point from prior research (DiYanni & Kelemen, 2008; Pinkham & Jaswal, 2011), and attempted to resolve the methodological differences in those studies by instituting a general exploration phase, manipulating effectiveness during that phase by giving infants different tools, and providing a single demonstration that contrasted with infant's exploratory experience following exploration. I predicted that infants who had used a soft tool to explore the task, a tool with which they were expected to fail to discover an effective method, would imitate the experimenter's demonstration of a hard tool by choosing that tool at test. Conversely, I predicted that infants who had used a hard tool to explore the task, with which they were expected to succeed, would fail to imitate the experimenter's demonstration of a soft tool and eschew that soft tool at test.

While this study has answered my question, it has not done so in the manner predicted. In designing this study, I focused my attention on variability in the properties of tools rather than on variability in tool use methods. The results of this study demonstrate clearly that both forms of variability are important predictors of the likelihood that infants will use a particular tool over another. While these infants did not imitate tool choices or methods indiscriminately, the reasoning behind their selections appear much more subtle and complex than mere effectiveness with the standard hammering method I had framed my hypotheses around.

The explore results demonstrate that contrary to my predictions, there was not a significant difference in effectiveness due to the tool infants were given, nor did using a hammer method during the explore phase predict effectiveness in the same phase. Rather, effectiveness was predicted by prior experience with hammers outside the lab, not simply with using a hammering method. Looking at the raw data directly illustrates this vividly: Although 23 of the 27 infants who were effective during the explore phase used a hammering method, only about half of them used a hammering method alone. The other half went on to push the peg with the tool head, use their hand, or in one case the tool handle as well.

This finding, especially taken with the failure to find significant predictors of whether infants would use a hammer method during the explore phase, suggests that prior experience with hammers does not only or even primarily increase infants' preference for hammering as a method. Instead, infants with more experience try more methods on the task. This may be because they better understand the goal of the task than infants with less experience, because they have had more chances to discover methods than infants with less experience, or both. Interestingly, infants in this study did not appear to use more methods because they had not been effective with their first method. Of the 16 infants who used multiple methods, exactly half of them had already been effective with their first method before trying another one. This somewhat perplexing pattern could be interpreted as a lack of interest in the task or lack of intention to accomplish it. From this perspective, infants who used multiple methods were acting at random, trying various actions with no regard for the goal or effectiveness. However, as these infants repeatedly directed behaviors at the peg it seems more probable that they were actually very interested in completing the task and were simply trying to complete it more rapidly. Although these multi-method infants' first actions were often effective at lowering the peg, they did not always move the peg down all the way. Therefore, their use of other alternative methods demonstrates a commitment to the task rather than disinterest: Babies were trying anything they could think of to move the peg.

The test findings also emphasize the fact that infants were using a number of different methods and are not confined to hammering alone. The method imitation findings, for example, show that infants who had used a hammer method during the explore phase were highly likely to use that method again immediately after seeing it demonstrated by the experimenter. However, infants who had tried many methods during the explore phase were less likely to use a hammering method immediately at test. These predictors suggest that infants who used hammering first at test were not blindly copying the experimenter's demonstration. Instead, those who already had a predilection for that method were likely to continue using it, while those who favored a broad approach and tried multiple methods on their own were unlikely to suddenly drop that approach and simply copy the experimenter. The main point here is that infants seemed to approach the task in a particular way on their own – either favoring the conventional hammering method or trying out a variety of methods – and stuck to that regardless of the experimenter's choice of method.

Although exploring many methods reduced the likelihood that infants would imitate the experimenter's choice of method, it actually increased the likelihood that they would imitate her choice of tool. This finding is intriguing in two ways. First, it suggests that making choices about tool use methods and making choices about tools are two different things, and shows that the same exploratory experience may lead to imitation of one and not the other. Second, this rounds out the profile of a generally enthusiastic explorer: An infant who is open to trying new methods and not fixated on a single one, and so is also open to trying a tool they have not explored before rather than sticking to an old familiar tool. Together, the imitation findings do not support the notion that infants imitate or do not imitate based on their reasoning about their own and others' effectiveness per se. Instead, it seems that their decisions are driven more by their own problem-solving style. Infants who try many methods will continue to do so despite seeing a social-pedagogical demonstration focused on a single method and tool, although they are eager to try that new tool as well if they get the chance. Infants who know and use a standard method are likely to continue using it after observing it demonstrated, even if that demonstration was ineffective, perhaps because the notion that this is the conventional method is reinforced by the mere fact of the demonstration.

While these findings seem to portray infants as oblivious to or unconcerned with their own or others' effectiveness with a tool, the tools that infants ultimately used preferentially over the course of the testing phase paint a more nuanced picture. Here, we see that effectiveness matters. Infants who had effectively accomplished the task on their own while exploring and those who had seen an effective demonstration of the hard tool were less likely to prefer the generally suboptimal soft tool. In this way, infants did exemplify a shadow of the hypothesized pattern of behavior. Infants were capable of using information either from their own exploration or from a social demonstration to guide their tool use preferences and came to a similar conclusion when using either source of information, but this effect was only perceivable over the long haul of problem-solving, not if we confined our view to infants' first actions and choices following the demonstration. Future studies on imitation and tool use would do well to include additional measures looking at what children do with tools over time, not just what they do right away. This idea will be revisited again in the forthcoming discussion of the effectiveness at test analysis, but first I will examine the unexpected significant effect of sex in predicting infants' tool preference at test.

There are two related explanations for the effect of sex on tool preference at test that deserve consideration. One experimenter's casual report of infants' affect during data collection hints that boys may have preferred the hard tool rather than the soft tool because it was a noisier toy, not necessarily because they recognized that it was a better tool for the task. Another, not mutually exclusive explanation might be that male infants preferred the hard tool because they better understood its appropriateness for the task than did females. This second explanation gains some credence when we compare the male and female infants' prior experience with hammers. The questionnaire data shows that males scored about 10 points higher than females did on average (66.40% versus 53.05%), and in particular scored more highly on questions relating to infants' experience using hammers to hit things (57.04% of points in that category versus 36.43%). As gendered play patterns have been demonstrated in children this age and even younger (Cherney, Kelly-Vance, Glover, Ruane & Ryalls, 2003; Lindsay, Mize & Pettit, 1997), particularly in regards to noisy or rough play (Lindsey & Mize, 2001; Morrongiello & Dawber, 1999), this effect may be attributable to larger differences in tool use play occurring outside the lab. For example, male children in general may be given loud, physical toys such as hammers and pegboard toys more often than are female children, males may be encouraged to hit things with hammers more often than are females, etc., a pattern that could lead both to differences in preferences for such toys as well as differences in how well boys and girls are able to discriminate between effective and ineffective tools.

Finally, I turn my attention toward the effectiveness at test analysis, which found no significant predictors despite the fact that the effectiveness at explore analysis found a significant association between exploring a large number of methods and being effective. To understand this lack of effect, it is important to recognize two facts. First, more infants in general were effective at test compared to explore. As mentioned previously, while 58.70% of infants were effective at explore, 80.43% were effective at test. Second, the test phase lasted for as long as infants continued to interact with the objects rather than the short 30 seconds that constrained the explore phase. Together, these facts suggest that more or less all infants who were ever going to be effective at test were effective. Looking more closely at the data illustrates this clearly. In the explore phase, effective infants used more methods on average compared to their ineffective counterparts (M = 1.6 methods compared to M = 0.6 methods, respectively) and somewhat unsurprisingly also spent more time on the task (M = 5.1 seconds compared to M = 1.5 seconds, respectively). While the difference in time spent on task between effective and ineffective infants remained at test (M = 9.4 seconds compared to M = 5.6 seconds, respectively), the difference in number of methods tried during that phase all but disappeared (M = 2.1 methods compared to M = 1.8 methods, respectively). This means that the babies who were ineffective at test were not trying fewer methods than their effective counterparts, they just weren't trying them for as long. This suggests that it was a lack of persistence at test that led to a lack of effectiveness at test, but persistence was not directly accounted for in any variable in the effectiveness analysis and so remained hidden.

In sum, the findings of this dissertation have shown that infants who are most likely to make strong, effective decisions about tool use are those who are willing not just to try new methods but also to pursue them to completion. But what is the origin of this combination of creativity and tenacity, what are its long-term outcomes, and can it be fostered in those who lack it if we so desire? These are questions that remain to be answered in future research, although there are hints of how we might proceed. First, intermittent longitudinal testing of increasingly challenging tool use problems could go far in separating the individual contributions of both exploring broadly and with persistence. Second, it is intriguing that the temperament measures, particularly surgency and effortful control, never played a significant role in predicting any of the effects that were predicted by exploring multiple methods. It is worth noting that in this particular sample the range and standard deviation of the temperament scores were fairly small, and it is possible that finding a broader sample along these ranges would lead to significant effects. It might also be that these measures are simply not meaningful in and of themselves, and that whatever relation they have to broad, persistent exploration is moderated by contextual factors that either did not exist or were not fully differentiated in this experiment. The fact that some of the temperament measures were marginally significant in predicting behaviors at test suggest that this might be the case: Infants who scored high on negative affect or on effortful control were marginally more likely to imitate the experimenter's tool at test ($p = .063$ and $p =.070$, respectively), while infants who scored high on effortful control were also marginally more likely to prefer the hard tool at test ($p = .068$). Perhaps if infants were frustrated by a challenging tool use problem or lulled by an easy one, or if their inhibition and self-control were boosted or hampered by changes in the research design we could better see the connections between temperament and tool use behavior that remain obscure in this study.

Besides indicating these other fruitful lines of further inquiry, this dissertation offers one other contribution to the research literature on the development of tool use. Namely it demonstrates the absolute necessity of including both discrete measures of infants' initial tool and tool use method choices as well as continuous measures of infants' changing tool preferences and methods over time. This is particularly crucial if we are to fully understand the intricacies of children's thinking about both tools versus tool use methods, which as this dissertation shows are related but distinct. Prior tool use studies with infants, children, and even adults and non-human primates have tacitly adhered to a distinction between examining tools and tool use methods, as researchers tend to look at either decisions about tools when methods are constrained (e.g., Brown, 1990; Buttelman, Carpenter, Call & Tomasello, 2008; DiYanni & Kelemen, 2008) or methods when the tool is constrained (e.g., Connolly & Dalgleish, 1989; Bongers, Smitsman & Michaels, 2004; McGuigan, Makinson & Whiten, 2011; Want & Harris, 2001). It is likely that this occurs in part because of the pragmatic challenge of manipulating and interpreting both factors in a research design, and the fact that researchers tend to be primarily interested in action planning and kinetic aspects of tool use or on social and cultural aspects of tool use but rarely both.

Although the contributions of these studies to our current understanding of tool use are inestimably valuable, this incidental split in taste and methodology has led to unfortunate gaps in our knowledge. On the one hand we have researchers who elegantly describe the development of general tool use skills over a long period of time (e.g., Lockman, 2000) but sadly give little information about how infants react to specific goal-directed tasks or reason in the moment about tool use challenges. On the other hand we have researchers who can almost uncannily predict infants' behavior in a single snapshot of problem-solving time (e.g., DiYanni & Kelemen, 2008), but unfortunately omit any details on infants' pattern of behavior following that moment or across developmental time in general. If we are to progress further in our understanding of this foundational life skill, it is time these paths merged into one. I hope this dissertation will stand as one step forward along that new, united path.

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