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The Child as an Active Learner

By

Zi Lin Sim

A dissertation submitted in partial satisfaction of the requirements for the degree of Doctor of Philosophy in Psychology in the Graduate Division of the University of California, Berkeley

Committee in charge:

Professor Fei Xu, Chair
Professor Alison Gopnik
Professor Thomas L. Griffiths
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Summer 2016
The Child as an Active Learner

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Abstract

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Doctor of Philosophy in Psychology

University of California, Berkeley

Professor Fei Xu, Chair

In the study of early childhood development, the “active child” is an enduring theme that has inspired, motivated, and puzzled developmental psychologists over the decades. Despite a large body of evidence demonstrating that young children can allocate their attention and exploration in a non-random manner, we still do not have clear answers for how being active influences children’s development. How does the “active child” fit into the story of development? It is often thought that activity is relevant to development because it contributes to learning. This idea is consistent with the perspective that children may be active learners. However, to date, there is very little empirical work showing that young children can successfully acquire specific pieces of knowledge through the voluntary allocation of attention, self-driven exploration/free play, or question-asking. At the same time, the definition of active learning itself has only grown more divergent. It has been taken to mean physical activity while engaging in a task, a learning method that emphasizes higher-order thinking, elaborative cognitive processes such as generating explanations, interactive or collaborative learning, etc.

The current dissertation seeks to understand whether learning can successfully occur through children’s own actions, as well as the processes that support such learning. Our work suggests that active learning is not a single unitary process—instead, it may be better described as multiple processes working in concert so that learning can actually occur. First, learners will need to detect situations when there is something to be learnt. Chapter 2 presents a series of experiments demonstrating that the looking times of 8-month-old infants appear to be driven by the evidence an observed event provides for a set of alternative hypotheses over the currently favored hypothesis. In other words, young infants can successfully detect situations where their current understanding is inaccurate or incomplete, and there may be a better explanation for the events that they observed. Second, learners will need to selectively attend to or approach potential sources of information. Chapter 3 provides evidence that 13-month-old infants preferentially approach and explore sources of unexpected events, which are great opportunities for obtaining information relevant for theory revision. Third, learners would have to generate evidence that is relevant to the learning goal. Chapter 4 presents empirical findings that
preschool children can systematically generate data to learn about categories, and the systematicity of their information search strategy is correlated with their classification performance. Chapter 5 demonstrates that preschool children judge the effectiveness of presented questions in a way that is consistent with maximizing information gain, suggesting that the computational foundations for developing effective information search strategies may be in place by an early age. Finally, learners would have to actually learn from the self-generated data, incorporating the observed outcomes into one’s knowledge. Chapter 6 shows that children as young as 2-years-old can acquire higher-order generalizations based on self-generated evidence through the course of free play.

Taken together, the experiments presented in this dissertation demonstrate that children are active learners, and the component processes that support active learning is present by early childhood. However, these processes may emerge at different time points during development, and the capacities also continue to develop, enabling children to become more adept at active learning over time.
To my parents
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I am reserved when it comes to expressing emotions, so please know that the words above belie the enormity of my gratitude to everyone acknowledged.
Chapter 1

Introduction

In the final accounting, what is the significance of exploratory activity and its perceptual consequences? May it not be the essential ingredient for building a foundation of knowledge about the world? (Gibson, 1988, p. 34)

In the study of early childhood development, the “active child” is an enduring theme that has inspired, motivated, and puzzled developmental psychologists over the decades. It is discussed in any developmental psychology textbook that one may casually pick up, being highlighted as a central idea of development, on par with other themes such as “nature vs. nurture” or “continuity vs. discontinuity of development.” This emphasis is not unwarranted—it is undeniable that children are incredibly active young humans. They draw on our walls, they play with their food, and they get into our phones and computers; they seem to be really testing out the boundaries of their environment.

From very early on, there is clear evidence that the activity that children engage in is non-random. For one, they make decisions about what to pay attention to from an early age. Newborns prefer to look at things that move and make sounds (Gibson, 1988), and their attention is particularly drawn to faces, especially when it is the face of their own mother (Bartrip, Morton, & Schonen, 2001). In more recent work, researchers have also found that infants allocate their visual attention in a rational manner: they prefer to look at visual sequences that are neither too simple nor too complex (Kidd, Piantadosi, & Aslin, 2012; Piantadosi, Kidd, & Aslin, 2014), and they devote more time to aspects of the environment that are learnable rather than unlearnable (Gerken, Balcomb, & Minton, 2011). Such voluntary selection of attention extends to manual exploration as well; there is some evidence showing that within the first year of life, infants are already capable of exploring objects and surfaces in a way that may allow them to discover their properties (Adolph, Eppler, & Gibson, 1993; Fontenelle, Alexander Kahrs, Ashley Neal, Taylor Newton, & Lockman, 2007; Hauf, Paulus, & Baillargeon, 2012). Along the same vein, Stahl and Feigenson (2015) recently demonstrated that 11-month-old infants engage in behaviors that are specifically related to a surprising event that they just observed. For example, infants spend more time banging rather than dropping a violation object after observing a solidity violation, but this pattern reverses if they observed a support violation instead: infants now spend more time dropping rather than banging the violation object.
Other researchers have demonstrated selective exploration in preschoolers as well. Results from Schulz and Bonawitz (2007) indicated that 5-year-olds prefer to play with a toy that produced ambiguous evidence for its causal structure as compared to a novel toy, and Cook, Goodman, and Schulz (2011) found that preschoolers were able to spontaneously select and design actions to disambiguate confounded evidence.

With the acquisition of language, older children begin to engage in another form of exploratory activity: asking questions. Children ask a large number of information-seeking questions – about 76 – 95 per hour! These information-seeking questions (“Why is the baby crying?”) constitute the majority of question that children ask, and children ask significantly more of these questions over non-information seeking questions, such as clarification (“What did you say?”) or permission (“Can I have an apple?”) questions (Chouinard, Harris, & Maratsos, 2007). They are also motivated to get the answers of the questions that they ask as well. Frazier, Gelman, and Wellman (2009) demonstrated that children’s questions are executed with the purpose of obtaining causal explanatory information. Children react very differently to adult responses to their questions (e.g., “Why did he laugh?”). When they are provided with an explanatory response (“Someone told him a funny joke”), children will agree or ask follow-up questions; but when they are provided with a non-explanatory response (“I don’t know”), children have the tendency to re-ask the original question (“But what made him laugh?”) or to provide their own explanation (“I think someone tickled him”). This pattern of results was found for both naturalistic situations, in which the researchers combed the CHILDES database for questions-response segments, as well as experimental situations, in which experimenters carefully controlled the responses that children received. Therefore, children appear to use specific conversational strategies to obtain the information that they desire.

Despite the large body of evidence demonstrating that young children can allocate their attention and exploration in a non-random manner, we still do not have clear answers for how being active impacts, shapes and contributes to children’s development. How does the “active child” fit into the story of development? The answer to this question seems readily apparent: exploratory activity is relevant to development, because it contributes to learning. This idea is consistent with the perspective that children may be “active learners” (see Piaget, 1955 for a discussion). However, to date, there is very little empirical work showing that young children can successfully acquire specific pieces of knowledge through the voluntary allocation of attention, self-driven exploration/free play, or question-asking. Without such evidence, the relevance of exploratory activity to cognitive development remains strictly theoretical. We may like to think that the systematic exploration that young children engage in have real consequences on their learning, but there is, in fact, little evidence to support this claim.

Further muddying the waters is the lack of consensus as to what “active learning” means. Researchers from many different fields have been interested in the idea of active learning, as understanding this aspect of development can have important implications on education, cognitive psychology, and artificial intelligence (among others). But as research on this topic accumulates, the definition of “active learning” has only grown more divergent (Chi, 2009). Within psychology and education, it has been taken to mean physical activity while engaging in a task, selective attention, and elaborative cognitive processes such as generating explanations, organizing information and making predictions. It has also been used to refer to interactive or
collaborative learning, or a learning method that emphasizes higher-order thinking, etc. (see Gureckis & Markant, 2012 for an overview). In fact, it appears at times that the best definition of active learning is everything other than passive learning, where children or learners are specifically viewed as merely passive recipients of information.

Part of the issue is that there are many dimensions along which we could construe active learning, e.g., a young infant selectively attends to parts of her environment, a toddler crawls to get to some toys but not others, a young child asks inquisitive questions about what happens after death, and a high-school student struggles to make a chemistry experiment work for a science fair submission. Active learning does not appear to be just one single unitary process—instead, it may be better described as multiple processes working in concert so that learning can actually occur.

This dissertation is focused on the component processes that enable active learning to occur in early childhood, and it presents a body of new empirical evidence aimed at exploring the following questions: can young children influence their own learning outcomes? How systematic are children in interacting and exploring their world? Are formal quantitative measures and computational models useful in capturing children’s choices and data generation? Do these interactions result in any learning? Are there instances when engaging in such independent exploration may result in better learning when compared to being directly provided with data? In this work, my collaborators and I tested children of different age groups, explored different domains of knowledge, and utilized different experimental methods to examine active learning in young children.

Through this work, we identified four components that contribute to active learning in early childhood. First, learners need to detect that there is something to be learnt. Second, they will need to attend to or approach the potential source of information. Third, learners would have to generate evidence that is relevant to the learning goal. And finally, they would have to actually learn from the self-generated data, incorporating the observed outcomes into one’s knowledge. The experiments presented in this dissertation demonstrate that these components are present by early childhood, but may emerge at different time points during development. These capacities also continue to develop, enabling children to become more adept at active learning over time.

1.1 Guiding Framework: Rational Constructivism

In this section, I briefly review the theoretical framework that inspires and guides much of the work presented in the dissertation: rational constructivism (e.g., Gopnik & Wellman, 2012; Xu & Kushnir, 2012, 2013). This perspective of cognitive development make several claims. First, human learning can be characterized as rational Bayesian inference. By this account, a learner begins with a prior probability distribution over a set of hypotheses. Upon observing some evidence, she updates her beliefs by computing the posterior probability of each hypothesis using Bayes rule: 

\[ p(\text{hypothesis}|\text{data}) = \frac{p(\text{hypothesis}) \times p(\text{data}|\text{hypothesis})}{p(\text{data})} \]

where \( p(\text{hypothesis}) \) is the prior probability for the hypothesis under consideration, \( p(\text{data}|\text{hypothesis}) \) is the likelihood of observing the data given that hypothesis, and \( p(\text{data}) \) is
the probability of observing that data given all hypotheses. It is important to note that such an account provides an explanation of learning at the computational level – it focuses on the logic of how learning occurs, rather than the specific cognitive processes involved.

Second, hypotheses are associated with probabilities, such that inferences are graded in nature. Rather than simply ruling in or ruling out hypotheses, learners are more or less confident about the different hypotheses. For example, Xu and Tenenbaum (2007) demonstrated that when preschoolers are shown three Dalmatians as exemplars of a novel word “fep,” they would generalize the new word at the subordinate level, i.e., only to other instances of Dalmatians, even though the evidence would be equally consistent with generalizing the novel word at the basic level, i.e., to other instances of dogs in general (Xu & Tenenbaum, 2007). In this case, the learner has more confidence in the hypothesis that “fep” generalizes to the subordinate level than in the hypothesis that “fep” generalizes to the basic level, because it would be odd that the first three examples selected by a teacher were all Dalmatians if the latter were to be true. The current characterization of learning as rational Bayesian inference allows for such graded beliefs, while a hypothesis elimination approach does not.

Third, much of early learning may be characterized as inductive learning, i.e. making principled and meaningful generalizations based on limited amounts of data. Research has repeatedly demonstrated that young children engage in such learning proficiently: They learn the meanings of some words with just a single labeled exemplar (Carey & Bartlett, 1978); they generalize non-obvious properties to novel objects after just a short demonstration (Baldwin, Markman, & Melartin, 1993; Gelman & Davidson, 2013; Welder & Graham, 2006), and they learn the physical rules of occlusion with just a single trial (Wang & Baillargeon, 2005). Other domains of knowledge in which young children also show such sophisticated inductive inferences include language (Chomsky, 1980), causality (Gopnik & Sobel, 2000; Gopnik, Glymour, Sobel, Schulz, et al., 2004), and biological kinds (Gelman & Wellman, 1991). In addition, young learners often make generalizations at multiple levels of abstraction, which is even more important for building large conceptual structures. Not only do they make first-order generalizations (e.g. dogs like to eat bones; rabbits like to eat vegetables), but they also make sophisticated second, third, or fourth-order generalization (e.g. each kind of animal has a favored food; each kind of animal has its own unique traits).

This view of early learning approaches the issue of the origins of inductive constraints and biases from the perspective that early input provides the basis for developing such constraints, and subsequent learning is guided and/or constrained by these learned constraints. Computational cognitive scientists have developed formal models, in particular Bayesian models that capture the idea of learning to learn (Kemp, Perfors, & Tenenbaum, 2007; Perfors, Tenenbaum, Griffiths, & Xu, 2011; Tenenbaum, Kemp, Griffiths, & Goodman, 2011) across a variety of domains, from causal learning to categorization to word learning, and from whole grammars to intuitive theories (Griffiths & Tenenbaum, 2009; Tenenbaum, Griffiths, & Niyogi, 2007). There is some evidence for such a capacity early on in development: Looking-time experiments with 9-month-old infants indicate that they can form second-order generalizations such as “boxes contain objects that are uniform in color” (Dewar & Xu, 2010); and Macario, Shipley, and Billman, (1990) showed that 4-year-old children could rapidly construct higher-
order generalizations about how objects were being categorized, successfully classifying new exemplars into novel categories by shape or color.

Fourth, learners have domain-general learning mechanisms that allow them to acquire domain-specific knowledge. A myriad of experiments on this topic have produced ample evidence demonstrating that children have powerful domain-general learning mechanisms that allows them to keep track of complex statistics in their input (e.g. Aslin, Saffran & Newport, 1998; Gopnik et al., 2004; Kirkham, Johnson & Slemmer, 2002; Saffran, Aslin & Newport, 1996; Xu & Garcia, 2008; among many others). This sophisticated input processing enables children to acquire new concepts and biases that are domain-specific. For example, after infants had used the statistics in a speech stream to carry out word segmentation, they attached these newly segmented words to objects (Graf Estes, Evans, Alibali & Saffran, 2007). Infants and young children also attribute specific references to an agent after observing nonrandom sampling (Kushnir, Xu, & Wellman, 2010; Wellman, Kushnir, Xu, & Brink, 2016).

Fifth, learners are actively engaged in their own learning process, beginning as early as infancy. This claim has been much less studied to date. In this claim, it is argued that children can sometimes generate the relevant and necessary evidence themselves, even in the absence of explicit instructions or demonstrations. This dissertation, in essence, explores this specific area, seeking to understand how learning occurs through children’s own actions, and the processes that support such learning.

1.2 Précis

The remainder of this document is divided into 6 chapters. The following five chapters report experimental findings related to the processes that support active learning in early childhood, using multiple methods and multiple age groups. In the final chapter, I discuss the implications of this work and suggest future directions for this area.

In Chapter 2, I discuss our work showing that 8-month-old infants can detect situations when there is more learning that is necessary. Using a combination of behavioral experiments and computational modeling, we show that infants’ looking times reflect inferential reasoning processes that approximate an attempt to evaluate observed events according to how well they support different hypotheses, enabling them to take the first steps towards building and revising intuitive theories in the face of unexpected evidence (Carey, 1985, 2009; Goodman et al., 2006; Gopnik & Meltzoff, 1997; Legare, Gelman, & Wellman, 2010). These findings open up the possibility that young infants consider multiple hypotheses for observed events, and that they are well-prepared and motivated to learn the true structure of the world.

Chapter 3 presents work indicating that infants actually act on these potential learning opportunities. We find that infants’ exploration behaviors may be biased for learning – they preferentially approach and explore sources of surprising events.

Chapter 4 investigates whether young children can generate data in systematic ways when they are allowed to make decisions about the data they wish to experience in order to accomplish some learning goal. Our results indicate that the information gathering demonstrated by 5- and 7-year-olds was systematically driven by uncertainty, and that the children show
superior learning under conditions of selection over yoked reception in a category-learning task, even though the two groups of children observed identical sequences of data.

In Chapter 5, we explore the underlying computational foundations that potentially supports children in generating the data necessary to support their own learning. We do so by examining one prototypical active learning behavior: question-asking, investigating whether 5-year-old children are already able to successfully identify the most effective among given questions, as measured by information gain.

Chapter 6 demonstrates that preschoolers can successfully learn from evidence that they independently generate during the course of free play, and the accuracy of their learning does not differ from that of children who had been trained by the experimenter.
Chapter 2

Inferential reasoning by 8-month-old infants:
An empirical and computational investigation

2.1 Introduction

Decades of developmental research have capitalized on the fact that infants look longer at unexpected events. With the advent of looking time measures, researchers were handed an unprecedented tool to assess the state of knowledge within a preverbal infant. In almost all of such violation-of-expectation (VOE) studies (Baillargeon, Spelke, & Wasserman, 1985; Denison, Reed, & Xu, 2013; Onishi & Baillargeon, 2005; Spelke, Breinlinger, Macomber, & Jacobson, 1992; Woodward, 1998; Wynn, 1992), researchers present infants with an expected event that is highly probable and an unexpected event that is highly improbable or impossible. When infants react with “surprise” (i.e., look longer) at the unexpected event, researchers have assumed that the surprise response arises from the infants observing a low-to-zero probability event given their prior expectations. This assumption has been tremendously productive, enabling researchers to make inferences about infants’ early knowledge across different domains.

Despite the important role of looking time data in developmental psychology, drawing inferences about infants’ prior knowledge and their underlying cognitive processes purely based on looking time is not a straightforward task. As Aslin (2007) explains, looking time is a global measure with many different underlying cognitive processes: sometimes infants look longer as an attentional response to novelty, sometimes they look longer because of recognition or familiarity, and at other times they do so due to a search for discrepancy. In other words, it is often ambiguous what cognitive processes drive patterns of looking time.

One way that researchers can begin to probe this issue is to model looking times in a quantitative manner. However, there is a dearth of such methods in the infant looking time literature – to date, only Téglás et al. (2011), Kidd et al. (2012), and Piantadosi et al. (2014) have done so. In Téglás et al. (2011), 12-month-old infants were presented with dynamic displays showing three objects of one type (e.g., blue objects) and one object of another type (e.g., yellow objects) bouncing inside a container. After several seconds, the contents of the container were occluded for 0.04s, 1s, or 2s, after which infants saw that either one of the majority objects (i.e., a blue object) or the minority object (i.e., a yellow object) exited from the container. The researchers also examined the effect of distance, such that half of the time, the
object that later exited from the container was near the exit right before the occlusion occurred, and the other half of the time, the object that later exited was far away from the exit before the occlusion occurred. The combination of these three variables (occlusion duration, a majority/minority outcome, and the physical arrangement of the four objects right before occlusion) generated 12 different events, and the researchers collected looking times for each of these events.

Téglás and colleagues compared these looking times with the predictions of an ideal Bayesian learner computing $P(\text{outcome})$, which is the probability of observing a particular object type (i.e., a blue object or a yellow object) emerging first from the container as a function of the three mentioned variables. They found that the obtained looking times were systematically related to $1 - P(\text{outcome})$; the model explained 88% of the variance in infants’ average looking times for the 12 different events. In addition, the model accounted for classic looking time findings with infants in the domain of object cognition (Aguiar & Baillargeon, 1999; Kellman & Spelke, 1983; Xu & Carey, 1996). These findings supported the researchers’ central claim that 12-month-old infants can integrate numerosity, spatial and temporal cues to form rational expectations about novel events. At the same time, the findings also suggested that infant looking times may be driven by a computation of event probabilities. When the probability of observing a specific outcome is low, 12-month-old infants tended to look for a longer period of time.

Kidd et al. (2012) found in two experiments that 8-month-old infants were selective in their allocation of visual attention. In Experiment 1, infants viewed several different visual sequences where a single object would be revealed (or not) by an occluder that was moving up and down. An example sequence here might be of a toy truck being “present, present, absent, present, absent, etc.” behind the occluder. In Experiment 2, infants instead viewed visual sequences where one of three objects would pop up behind one of three distinctive boxes. An example sequence in this experiment might be “pretzel behind Box A, bottle behind Box C, pretzel behind Box A, cookie behind Box B, pretzel behind Box A, etc.” For each of these sequences, the researchers recorded the looking time before infants met the look-away criterion of an off-screen gaze for 1 consecutive second.

When these looking times were analyzed quantitatively, Kidd et al. (2012) found that the infants’ probability of looking away during a particular sequence was modulated by complexity, which was formalized as the negative log probability of observing a particular event given the distribution of previously observed events. More specifically, infants tended to look away when an observed event had very low complexity (i.e., very high in probability given previously observed events) or very high complexity (i.e., very low in probability given previously observed events), in a bid to avoid allocating cognitive resources to such events. The researchers also found that these findings held up within individual infants and were not an artifact of averaging infants with different types of behavior (Piantadosi et al., 2014).

Based on such evidence, the argument can be made that infant looking times may be driven by the detection of low-probability events (but not too low, as demonstrated in Kidd et al., 2012). These events are typically violations of our prior expectations. For example in a visual scene, after observing a pretzel pop up behind Box A four times, seeing the pretzel pop up once again behind Box A is highly expected, while seeing a cookie pop up behind Box C next instead
violates our expectation; our updated beliefs about the distribution of possible events would lead us to think that the probability of observing the latter event is quite low.

In a recent study, Stahl and Feigenson (2015) showed that experiencing such “surprise,” defined as longer looking in VOE experiments, may have important implications for learning in 11-month-old infants. In several experiments, they showed that after witnessing a violation of expectation, such as seeing a toy car pass through a solid wall, infants showed better learning of a hidden auditory property of the violation object, they were more interested in playing with the violation object as compared to a novel object, and they engaged in more exploratory behaviors that were directly related to the specific violation event that they observed, e.g. banging the violation object after observing a solidity violation, or dropping the violation object after observing a support violation.

Putting these studies together, a picture emerges that infant looking time, or “surprise,” is driven by the detection of low-to-zero probability events, and detecting such events may lead to new learning opportunities. However, this picture may not be quite complete. The cognitive process that drives infant looking times may actually be more sophisticated, in that infants may have the capacity to go beyond considering the mere probability of observed events given their prior expectations. For example, consider rolling a six-sided die four times. The sequence 2-1-4-3 occurs with a probability of .00077, and this sequence is hardly surprising. Consider now that the sequence 1-1-1-1 had occurred. Once again, this sequence occurs with a probability of .00077, but this time, we are surprised. Even though the two events have equal probabilities of occurring according to our current hypothesis (i.e., a fair die is being rolled), the latter sequence, and not the former, arouses our suspicions about the die. Therefore, events are considered surprising not simply because they have a low-to-zero probability of occurrence. Furthermore, our surprise reaction to observing the sequence 1-1-1-1 cannot be explained by its complexity—in fact, this sequence is highly predictable and therefore extremely low in its complexity, and yet we are more drawn towards such a sequence as compared to the sequence 2-1-4-3.

A natural response to this line of reasoning is that perhaps we are engaging in a different form of probability computation. Returning to the die example, the sequence 1-1-1-1 is surprising because it is of a kind that is less likely than the sequence 2-1-4-3 – seeing rolls that are all the same is less probable than seeing rolls that are all different. But the idea that we are surprised by events of an unlikely kind is problematic. It is possible to construct counter-examples: for instance, compare the sequences 1-1-1 and 2-5-4-3-6-1. The former seems more noteworthy, but the probability of getting the same number every time on three rolls (.028) is actually higher than the probability of getting different numbers every time on six rolls (.015). Being an unlikely kind of event is therefore not sufficient to account for adult intuitions about surprising events.

So what are human learners actually surprised by? We hypothesize that observed events are considered improbable when their occurrence is more consistent with a set of alternative hypotheses being true (e.g., the die is loaded; the die has the number 1 on all its faces) than that for the original hypothesis (e.g., the die is fair). This idea follows a Bayesian account of the sense of coincidence in adults (Griffiths & Tenenbaum, 2007) and is congruent with principles of Bayesian learning (Griffiths, Chater, Kemp, Perfors, & Tenenbaum, 2010; Tenenbaum & Griffiths, 2001; Tenenbaum et al., 2011): in this learning process, the ideal learner assesses the
fit between the observed evidence and her current highest-probability hypothesis, and compares it to the fit provided by a set of lower-probability hypotheses.

In this paper, we use behavioral experiments and computational modeling to show that infants’ surprise responses in looking time experiments are not well-predicted by the mere probabilities of events or kinds of events given their prior expectations. Instead, they look longer when observed events are better accounted for by an alternative hypothesis than the original hypothesis. Preverbal infants, like scientists, appear to be sensitive to the degree to which evidence supports a current hypothesis versus an alternative hypothesis, which is consistent with principles of Bayesian reasoning. Looking time may therefore reflect inferential reasoning on the infants’ part. Such a capacity will enable infants to get closer to figuring out the rules and regularities that govern their environment, and may constitute a first step towards building and revising intuitive theories in the face of unexpected evidence (Carey, 2009; Goodman et al., 2006; Gopnik & Meltzoff, 1997; Legare et al., 2010).

In what follows, we first formalize three accounts of infant looking times. We then present looking time experiments with 8-month-old infants that test the qualitative predictions of these models. We chose to study an abstract domain such that infants were presented with sequences of balls randomly tossed out of a box, in order to also carefully test the quantitative predictions of these different accounts, without having to be concerned with infants’ prior knowledge of particular domains. In our experiments, infants were familiarized to a box containing 6 different colored balls. Analogous to the die examples above, an experimenter tossed out different sequences of balls from the box (sampling with replacement, see Figure 2.1), and looking time for each sequence was measured. These looking times, together with some additional data, were then used to conduct a quantitative analysis of the different formal models. We close with a discussion of our results and their implications.

2.2 Models

To characterize the underlying processes that give rise to different levels of looking, we formalize three accounts using probabilistic models: 1) a mere probability model, 2) a probability of kinds model, and 3) a Bayesian inferential model. As the assumptions of each model are outlined explicitly, we can calculate the predictions made by these models and thus determine which model best accounts for infant looking times. For these models, \( h \) is a hypothesis, \( d \) is a specific sequence of balls observed, \( k \) is the number of uniquely colored balls in the large population box, and \( N \) is the number of independent draws from the box.

2.2.1 Mere Probability Model

First, infants could simply consider low-probability events to be surprising given their current expectations. This account is intuitive, as we tend to consider surprising events as having a low probability of occurrence. Furthermore, previous studies have demonstrated that infants as young as 6 months do look longer at low-probability events as compared to high-probability events (Denison et al., 2013; Téglás et al., 2011; Téglás, Girotto, Gonzalez, & Bonatti, 2007; Xu & Garcia, 2008). This model thus predicts that infants’ looking times will be closely related to
the mere probability, $P(d|h_{random})$, of individual sequences of balls, $d$, given $h_{random}$, the hypothesis that the balls are being tossed out randomly from the box. We use the negative log probability of these events, as this measure quantifies how surprising it is to see a particular outcome (Kidd et al., 2012). The probability of the sequences is

$$P(d \mid h_{random}) = \left( \frac{1}{k} \right)^N,$$

(1)
as the probability of each color is inversely proportional to the number of colors $k$ and the draws are assumed to be independent.

### 2.2.2 Kinds Model

The second computational strategy is to use the probability of kinds of events, $P(kind(d) \mid h_{random})$, such that sequences of an unlikely kind are considered surprising. As such, rather than evaluating the probability of a single event, we evaluate the probability of sets of events. Under this approach, events can be segmented into three “kinds”: “all outcomes are the same,” “all outcomes are different”, and “all other outcomes”. For the first set of events, the probability is

$$P\left(\text{kind outcomes are all the same (d)} \mid h_{random}\right) = k \cdot \left( \frac{1}{k} \right)^N.$$  

(2)

The intuition underlying this equation is as follows: when there are $k$ different colored balls (e.g. red, blue, and green balls) in the population box, there is a maximum of $k$ sequences where all the outcomes are the same (e.g. all reds, all blues, and all green balls). By multiplying $k$ by the probability of each specific sequence (given by Eq. 1), we obtain the total probability of the set “all outcomes are the same”.

For “all outcomes are different”, the probability of the sequences is

$$P\left(\text{kind outcomes are all different (d)} \mid h_{random}\right) = \frac{k!}{(k-N)!} \cdot \left( \frac{1}{k} \right)^N.$$  

(3)

The intuition underlying this equation is very similar to that explained above: there are $k!/(k-N)!$ sequences where each outcome is different, so we multiply this term by the probability of each specific sequence (given by Eq. 1) to obtain the total probability of the set “all outcomes are different”.

For “all other outcomes,” the probability of the sequences is
\[
P\left( \text{kind}_{\text{allotheroutcomes}}(d) \mid h_{\text{random}} \right) = 1 - k \cdot \left( \frac{1}{k} \right)^N - \frac{k!}{(k-N)!} \cdot \left( \frac{1}{k} \right)^N,
\]

since this set of events encompasses all events that are neither all the same nor all different.

### 2.2.3 Bayesian Model

Finally, infants could evaluate the evidence that a certain sequence, \( d \), provides for the alternative theory, as compared to the currently favored theory, \( P(d \mid h_{\text{alternative}})/P(d \mid h_{\text{current}}) \) (the likelihood ratio; Griffiths & Tenenbaum, 2007), in a process that is consistent with principles of Bayesian learning (Griffiths et al., 2010; Tenenbaum & Griffiths, 2001; Tenenbaum et al., 2011). For our experiments, \( h_{\text{current}} \) refers to the currently favored hypothesis that sampling is random (i.e., \( h_{\text{random}} \)), as the experimenter appears to have no control over the outcome of the tosses from the box. The alternative hypothesis, \( h_{\text{alternative}} \), is thus that sampling is not random (i.e., \( h_{\text{biased}} \)).

Assuming each biased distribution is equally likely, a derivation given in Appendix A yields the likelihood ratio:

\[
\frac{P(d \mid h_{\text{alternative}})}{P(d \mid h_{\text{current}})} = \frac{(k^N)(k-1)! \left( \prod_{i=1}^{k} n_i ! \right)}{(N + k - 1)!}.
\]

A likelihood ratio larger than 1 indicates that there is a greater probability of observing event \( d \) under the alternative theory than the currently favored theory. Under this model, an event becomes more surprising as the likelihood ratio increases. For example, the probability of observing a ball pass through a wood panel is virtually zero given our solidity expectations: objects move only on unobstructed paths (Spelke et al., 1992). When such an event is observed (typically only in an infant VOE experiment), the likelihood ratio is practically infinite since the event is much better accounted for by alternative hypotheses. As such, a solidity violation would produce longer looking times in infants.

### 2.2.4 Model Predictions

These models make different predictions, which we test in two looking time experiments. In Experiment 1 (Figure 2.1A), we considered two sequences of balls randomly sampled from a population of 6 different colored balls: a uniform sequence (e.g. orange, orange, orange, orange), and a variable sequence (e.g. red, green, blue, orange). As the two sequences are equal in probability given random sampling, the mere probability model predicts that looking times should not be different for the two sequences. However, the kinds and Bayesian models predict otherwise: infants should look longer at the uniform sequence as it is of a \( \text{kind} \) of event that is less probable, and because it has a higher likelihood ratio, respectively.

In Experiment 2 (Figure 2.1B), we considered sequences of different lengths, e.g. a uniform sequence of 3 balls, and a variable sequence of 6 balls. Both the mere probability and
kinds models predict that infants should look longer at the variable sequence as it is respectively a lower probability event and of a less probable kind of event. In contrast, the Bayesian model predicts that infants should look longer at the uniform sequence, as it continues to have a higher likelihood ratio.

2.3 Experiment 1: Evaluating mere probability

2.3.1 Method

Participants. Forty infants (21 males and 19 females, $M = 8; 6$ [months; days], $R = 7; 3$ to 9; 1) were tested. All were recruited from Berkeley, California, and its surrounding communities. An additional 7 infants were tested but excluded due to fussiness (N = 5) or experimenter error (N = 2).

Materials. A total of 36 colored balls (7 cm in diameter) were used. The balls came in 6 colors: red, purple, blue, green, yellow and orange. A small white box (28 cm x 10 cm x 7.5 cm) constructed from foam core was used in the Free Play phase of the experiment (see Design and Procedure). The box contained 3 different colored balls. A small, transparent plastic container with an open top (16.5 cm x 7.5 cm x 9 cm) was used to display the sampled ball during the test trials. A large box (30 cm x 26 cm x 21 cm) was used to display the population of 6 different colored balls during the familiarization and test phase. The box was rectangular, with a Plexiglas window to show the population of balls, and two hidden back compartments. One compartment was used to hold the balls to be tossed out later during the test trials, while the other compartment was to contain the balls that were being returned to the box after each toss. From the infants’ perspectives, the box appeared as one single unit, filled only with 6 different colored balls. The Plexiglas display window was covered with a fabric curtain to ensure that the population would be hidden from sight while each ball was being tossed out.

Apparatus. The testing room was divided in half by curtains spanning its width and height. The curtains had a cut-out above a puppet stage that measured 94 cm x 55 cm (width x height). The experimenter sat behind the stage with her upper body and head visible to the infant. A black back curtain was attached to the stage, such that the experimenter was hidden from view when it was dropped. An observer, present to code the infant’s looking times, sat in a corner of the room and was not visible to the infant. She watched the infant on a TV monitor and coded the infant’s looking behavior online using JHAB (Casstevens, 2007). The observer was blind to the order of the test trials.

Infants sat in a high chair about 70 cm from the center of the stage. One parent sat next to the infant facing the opposite direction, and was instructed to avoid looking at the stage. Two video cameras were used to record each experimental session, one to record the infant’s looking behavior, and the other to record the experimenter’s presentation of the trials.

Design and Procedure. Each infant was randomly assigned to an Experimental condition or a Control condition. Both conditions consisted of a Calibration phase, a Free Play phase, a Familiarization phase, and a Test phase.
Figure 2.1: Sequences of balls presented to infants over two experiments. Looking times were measured for each of these sequences, and the order of presentation for all pairs of sequences was counterbalanced across infants.

**Experimental Condition.** To calibrate each infant’s looking window, a squeaky toy or keys were used in the Calibration phase to direct the infant’s attention to the outside parameters of the stage. In the subsequent Free Play phase, the infant was shown a white box containing three different colored balls. She was encouraged to play with the balls for approximately 30 seconds, and the experimenter ensured that the infant touched every ball. This phase was to allow the infants to become familiar with the balls used in the experiment.

The Familiarization phase that followed consisted of two trials. To begin each trial, the experimenter placed the large box on the stage with its front curtain down. Then, she lifted the curtain to reveal a population of 6 different colored balls, saying “See this?” She proceeded to shake the box side to side 4 times, and then put the box back to the center of the stage. While the infant was looking at the stage, the experimenter said “Look, [baby’s name], look!” and dropped the back curtain, hiding herself from view of the infant. The observer began timing upon hearing the second “look”. Trials ended when the infant looked away for 2 consecutive seconds.

The large box was removed from the stage between each familiarization trial, and the back curtain was lowered to conceal the experimenter. These trials were included to familiarize the infants to the population of balls in the large box, as well as to the general procedure of the experiment. The familiarizations lasted about 2 minutes.

The Test phase consisted of two test trials, a Uniform trial and a Variable trial. On each test trial, the experimenter placed the large box and the small plastic container on the left and
right side of the stage (infant’s view) respectively, about 8 cm apart. The experimenter then lifted
the front curtain of the large box, saying “What’s this?” She lowered her head and directed her
eye gaze at the box for 1 second, in order to remind the infant of the population of balls in the
large box. She then picked up the box and shook it 4 times. After the box was set back down, the
experimenter lowered the front curtain to conceal the box’s display window. Then, the box was
lifted and tilted to its side, allowing one ball to fall out into the small container. Although it
appeared that the ball had fallen out from the population of balls at random, the ball actually fell
out of the back compartment of the box, which contained balls that had initially been set up by
the experimenter. The experimenter then directed her gaze towards the ball in the small
container, saying “Look at that!” After 1 second, the ball was returned into the box. This process
of sampling-with-replacement – revealing the population, shaking the box, covering the front of
the box, and tossing a ball out then returning it to the box – was repeated 3 more times, to make a
sequence of 4 sampled balls. When the 4th ball was tossed out into the small container, the
experimenter said “Look, [baby’s name], look!” and dropped the back curtain of the stage. The
observer began timing upon hearing the second “look,” and ended the trial after the infant looked
away for 2 consecutive seconds. Between trials, the stage was cleared and the back curtain was
lowered. Each test trial lasted for approximately 2 minutes.

Each infant participated in a Uniform trial and a Variable trial (See Fig. 1). The last ball
that was tossed out in the Variable trial was always identical in color to the balls used in the
Uniform trial, to ensure that any difference in looking time was not due to a preference for balls
of a certain color. Trial order and the colors of the sampled balls were counterbalanced across
infants.

**Control Condition.** The procedure in the Control condition was similar to that of the
Experimental condition. The key difference between the two conditions occurred in the Test
phase. To begin this phase, each infant was presented with a large box containing 6 different
colored balls, and a small plastic container. The experimenter then lifted the front curtain of the
large box to remind the infant of the population of balls within. As before, the box was picked
up, and the experimenter shook it side to side 4 times. The box was then set back down, and its
front curtain was lowered to conceal the box contents. Now, instead of tilting the box to its side
for a ball to fall out, the experimenter, while looking straight ahead at the infant, reached into her
pocket and pulled out a colored ball. This ball was placed in the small container, and the
experimenter directed her gaze towards it, while saying, “Look at that!” After 1 second, the ball
was returned into the experimenter’s pocket. This process was repeated 3 times, to make a
sequence of 4 balls. When the 4th ball was placed into the small container, the experimenter said
“Look, [baby’s name], look!” and dropped the back curtain of the stage. The observer began
timing upon hearing the second “look,” and ended the trial after the infant looked away for 2
consecutive seconds.

Like the infants in the Experimental condition, infants in the Control condition each
viewed a Uniform trial and a Variable trial. As such, the perceptual displays in the two
conditions were identical. The Control condition thus provided a measure of the infants’ pattern
of looking times for the two types of trials, in the absence of sampling from the population (see
Xu & Garcia, 2008, for a similar manipulation).
2.3.2 Results

A second observer coded 50% of the infants’ videos offline. Reliability was calculated as the proportion of total time both observers agreed that infants were looking at the displays. Thus, percentage agreement = 1 – [(absolute difference in time between original and second coder)/original coder]. Interscorer reliability averaged 95%. We then obtained an average across all 80 trials (40 familiarization trials and 40 test trials).

Preliminary analyses found no effects of gender or the order that the test trials were presented (Uniform trial first vs. Variable trial first) on looking times. Subsequent analyses were collapsed over these variables.

Looking times for the test trials were analyzed using a 2 x 2 repeated-measures ANOVA with Condition (Experimental vs. Control) as the between-subjects factor and Trial Type (Uniform vs. Variable) as the within-subjects factor. There were no main effects. There was a significant interaction between Condition and Trial Type, $F(1, 38) = 11.58$, $p = .002$, $\eta^2_p = .23$.

To break down the interaction, we conducted follow-up t-tests examining the effect of Trial Type (Uniform vs. Variable) for each Condition separately. In the Experimental condition, infants looked significantly longer in the Uniform trial ($M = 13.68s$, $SD = 9.87$) than the Variable trial ($M = 10.22s$, $SD = 6.35$), $t(19) = 2.49$, $p = .02$, $d = .42$. Thirteen out of 20 infants in this condition looked longer in the Uniform trial, Wilcoxon signed-ranked test: $z = 1.93$, $p = .05$. In contrast, infants in the Control condition looked significantly longer in the Variable trial ($M = 15.96s$, $SD = 9.02$) than the Uniform trial ($M = 10.14s$, $SD = 6.01$), $t(19) = 2.48$, $p = .02$, $d = .76$. Fifteen out of 20 infants in this condition looked longer in the Variable trial, Wilcoxon signed-ranked test: $z = 2.35$, $p = .02$.

2.3.3 Discussion

Infants looked longer when randomly sampled balls were of the same color than of different colors, even though the sequences had equal probabilities of occurring. Hence, infants’ longer looking is not simply a consequence of observing low-to-zero probability events. Additionally, looking times were reversed in the Control condition, indicating that the results cannot be attributed to a preference for looking at sequences of identical events.

2.4 Experiment 2: Evaluating the probability of kinds of events

2.4.1 Method

Participants. Forty infants (23 males and 17 females, $M = 8; 4$ [months; days], $R = 7; 6$ to 9; 0) were tested. All were recruited from Berkeley, California, and its surrounding communities. None of them had participated in Experiment 1. An additional 5 infants were tested but excluded due to fussiness ($N = 2$), experimenter error ($N = 1$), or parental interference ($N = 2$).
Materials, Apparatus, and Procedure. The materials, apparatus, and procedure were identical to Experiment 1, except that the Uniform sequence now consisted of 3 balls of the same color, and the Variable sequence consisted of 6 balls of different colors (see Fig. 2).

2.4.2 Results

A second observer coded 50% of the infants’ videos offline. Interscorer reliability averaged 93%. Preliminary analyses found no effects of gender or the order that the test trials were presented (Uniform trial first vs. Variable trial first) on looking times. Subsequent analyses were collapsed over these variables.

A repeated-measures ANOVA with Condition (Experimental vs. Control) as the between-subjects factor and Trial Type (Uniform vs. Variable) as the within-subjects factor was performed on the obtained looking times. There were no main effects. There was a significant interaction between Condition and Trial Type, \( F(1, 38) = 12.16, p = .001, \eta_p^2 = .24. \)

We analyzed the interaction with follow-up t-tests that examined the effect of Trial Type (Uniform vs. Variable) for each Condition separately. Similar to the results found in Experiment 1, infants in the Experimental condition looked significantly longer in the Uniform trial \((M = 14.07 \text{s}, SD = 7.55)\) than the Variable trial \((M = 9.91 \text{s}, SD = 6.07)\), \(t(19) = 3.14, p = .005, d = .72. \)

Fifteen out of 20 infants in this condition looked longer in the Uniform trial, Wilcoxon signed-rank test: \(z = 2.60, p = .009. \) In contrast, infants in the Control condition looked significantly longer in the Variable trial \((M = 16.76 \text{s}, SD = 8.88)\) than the Uniform trial \((M = 13.06 \text{s}, SD = 6.02)\), \(t(19) = 2.03, p = .05, d = .48. \) Thirteen infants looked longer in the Variable trial than the Uniform trial in the Control condition, marginally significant by a Wilcoxon signed-ranked test: \(z = 1.87, p = .06. \)

2.4.3 Discussion

In Experiment 2, we replicated the earlier results: infants looked longer at the uniform sequence, consistent with the predictions of the Bayesian model. Once again, the pattern of looking times was reversed in the Control condition, showing that infants do not preferentially look at sequences of identical events.

2.5 Quantitative Analysis

Experiments 1 and 2 tested the qualitative predictions of the three formal models, but the statistical analyses so far do not provide a quantitative measure of how well the different models perform with regards to capturing patterns of infant looking times. To get a better sense of these models, it was necessary to obtain more data. Using the Bayesian model, we identified new sequences in which infants were predicted to show looking times that would fall between the mean looking times obtained for the Variable trials and those obtained for the Uniform trials in the Experimental conditions. We tested these predictions with twenty infants (11 males and 9 females, \(M = 8; 4 \text{ [months; days]}\), \(R = 7; 2 \text{ to 9; 0}\)) using the same method as Experiments 1 and 2. Infants were presented with sequences that varied in the number of repeated colors (e.g., 2
repeats: red, green, orange, orange, and 3 repeats: red, orange, orange, orange). Altogether, we obtained average looking times for 10 different types of events: the Experimental condition covered 6 different types of events, and the Control condition covered 4 different types of events (modeling details for the Control conditions are provided in Appendix A).

Figure 2.2 plots the average looking times on the vertical axis, against the predictions made by the three different computational models on the horizontal axis. We use negative log probability on the horizontal axis for the first two models. For the Bayesian model, the horizontal axis gives log likelihood ratios.

![Figure 2.2: Correlation between the model predictions (horizontal axis) and infants’ mean looking times obtained across experiments (vertical axis). The red data points correspond to sequences of balls presented in the Experimental condition, and the blue data points correspond to sequences of balls presented in the Control condition.](image)

The mean looking times obtained for the additional sequences of balls containing repeated colors (2 repeats: $M = 10.89s$, $SD = 6.08$; 3 repeats: $M = 12.97s$, $SD = 8.21$) were consistent with the predictions of the Bayesian model. Only the Bayesian model provided a high quality of model fit ($r = .96$, $df = 8$, $p = .0002$). The predictions correlated well with the looking times, unlike those of the mere probability model ($r = .53$, $df = 8$, $p = .32$) and kinds model ($r = .53$, $df = 8$, $p = .32$) (see Appendix A).

Our correlation estimate for the Bayesian model and the standard errors of our infant looking times across the 10 different events were similar to those in Téglás et al. (2011), who based their conclusions on the analysis of 12 different events. As an additional step to address possible concerns about modeling small numbers of events, we used a bootstrap analysis (Efron & Tibshirani, 1993) to construct confidence intervals, resampling our datasets 10,000 times for each of the conditions reported. For the full set of conditions (Experiment and Control; 10 different events), we obtained a 95% confidence interval of [.607, .959] for the correlation reported for the Bayesian model, with the observed correlations for the other two models falling outside this interval. The confidence intervals for the other two models were [.142, .769] for the
mere probability model and [.172, .762] for the kinds model, with the observed correlation for the Bayesian model falling outside both intervals.

One remaining objection to this account is that a modified Kinds model may better capture the obtained looking times in our experiments. This model is similar to the Kinds model, except that instead of three “kinds” of events, only two “kinds” of events are specified: “all outcomes are the same,” vs. “not all outcomes are the same.” For the Experimental conditions and Control conditions, the negative log probability of the sequences for the former set of events can be computed using Equation 2 and Equation A6, respectively (see Appendix A). As for the latter set of events, the probability of the sequences in the Experimental condition is

\[ P\left(kind_{not\ all\ outcomes\ are\ the\ same\ (d)}\mid h_{random}\right) = 1 - \left[ k \cdot \left( \frac{1}{k} \right)^N \right], \tag{6} \]

and the probability of the sequences in the Control condition is

\[ P\left(kind_{not\ all\ outcomes\ are\ the\ same\ (d)}\mid h_{alternative}\right) = 1 - k \cdot (k - 1)! \frac{\prod (n_i)}{(N + k - 1)!}. \tag{7} \]

The probability of the latter set of sequences can be computed as such because the two categories of events are complementary. As shown in Figure 2.3, the modified kinds-based model did not provide a high quality of model fit.

**Figure 2.3:** Correlation between the predictions of the Modified Kinds model (*horizontal axis*) and infants’ mean looking times obtained across experiments (*vertical axis*). The red data points correspond to sequences of balls presented in the Experimental condition, and the blue data points correspond to sequences of balls presented in the Control condition.
2.6 General Discussion

Overall the Bayesian model provided the most satisfying account of looking times, demonstrating that 8-month-old infants’ surprise reactions are sensitive to the strength of evidence against the current hypothesis: they modulated their looking times according to how well the data supported the set of lower-probability alternative hypotheses over the currently favored hypothesis.

In our experiments, seeing the same colored ball fall out each time is surprising because it is better supported by alternative hypotheses (e.g. there might be a hidden compartment full of orange balls; the orange ball is heavier; the experimenter has played a trick on me; my understanding of random sampling is incorrect, etc.) than the currently favored hypothesis (i.e. sampling is random). Infants looked longer when the observed sequence better supported an account of the world that is substantially different than expected.

We emphasize here that the current work represents a computational level account (Marr, 1982) of infant looking times. We do not claim that infants are computing likelihood ratios according to the equations we used to specify our model, and it remains an open question how the workings of our Bayesian model may correspond to actual mechanisms of infant reasoning. That being said, we believe these findings will guide future research into the precise mechanisms and representations that infants use to evaluate incoming data from their environment (Griffiths, Chater, Kemp, Perfors, & Tenenbaum, 2010).

One way that these findings have started to accomplish that is to offer a notably divergent view from traditional interpretations of the looking time method. Developmental researchers have typically considered longer looking times to be a reflection of perceptual discrimination (Fantz, 1964; Johnson, 2011) or a reaction to witnessing a violation of prior expectations (Baillargeon et al., 1985; Spelke, 1985; Wynn, 1992). The current work investigates the underlying processes that give rise to different levels of looking by finding quantitative fits between infant looking times and pre-specified formal models (c.f. Teglas et al., 2011). When put together, our empirical results and modeling work support the hypothesis that looking time may reflect inferential reasoning processes in young human learners.

At the same time, the current study builds upon previous results demonstrating that infants’ looking times are well-predicted by the probability of observed events (Kidd et al., 2012; Téglás et al., 2011). The use of sequences of events in our experiments allowed us to expand on and clarify this body of work by showing that the inferential capacity of young infants goes beyond evaluating single-event probabilities. Infants are not only evaluating the probability of events themselves, they also evaluate and compare the probability of observing the event given their currently favored hypothesis against the probability of observing the same event given an alternative hypothesis. Accordingly, our Bayesian model is able to capture the obtained looking time patterns in Téglás et al. (2011) and Kidd et al. (2012) with no further assumptions (see Appendix A).

Our findings also dovetail with the view recently presented in Stahl and Feigenson (2015): Looking time has been extraordinarily useful over the past few decades as a tool for researchers to diagnose core knowledge. However, relatively few studies have been conducted to understand the role that looking time itself plays in an infant’s development. Stahl and Feigenson
(2015) found that the surprise reaction, which is manifested in the form of longer looking time, signals new opportunities for learning. Likewise, Sim and Xu (2013) found that 13-month-old infants preferentially explore sources that generate sequences that infants are surprised by. Similar to such previous work, our findings indicate that the underlying cognitive process that drives looking times is congruent with the principles of Bayesian learning: the learner begins the learning process by assessing the fit between the observed evidence and her current highest-probability hypothesis, and compares it to the fit provided by a set of lower-probability hypotheses. Taking this first step enables the learner to move towards theory building and theory revision, allowing her to form the best generative model for the data observed in the world.

Our use of probabilistic reasoning tasks—which may appear abstract and contentless—may make the reader wonder if the inferential capacity uncovered here will apply to other knowledge domains such as physical reasoning or psychological reasoning in infants. We believe it will, for several reasons. Our task taps into infants’ physical and psychological knowledge: Infants need to understand that the balls are individual objects that can be manipulated, and the sampling process may be driven by pure chance (experimental conditions) or may be controlled by an agent (control conditions). These two sampling assumptions indeed led to different inferences by the infants. Furthermore, other studies in the literature suggest that infants can integrate physical and psychological constraints in their probabilistic computations. For example, Téglás et al. (2011) showed that infants were exquisitely sensitive to the duration of occlusion time when faced with a lottery-machine (e.g., three blue and one yellow objects bouncing around), and looking times were well-predicted by integrating probability with physical knowledge (also see Denison & Xu, 2010). Similarly, Xu and Denison (2009) demonstrated that infants apply psychological constraints, e.g., expressed goal and visual access, in computing probable vs. improbable outcomes. In each of these cases, infants may have entertained alternative hypotheses when the improbable outcomes were shown, but the methods employed in these studies do not allow us to probe this question. In future studies, one can manipulate the degree of a violation and the strength of the counterevidence (e.g., what if not just 1, but 10 balls pass through a barrier, violating solidity) in order to investigate further how infants generate alternative hypotheses, and how they revise their current beliefs.

Finally, our results may remind readers of Tversky and Kahneman’s (1971) well-known heuristic, “the law of small numbers”. According to this heuristic, infants may have looked longer at the uniform sequences because they were less representative of the population of six distinctly colored balls. However, this heuristic describes an important phenomenon of human reasoning—it does not explain it (Gigerenzer, 1996, Griffiths & Tenenbaum, 2001). We argue, instead, that intuitions about representativeness may be accounted for by an evaluation of the observed evidence against current and alternative hypotheses.

Together, our results constitute the first evidence that when infants observe events in their environment, they reason in a manner that is consistent with evaluating those events according to how well they support different hypotheses. When infants encounter unexpected evidence, they compute (implicitly) how likely the evidence has been generated by alternative hypotheses. Our findings do not speak directly to what alternative hypotheses infants entertain, but they capture the idea that a placeholder for an alternative hypothesis has been warranted by the unexpected evidence. This is analogous to what scientists do: researchers often start with the suspicion that a
surprising set of data is not just a coincidence (Griffiths & Tenenbaum, 2007), then they go on to test specific alternative hypotheses with further data gathering.

By considering alternative hypotheses, infants may thus be in good stead to engage in theory building and theory revision in the face of surprising information. The finding that such a sophisticated form of processing is in place so early on in development lends credence to the idea that infant learners are well-prepared and motivated to uncover the true structure of the world.

2.7 Appendix

2.7.1 Derivation of the Bayesian Model: Experimental and Control Conditions

When we have any two hypotheses, \( h_{\text{current}} \) and \( h_{\text{alternative}} \), we can express the relative degree of belief in the alternative hypothesis, \( h_{\text{alternative}} \), over the currently favored hypothesis, \( h_{\text{current}} \), after seeing the data, \( d \), by using the posterior odds,

\[
\frac{P(h_{\text{alternative}} \mid d)}{P(h_{\text{current}} \mid d)} = \frac{P(d \mid h_{\text{alternative}})P(h_{\text{alternative}})}{P(d \mid h_{\text{current}})P(h_{\text{current}})},
\]

which is the product of the likelihood ratio, \( \frac{P(d \mid h_{\text{alternative}})}{P(d \mid h_{\text{current}})} \), and the prior odds, \( \frac{P(h_{\text{alternative}})}{P(h_{\text{current}})} \).

The likelihood ratio is essentially the ratio of the likelihood of observing the data given the alternative hypothesis, to the likelihood of observing the data given the currently favored hypothesis. When the likelihood ratio is smaller than 1, it means that the learner is more likely to observe \( d \) when the current hypothesis, \( h_{\text{current}} \), is true, than when the alternative hypothesis, \( h_{\text{alternative}} \), is true. When the likelihood ratio is greater than 1, e.g. 1.8, it means that the learner is 1.8 times more likely to observe \( d \) when the alternative hypothesis, \( h_{\text{alternative}} \), is true, than when \( h_{\text{current}} \), is true. In other words, the likelihood ratio, \( \frac{P(d \mid h_{\text{alternative}})}{P(d \mid h_{\text{current}})} \), quantifies the support that \( d \) provides for \( h_{\text{alternative}} \) over \( h_{\text{current}} \).

The prior odds capture the relative belief that we have for \( h_{\text{alternative}} \) over \( h_{\text{current}} \) before observing any evidence. When the prior odds is smaller than 1, it means that the learner has a greater prior belief in the current hypothesis, \( h_{\text{current}} \), than the alternative hypothesis, \( h_{\text{alternative}} \). Whereas when the prior odds are greater than 1, it means that the learner has a greater prior belief in the alternative hypothesis, \( h_{\text{alternative}} \), than the current hypothesis, \( h_{\text{current}} \). However, the latter situation is unlikely since a learner typically has a stronger belief in the currently favored hypothesis before he/she has observed any evidence.
If infants’ surprise reaction is driven by the evaluation of the observed evidence against different alternative hypotheses, we can model their looking times using the likelihood ratio term. The derivation of the likelihood ratio term for the Experimental condition is as follows. In the Experimental condition, the dominant hypothesis is that balls are being tossed out randomly from the box. As such, $h_{current}$ is in effect $h_{random}$, and the likelihood for $h_{current}$ is the same as that used in the mere probability model:

$$P(d \mid h_{current}) = \left(\frac{1}{k}\right)^N$$

On the other hand, $h_{alternative}$ for this condition is $h_{biased}$, which states that there is a biased process at work, i.e. sampling is not random. To compute $P(d \mid h_{alternative})$ for the Experimental condition, we compute the likelihood for $h_{biased}$. First, we define a prior distribution on $\theta = (\theta_1, \theta_2, \ldots, \theta_k)$, where $\theta_i$ is the true probability of observing the event where a ball tossed out belongs to category $i$. If we define a prior distribution, $P(\theta)$, then we can compute

$$P(d \mid h_{alternative}) = \int P(d \mid \theta) P(\theta) \, d\theta$$

(A2)

We have to take this integration step because there are many possible alternative hypotheses about the true probability of observing each colored ball. For example, one alternative hypothesis might be that the orange ball is slightly heavier than the rest of the balls, such that the probability of observing an orange ball is .2, while the probability of observing each of the rest of the balls is .16. Another alternative hypothesis might be that there are many more orange balls hidden in an unseen back compartment, such that the probability of observing an orange ball is .8 instead, while the probability of observing each of the rest of the balls is only .04.

Since there is no a priori reason to consider one alternative hypothesis to be more likely than another, we take $P(\theta)$ to be a Dirichlet-Multinomial distribution where $\alpha = (\alpha_1, \alpha_2, \ldots, \alpha_k) = 1$, corresponding to a uniform prior expectation about the distribution of events,

$$P(\theta \mid \alpha) = \frac{\Gamma\left(\sum_{i=1}^{k} \alpha_i\right)}{\prod_i \Gamma(\alpha_i)} \prod_i \theta_i^{\alpha_i-1}$$

$$= \Gamma(k)$$

(A3)

Substituting this distribution into Equation A2, we obtain

$$P(d \mid h_{alternative}) = \int \prod_i \theta_i^n \, \Gamma(k) \, d\theta$$
\[ = \Gamma(k) \int \prod_{i} \theta^{n_i} d\theta \]
\[ = \Gamma(k) \frac{\prod_{i} \Gamma(n_i + 1)}{\Gamma \left( \sum_{i=1}^{n} n_i + 1 \right)} \]
\[ = \Gamma(k) \frac{\prod_{i} \Gamma(n_i + 1)}{\Gamma(N + k)} , \]

where \( n_i \) is the number of times a particular category \( i \) has been observed.

\[ \Gamma(r) = \int_{0}^{\infty} x^{r-1} e^{-x} dx \]

is the generalized factorial function, with \( \Gamma(r) = (r-1)! \) for integer values of \( r \). Given the \( k \), \( N \), and \( n_i \) are integers, this simplifies to

\[ P(d \mid h_{\text{alternative}}) = (k-1)! \frac{\prod_{i} (n_i)!}{(N + k - 1)!} , \]

(A4)

from which it follows that the likelihood ratio in favor of \( h_{\text{alternative}} \) in the Experimental condition is

\[ \frac{P(d \mid h_{\text{alternative}})}{P(d \mid h_{\text{current}})} = \frac{(k^N)(k-1)! \left( \prod_{i}^{k} n_i! \right)}{(N + k - 1)!} . \]

(A5)

For the Control condition, the experimenter appears to have full control over the presented sequence of balls. Therefore, \( h_{\text{current}} \) now refers to \( h_{\text{biased}} \), the hypothesis that sampling is not random, while \( h_{\text{alternative}} \) is now \( h_{\text{random}} \), the hypothesis that sampling is random. The likelihood ratio term for the Control condition is thus simply the inverse of that for the Sampling condition.

2.7.2 Mere Probability Model: Control Conditions

As the experimenter has full control over the presented sequence of balls and there is no a priori reason to consider one alternative hypothesis to be more likely than another, Equation A4 can be used to compute the negative log probability of the sequences presented in the control conditions.
2.7.3 Kinds Model: Control Conditions

For the Control conditions, the computational strategy employed by the Kinds model is to use the probability of *kinds* of events given the alternative hypothesis that sampling is not random. As such, the three “kinds” of events (“all outcomes are the same,” “all outcomes are different,” and “all other outcomes”) can be computed as follows:

\[
P\left(\text{kind outcomes are all the same (d)} \mid h_{\text{alternative}}\right) = k \cdot (k-1)! \cdot \frac{\prod_{i} (n_i!)}{(N + k - 1)!}\]

(A6)

\[
P\left(\text{kind outcomes are all different (d)} \mid h_{\text{alternative}}\right) = \frac{k!}{(k-N)!} \cdot \frac{(k-1)!}{(N + k - 1)!} \cdot \frac{\prod_{i} (n_i!)}{(N + k - 1)!}\]

(A7)

and \( P\left(\text{kind all other outcomes (d)} \mid h_{\text{alternative}}\right) \) is 1 minus the probabilities given in Equations A6 and A7.

2.7.4 Modeling Kidd et al. (2012) and Téglás et al. (2011)

When considering events that result from a single random draw from a population, an account of looking times based on the probability of events, \( P(d \mid h_{\text{current}}) \), makes identical predictions as an account based on likelihood ratios, \( \frac{P(d \mid h_{\text{alternative}})}{P(d \mid h_{\text{current}})} \) when the alternative hypothesis corresponds to a process characterized by an unknown distribution over a discrete set of possibilities. The reason is that when computing \( P(d \mid h_{\text{alternative}}) \), the prior distribution is taken to be a Dirichlet-Multinomial distribution where \( \alpha = (\alpha_1, \alpha_2, \ldots, \alpha_k) = 1 \), which results in the alternative hypothesis giving a constant probability to the different possible single-event outcomes. Since \( P(d \mid h_{\text{alternative}}) \) is a constant in these special cases, \( P(d \mid h_{\text{current}}) \) would be predictive of infant looking times. Figure 2.A1 shows that results from previous work on sensitivity to probabilities in infants are equally consistent with sensitivity to likelihood ratios.
Figure 2.A1: Modeling previous results. (A) Results from Kidd, Piantadosi and Aslin (2012) showing that probability is predictive of infants’ look-away probability. Using a Bayesian model based on likelihood ratios (B) produces identical findings. (C) Results from Teglas et al. (2011) showing that probability is predictive of infants’ looking times for physical scenes. Again, the Bayesian model (D) produces identical findings.
Chapter 3

Infants Preferentially Approach and Explore the Unexpected

3.1 Introduction

Over the last few decades, looking time experiments based on the violation-of-expectation (VOE) method have revealed infants’ extant knowledge in a variety of domains, such as their physical knowledge (e.g., Baillargeon et al., 1985; Baillargeon, 2008; Spelke et al., 1992), numerical knowledge (e.g., Mccrink & Wynn, 2015; Wynn, 1992), statistical and probabilistic intuitions (e.g., Fiser & Aslin, 2002; Kirkham, Slemmer, & Johnson, 2002; Téglás et al., 2007; Xu & Garcia, 2008), and theory of mind (e.g., Gergely, Nádasdy, Csibra, & Bíró, 1995; Onishi & Baillargeon, 2005). Such studies have consistently demonstrated that infants look longer when their expectations are violated, but it remains an open question whether similar effects will be observed in infants’ approach behaviors. Specifically, do infants selectively approach and explore sources that violate their expectations?

We hypothesize that infants will do so, as unexpected events are great opportunities for learning since the world is behaving differently from one’s representation of it. Learners may thus seek new information that will allow them to possibly revise their beliefs (Griffiths & Tenenbaum, 2007; Leslie, 2004). There is some evidence supporting this hypothesis: in a recent study, researchers demonstrated that 11-month-olds, who were presented with two objects within their reach on their high-chair tray, preferentially explored the object involved in a prior event that violated their “core” physical expectations, over a novel object (Stahl & Feigenson, 2015).

In the current study, we contend that this selectivity is not limited to events that violate expectations drawn from core knowledge (Carey, 2009; Izard, Sann, Spelke, & Streri, 2009; Spelke et al., 1992). Expectancy violations involving other types of knowledge can similarly influence an infant’s subsequent exploration. Furthermore, the events do not have to be impossible; events that are improbable may also result in similar effects on exploration. We also attempt a stronger test of such selective exploration by employing a crawling paradigm in this study, which may be more reflective of an infants’ real-world environment as compared to previous reaching tasks (Hamlin, Wynn, & Bloom, 2008; Hespos & Baillargeon, 2006, 2008; Stahl & Feigenson, 2015).

To those ends, we designed a set of events analogous to sequences of die rolls. Infants were familiarized to a box containing 6 different-colored balls. They subsequently saw two sequences generated by random sampling with replacement: a variable sequence, in which a
different-colored ball fell out of the box over 4 tosses (e.g. blue-green-red-yellow), and a uniform sequence, in which the same-colored ball fell out each time (e.g. yellow-yellow-yellow-yellow). There are two types of reactions to these events: some adults judge the uniform sequence to be less probable because there are fewer possible same-color events than all possible different-color type events, while others judge the two sequences to be equally probable, as the tosses are independent of one another. However, note that all adults find the uniform sequence to be more unexpected than the variable sequence, reflecting the intuition that the former seems like a coincidence – it provides better support for a set of alternative theories (e.g. the box contains many hidden yellow balls; or the yellow ball has a special property), as compared to the currently favored theory of a fair box (See Griffiths & Tenenbaum (2007); Sim & Xu (2013) for further discussion).

Using this set of contrasting events, we conducted two experiments to investigate whether infants preferentially approach sources of unexpected events. Given the complexity of our looking time displays, as well as previous research that has shown success with 10- to 14-month-olds using a crawling paradigm (Cheries, Mitroff, Wynn, & Scholl, 2008; Denison & Xu, 2010; Feigenson, Carey, & Hauser, 2002), we chose to test 12- and 13-month-old infants in the current study. In Experiment 1, we used the traditional VOE method to first establish that under conditions of random sampling, 12- and 13-month-old infants consider the uniform sequence to be more unexpected than the variable sequence. The second experiment used a crawling paradigm to examine whether the selectivity observed in looking time will be similarly observed in approach behaviors.

3.2 Experiment 1: Looking Time Preferences

3.2.1 Method

Participants. Twenty-four infants (10 males and 14 females, $M = 13; 5$ [months; days], $R = 12; 17$ to 14; 1) were tested. An additional 4 infants were tested but excluded due to fussiness ($N = 3$) or experimenter error ($N = 1$).

Materials. Balls (7 cm in diameter) of 6 colors (red, purple, blue, green, yellow and orange) were used. There was also a small white box (28 cm x 10 cm x 7.5 cm), a transparent container with an open top (16.5 cm x 7.5 cm x 9 cm), and a large box (30 cm x 26 cm x 21 cm) with a Plexiglas window that displayed the population of balls within it. The large box had two hidden back compartments. From the infants’ perspectives, the box appeared as one single unit, filled only with 6 different colored balls. The Plexiglas display window was covered with a fabric curtain.

Design and Procedure. Infants sat in a high chair about 70 cm from a display stage, with their parents sitting next to them while facing the opposite direction. Each infant was randomly assigned to a Sampling condition or a Control condition. In the former, the experimenter tilted the large box to toss out sequences of balls. In the latter, the experimenter pulled the sequences of balls out of her pocket instead. Both conditions consisted of a Calibration phase, a Free Play phase, a Familiarization phase, and a Test phase.
**Sampling Condition.** To calibrate each infant’s looking window, a squeaky toy or keys was used to direct the infant’s attention to the outside parameters of the stage. In the subsequent Free Play phase, the infant was shown a white box containing three different colored balls. She was encouraged to play with the balls for approximately 30 seconds.

The Familiarization phase that followed consisted of two trials. To begin each trial, the experimenter placed the large box on the stage and lifted the curtain to reveal a population of 6 different-colored balls. She then shook the box 4 times. While the infant was looking at the stage, the experimenter said “Look, [baby’s name], look!” and dropped the back curtain to conceal herself. Upon hearing the second “look,” the observer began timing the infant’s looking behavior by using JHAB (R. Casstevens, 2007). Trials ended when the infant looked away for 2 consecutive seconds. These trials familiarized the infants to the population of balls in the large box.

The Test phase consisted of a Uniform trial and a Variable trial. On each test trial, the experimenter placed the large box and the small transparent container on the stage, 8 cm apart. The experimenter then lifted the front curtain of the large box, saying “What’s this?” She lowered her head and directed her eye gaze at the box for 1 second. She then shook it 4 times. After the box was set down, the experimenter lowered the front curtain to conceal the box’s contents. Then, the box was tilted to its side, allowing one ball to fall out into the small container. Although it appeared that the ball had been randomly sampled, it actually fell out of the back compartment of the box, which contained balls that had been previously set up. The experimenter then directed her gaze towards the “sampled” ball, saying “Look at that!” After 1 second, the ball was returned into the box. This process of revealing the population, shaking the box and tossing a ball out was repeated 3 more times, to make a total of 4 “sampled” balls. When the 4th ball was tossed out, the experimenter said “Look, [baby’s name], look!” and dropped the back curtain of the stage. The observer began timing upon hearing the second “look,” and ended the trial after the infant looked away for 2 consecutive seconds.

Each infant participated in a Uniform trial and a Variable trial (Figure 3.1A). In the Uniform trial, the 4 “sampled” balls were all of the same color, while in the Variable trial, the 4 “sampled” balls were all of a different color. The last ball tossed out in the two trials was always identical. Trial order and the colors of the sampled balls were counterbalanced across infants.

**Control Condition.** The purpose of this condition was to ensure that any differences in looking times found for the Uniform and Variable trial in the Sampling condition was due to the infants observing unexpected vs. expected sequences being generated by a random sampling process. As such, the procedure in the Control condition was identical to the Sampling condition, except that the four balls were individually taken out from and returned to the experimenter’s pocket, instead of being randomly tossed out of the population box. This condition thus provided a measure of infants’ preferences for the two different sequences of balls, Uniform vs. Variable, when the assumption of random sampling does not hold. The Control condition also ensured that any difference found between the looking times for the sequences presented in the Sampling condition was not due to an intrinsic preference for a certain type of sequence.
3.2.2 Results

A second observer coded 50% of the infants’ videos offline. Reliability was calculated as the proportion of total time both observers agreed that infants were looking at the displays. Thus, percentage agreement = 1 – [(absolute difference in time between original and second coder)/original coder]. Interscorer reliability averaged 95%. Preliminary analyses found no effect of gender on looking times. Subsequent analyses were collapsed over this variable.

Looking times for the test trials were analyzed using a 2 x 2 x 2 repeated-measures ANOVA with Condition (Sampling vs. Condition) and Trial Order (Uniform trial first vs. Variable trial first) as between-subjects factors and Trial Type (Uniform vs. Variable) as the within-subjects factor. There was a significant interaction between Condition and Trial Type, $F(1, 20) = 14.74, p = .001, \eta^2_p = .42$. There were no other interactions or main effects. Follow-up t-tests examined the effect of Trial Type (Uniform vs. Variable) for each Condition separately (See Figure 3.2 for mean looking times). In the Sampling condition, infants looked significantly longer in the Uniform trial ($M = 10.82s, SD = 6.52$) than the Variable trial ($M = 7.24s, SD = 4.78$), $t(11) = 2.56, p = .027, d = .78$. A non-parametric Wilcoxon signed-rank test also showed a significant difference in the infants’ looking times in the Uniform and Variable trial, $z = 1.96, p = .05$. Nine out of 12 infants in this condition looked longer in the Uniform trial, while the remaining three infants looked longer in the Variable trial.

In contrast, infants in the Control condition looked significantly longer in the Variable trial ($M = 10.44s, SD = 5.37$) than the Uniform trial ($M = 5.68s, SD = 1.65$), $t(11) = 2.87, p$
The result from the Wilcoxon signed-rank test was also significant, $z = 2.35$, $p = .019$. Ten out of 12 infants in this condition looked longer in the Variable trial, while the remaining two infants looked longer in the Uniform trial.

![Figure 3.2](image-url)  
*Figure 3.2.* Mean looking times in the Sampling condition and the Control condition. Error bars represent standard error.

### 3.2.3 Discussion

In the Sampling condition, infants looked reliably longer when 4 randomly generated balls were all of the same color, rather than of different colors. This pattern was reversed in the Control condition, suggesting that it was the presentation of a random sampling process that led infants to show longer looking times at the Uniform trial in the Sampling condition; infants did not intrinsically prefer sequences of identical balls. Hence, infants’ reactions are consistent with those of adults: under random sampling, the uniform sequence is more unexpected than the variable sequence.

### 3.3 Experiment 2: Exploration Preferences

#### 3.3.1 Method

**Participants.** Forty infants (22 males and 18 females, $M = 13; 4$ [months; days], $R = 12; 14$ to $13; 29$) were tested. An additional 5 infants were tested but excluded due to parental interference ($N = 4$) or failure to make a choice ($N = 1$).
Materials. The materials were identical to those used in Experiment 1, except that there were now two large population boxes (29 cm x 23 cm x 22 cm). The display window of one box was covered with a black fabric curtain, and the other a white one.

Design and Procedure. Infants were tested in a forced-choice paradigm. Each infant sat on her parent’s lap on the floor facing an elevated display stage approximately 1.2 meters away. The stage was about 75 cm above the ground and the base of the stage was covered with cloth. Parents were instructed to hold on to their infant, and then to set their infant on the floor when they heard the instruction, “Do you want to come and play?” Infants were randomly assigned to a Sampling condition or a Control condition. Both conditions consisted of a Free Play phase, a Demonstration phase, and a Test phase.

Sampling Condition. The Free Play phase was identical to that of Experiment 1. To begin the Demonstration phase, the experimenter placed two large boxes on the stage about 20 cm apart. The large box placed on the left side of the stage (infant’s view) was always the box with black fabric, and the large box on the right was always the box with white fabric. The experimenter always started the Demonstration phase with the box on the left, but whether this box was assigned to be a Uniform box or a Variable box was counterbalanced across infants. For half of the infants, the black box on the left was the Uniform box and the white box on the right was the Variable box, and for the other half of the infants, the black box on the left was the Variable box and the white box on the right was the Uniform box. The Uniform box was set up beforehand to produce a sequence of 4 same-colored balls, while the Variable box was set up beforehand to produce a sequence of 4 different-colored balls (Figure 3.1B).

The experimenter also placed a transparent container in the space between the two boxes. She then drew the infant’s attention to the box on the left, saying “What’s in this box?” The front curtain of this box was lifted, revealing a population of 6 different-colored balls. The procedure that followed was identical to the test trials in Experiment 1, in which the experimenter seemingly tosses out 4 balls from the box at random, one after another with replacement. The only exception was that the 4th ball was returned to the box after 1 second, as looking behaviors were not of interest here. She then pointed to the other box, and said “Let’s see what’s in this box!” The experimenter then repeated the steps performed on the previous box.

In the test phase, the experimenter always first placed the left box down on the ground to the left side of the stage, and then the right box down on the ground to the right side of the stage. The infant was equidistant from the two boxes, which were each approximately 1 meter away. The experimenter then returned to the center of the stage. After ensuring that the infant was looking directly at her, she said, “Do you want to come and play?” She then discontinued the joint attention with the infant by looking downwards towards the stage. When the infant touched one of the boxes, the experimenter started a timer and the test trial ended after 60 seconds.

If the infant did not touch either of the two boxes after 30 seconds, the experimenter retrieved the two boxes and sat on the floor approximately 60 cm from the infant. Using both hands, the experimenter then brought the two boxes forward simultaneously, keeping each box about 30 cm away from the infant. She repeated the instruction, “Do you want to come and play?” Once again, the experimenter only started a timer when the infant touched one or both of the boxes, and the test trial ended after 60 seconds.
Control Condition. The purpose of this condition was to ensure that any differences in the approach and exploration behaviors for the two boxes presented in the Sampling condition was due to the infants observing unexpected vs. expected sequences being generated by a random sampling process. As such, the procedure was identical to that in the Sampling condition, except that the balls were drawn out by the experimenter after she looked into the box. Since the experimenter had visual access while sampling from the box, this violates the random sampling assumption: the experimenter could pick and choose which balls she wanted to draw from the population box. This condition thus provided a measure of infants’ approach and exploration behaviors for the two boxes when the assumption of random sampling does not hold. Again, the Control condition also ensured that any difference found in the Sampling condition was not due to an intrinsic preference for a certain type of sequence/box.

Coding. All of the infants’ behaviors were coded offline. We had two measures: touching the boxes, and reaching into the boxes. The touching measure was operationalized as the amount of time that an infant’s either or both hands were in contact with each of the two boxes. Times in which an infant was in contact with the boxes through her body or her legs were not included within this measure. The reaching measure was operationalized as the amount of time that an infant’s hand was inserted into one of the openings present on the surfaces of the two boxes, such that the hand is no longer fully visible. Note that the two measures were not mutually exclusive. The video coding was performed using Datavyu (Datavyu Team, 2014).

3.3.2 Results

A second observer coded 50% of the infants’ videos offline. Reliability was calculated as the proportion of total time both observers agreed that infants were touching/reaching into either the Uniform box or the Variable box. Thus, percentage agreement = 1 – [(absolute difference in time between original and second coder)/original coder]. Interscorer reliability averaged 93% for the touching measure, and 96% for the reaching measure. Preliminary analyses found no effect of gender on the two measures; subsequent analyses were collapsed over this variable.

We first analyzed the time infants spent touching the boxes by performing a 2 (Condition: Sampling vs. Control) x 2 (Demonstration Order: Uniform box first vs. Variable box first) x 2 (Box Type: Uniform vs. Variable) ANOVA with repeated-measures on the last factor. As hypothesized, the analysis yielded a significant interaction between Condition and Box Type for the amount of time infants spent touching the boxes, $F(1, 36) = 28.72, p < .001, \eta^2_p = .44$. No other interactions or main effects were found.

Follow-up t-tests to analyze the significant interaction revealed that infants in the Sampling condition touched the Uniform box ($M = 29.52s, SD = 25.44$) significantly longer than the Variable box ($M = 5.72s, SD = 10.47$) at test, $t(19) = 3.33, p = .004, d = .76$ (Figure 3.3). The result of a Wilcoxon signed-rank test was significant, $z = 2.46, p = .014$. Fourteen out of 20 infants preferred touching the Uniform box to the Variable box, while the remaining 6 infants preferred touching the Variable box. In contrast, infants in the Control condition touched the Variable box ($M = 32.57s, SD = 20.68$) significantly longer than the Uniform box ($M = 8.52s, SD = 9.65$), $t(19) = 4.05, p = .001, d = .92$. A Wilcoxon signed-rank test also showed a significant difference between the amount of time infants in this condition touched the two boxes, $z = 3.02,$
15 infants out of 20 infants in this condition preferred touching the Variable box, while the remaining five infants preferred the Uniform box.

Furthermore, there was no difference found in the overall amount of time that infants spent touching the two boxes between conditions (Sampling condition: $M = 35.25s$, $SD = 22.18$; Control condition: $M = 41.09s$, $SD = 18.35$), $t(38) = .91, p = .37$.

**Figure 3.3.** Mean amount of time spent touching the Variable box and the Uniform box in the Sampling condition and the Control condition. Error bars represent standard error.

We next analyzed the time infants spent reaching into the boxes by performing a 2 (Condition) x 2 (Demonstration Order) x 2 (Box Type) ANOVA with repeated-measures on the last factor. The analysis yield a significant interaction between Condition and Box Type, $F(1, 36) = 15.14, p < .001, \eta_p^2 = .3$. There were no other interactions or main effects.

Follow-up analyses on the amount of time infants spent reaching into the boxes revealed that infants in the Sampling condition spent a significantly longer time reaching into the Uniform box ($M = 10.96s$, $SD = 15.04$) than the Variable box ($M = 0.70s$, $SD = 2.24$) during the 60s test period, $t(19) = 2.93, p = .009, d = .76$. The result of a Wilcoxon signed-rank test was significant, $z = 2.51, p = .012$. Ten out of 20 infants preferred reaching into the Uniform box than into the Variable box, while only two infants preferred reaching into the Variable box over the Uniform box. The remaining 8 infants did not reach into either boxes. According to these reaching times, 37.12% of the time that these infants were in physical contact with the Uniform box was spent reaching into it, while only 12.23% of the time that they were in physical contact with the Variable box was spent reaching into it.

In contrast, infants in the Control condition reached into the Variable box ($M = 8.97s$, $SD = 13.86$) for a significantly longer time than the Uniform box ($M = 1.52s$, $SD = 3.45$), $t(19) = 2.35, p = .03, d = .59$. The Wilcoxon signed-rank test showed a significant difference in the
amount of time infants spent reaching into the two boxes, \( z = 2.29, p = .022 \). Eleven out of the 20 infants preferred reaching into the Variable box over the Uniform box, while only three infants preferred reaching into the Uniform box than into the Variable box. The remaining 6 infants did not reach inside of either boxes. These infants spent 27.54% of the time that they were in contact with the Variable box on reaching inside of it, but only spent 8.22% of the time that they were in contact with the Uniform box on reaching inside it. (See Figure 3.4 for mean reaching times).

![Figure 3.4](image)

**Figure 3.4.** Mean amount of time spent reaching into the Variable box and the Uniform box in the Sampling condition and the Control condition. Error bars represent standard error.

### 3.3.3 Discussion

Infants in the Sampling condition preferentially explored the box that was the source of an unexpected event. Not only did they touch the Uniform box for a longer time than the Variable box, they also spent more time reaching inside of it, which is a behavior that could yield information about the mechanics of the sampling process. These results correspond with the findings in Experiment 1, demonstrating that infants selectively approach and explore sources that violate their expectations. The pattern of exploration behaviors was reversed in the Control condition, demonstrating it was the presentation of a random sampling process that led infants to preferentially approach and explore the Uniform box; infants did not show an intrinsic preference for a box generating a uniform sequence.

### 3.4 General Discussion

Using a probability task over two experiments, we demonstrate that infants preferentially approach and explore sources of unexpected events. In Experiment 1, 13-month-old infants saw balls ostensibly being randomly sampled from a population of 6 different-colored balls, creating
an expectation that different-colored balls should be produced. Infants looked longer when the box generated four identical balls, establishing that infants share adult intuitions towards such events: under conditions of random sampling, a uniform sequence of ball tosses is more unexpected than a variable one. Experiment 2 demonstrated that infants spent more time touching and reaching into a box that generated a uniform sequence instead of a variable sequence. Thus, the selectivity observed in infant looking time in VOE experiments is similarly observed in infant approach behaviors in action tasks.

Our findings replicate and extend recent results by Stahl and Feigenson (2015), demonstrating that when infants observe events that defy their expectations, they show preferential approach and exploration – they are drawn towards the source of the anomaly. This selectivity in exploration is not limited to domains of core knowledge or only to events that are impossible. The consequences of observing an expectancy violation may apply across many different domains of knowledge, placing infants in good stead to learn about different aspects of their world.

It is interesting that we also found a strong correspondence between the Control conditions in the two experiments: 13-month-olds looked longer at the variable sequence in Experiment 1, and they also spent more time with the Variable box in Experiment 2. We speculate that other factors, such as novelty and perceptual salience, may drive infants’ looking and exploration patterns as well. This explanation is consistent with the well-established finding that many animals, including children, selectively explore novel stimuli (Berlyne, 1966; Dember & Earl, 1957; Henderson & Moore, 1980; Hutt & Bhavnani, 1972).

The current work also makes a novel contribution to the field of research examining infants through the use of action tasks. Previous studies have largely focused on tasks that provide converging evidence for early competencies demonstrated in VOE experiments. For example, researchers show that infants reach towards helpful characters (Hamlin et al., 2008), or containers/occluders that should contain a retrievable toy (Hespos & Baillargeon, 2006, 2008). They also crawl towards a bucket that has a greater amount of crackers (Feigenson et al., 2002) or a jar that has a higher probability of obtaining a preferred lollipop (Denison & Xu, 2010, 2014). Although these tasks involve approach, they do not shed light on the downstream consequences of observing an expectancy violation. Our results indicate that infants spontaneously explore sources that violate their expectations, potentially providing themselves with new learning opportunities. This claim is corroborated by several recent studies demonstrating the children can actively influence their own learning outcomes by allocating their attention in systematic ways (Cook et al., 2011; Gerken et al., 2011; Kidd et al., 2012; Schulz & Bonawitz, 2007).

In summary, our experiments provide strong evidence that infants preferentially explore sources of unexpected events. Future work is necessary to examine the information that infants might be gathering from their physical exploration, and how such evidence might be incorporated into their knowledge base. This line of research may shed light on how infants play an active role in driving their own development, providing new insights for characterizing the learning infant.
Chapter 4

Young Children Benefit from Self-Directed Learning

4.1 Introduction

“Don’t pinch so hard on her cheeks! You have to be gentle with your baby sister!” Adults often tell children what to do (and what not to do), but the instructions they give can be quite opaque at times. In this example, how should a child learn to draw the line between pinching the baby sister on the cheeks “gently” vs. “too hard”? The child may observe how other people do it: gently pinching the baby’s cheeks results in delightful giggles but pinching too hard results in crying in distress. Alternatively, she may want to collect more data herself and figure out where to draw the line.

Children use a number of different strategies to learn about the world. As seen from the example above, there are at least two modes of learning that people engage in to refine their beliefs: reception learning, in which learners use the data that is directly provided to them; and selection learning, in which learners are allowed to make decisions about the data they wish to experience in order to accomplish some learning goal (Bruner, Goodnow, & Austin, 1956; Bruner, 1961; Gureckis & Markant, 2012; Markant & Gureckis, 2013).

It is indubitable that children sometimes generate data on their own: they tinker with toys, point to things to ask for more information, query adults about words they do not understand, and spontaneously ask questions from an early age (Chouinard et al., 2007; Frazier et al., 2009; Mosher & Hornsby, 1966). Such forms of selection learning, also known as active learning, have been lauded in education for many years, but work on this topic rarely takes the form of controlled experiments (e.g., Bonwell & Eison, 1991; Bruner et al., 1956; Chi, 2009; Freeman et al., 2014). While developmental researchers have made some inroads by demonstrating that young children may be able to gather data in a non-random manner (Cook et al., 2011; Kidd et al., 2012; Legare, Mills, Souza, Plummer, & Yasskin, 2013; Nelson, Divjak, Gudmundsdottir, Martignon, & Meder, 2014; Schulz & Bonawitz, 2007; Sim & Xu, 2014), these studies have not directly compared different learning conditions to see if they result in different learning outcomes. Consequently, it remains unclear whether children can systematically generate their own data in order to achieve a learning goal, and whether there are benefits associated with selection learning compared with reception learning. For example, can children generate useful data iteratively by focusing on their own uncertainty over time? Does allowing a child to select her own sequence of observations convey a learning benefit that is beyond simply providing her
with the very same sequence of data? Addressing such questions would provide insights into the developmental origins of selection learning and its underlying mechanisms.

Recent research has shown that in category-learning tasks, adults indeed benefit from selection learning compared with reception learning (Castro, Kalish, & Nowak, 2008; Markant & Gureckis, 2013). In Markant and Gureckis (2013), adults were presented with “loop antennas” that varied in their angle and size. They were told that these antennas receive either Channel 1 or 2, and their task was to learn to discriminate between the two types of antennas. The critical contrast was between two learning conditions: Selection condition and Yoked Reception condition. In the Selection condition, participants were allowed to design antennas to test which channel they received, and in the Yoked Reception condition, participants were each presented with a sequence of antennas designed by a matched participant in the Selection condition. In a subsequent classification test, selection learners were more accurate as compared to the yoked reception learners. That is, even though both groups observed identical sequences of data, selection learners showed a clear advantage in achieving the learning goal. Additional analyses suggested that learners benefit from selection learning because they can gather data in a non-random, systematic way: the items sampled were often close to the true category boundary. By using such a strategy, selection learners avoid generating redundant information, thus maximizing their own future learning. In contrast, observing data that were useful for another person’s learning did not result in any learning advantage, as was shown by the participants in the Yoked Reception condition (Markant & Gureckis, 2013).

In the current study, we examine whether children perform better in a category-learning task when they can select the data they wish to learn from, as compared to when they receive data that is provided to them. Our experiments were inspired by methods that have only been used with adult participants thus far (Castro et al., 2007; Markant & Gureckis, 2013), so we tested 5- and 7-year-old children because previous work have indicated some success within these age ranges for related tasks, e.g., asking questions (Legare et al., 2013) and designing informative interventions (Cook et al., 2011). In two experiments, children were presented with a row of items that were ascending in size. These items were associated with two different categories (see Figure 4.1). The category that each item belonged to depended on the size of the item, so the goal of the game was to figure out where the category boundary was. The optimal selection strategy in this task is a deterministic binary bisection strategy, such that one should always select the item that is at the midpoint of one’s region of uncertainty. For example, when presented with a row of 15 items arranged in ascending order by size, if a learner knows that Item 3 is the leftmost item that belongs to Category X within the row, and Item 11 is the rightmost item that belongs to Category Y within the row, then the optimal strategy would be to select the item exactly in the middle (Item 7) to learn whether it belongs to Category X or Y. By employing such a strategy iteratively, one’s uncertainty decreases at an exponential rate since the region of uncertainty is being halved at each step (Castro et al., 2008).

In our experiments, children were randomly assigned to one of two conditions: selection, where each child chose sequentially which items to learn about, or yoked reception, where each child was presented with a sequence of items that was previously selected by a participant in the selection condition. The use of a yoked reception condition rather than a random reception condition (i.e., where observations are randomly generated, e.g. Castro et al., 2008) ensured that
the sequence of observations were identical between matched pairs, thereby controlling for the effect that the quality of observations might have on children’s category learning. We hypothesized that (1) children can learn successfully under conditions of selection, (2) they can gather data in a systematic manner, and (3) selection learning has distinct advantages over reception learning.

Each experiment consisted of 4 test blocks. Within each block, children learned about 2 items and were then given a classification task. The use of multiple test blocks allowed us to examine the children’s learning performance and their information gathering strategy over time.

### 4.2 Experiment 1A: Self-Directed Learning in 7-year-olds

#### 4.2.1 Method

**Participants.** Forty-eight English-speaking 7-year-olds (22 boys and 26 girls) with a mean age of 90.8 months (range = 71.6 to 107.8 months) were tested. All were recruited from schools and children’s museums in Berkeley, California, and its surrounding communities. An additional 7 children were tested but excluded due to difficulties in following task instructions (N = 3), technical error (N = 3), and experimenter error (N = 1).

**Materials.** The experiment was presented in the form of an interactive PowerPoint presentation, with each presentation sequentially showing 3 sets of animals. The first two sets of animals were for demonstration purposes and consisted of 13 identical animal images that varied only in their size (i.e. heights and widths). The third set of animal was used at test, and consisted of 15 identical animal images that varied only in their size.

The animals were all arranged from smallest to largest (left to right). They lived in either a green farm or a blue farm, and these farms were represented by colored images placed on the top left and top right of the screen respectively (Figure 4.1). When an animal image was clicked, it would move across the screen towards its designated farm, disappearing upon arrival.

**Procedure.** Children were tested individually in our laboratory, a quiet room in their elementary school, or in a quiet area at a museum. Each child was assigned to a Selection condition or a Yoked Reception condition. Children in the Yoked Reception condition were individually matched to a randomly selected child in the Selection condition.

An experimenter sat next to the child to control the slide show. The procedure for both the Selection and Yoked Reception conditions consisted of a demonstration phase, a cueing phase, and 4 test blocks. Each test block consisted of two sampling trials, followed by a classification test. The experiment lasted about 10 minutes.

**Demonstration Phase.** The demonstration phase consisted of two practice trials. These practice trials were to establish to the child that (1) the displayed animals lived in one of the two farms, (2) the farm that each animal lived in was determined by an invisible category boundary that divided the animals into two groups, and (3) that the location of the boundary was different for each set of animals.

In the first practice trial, the participant was shown a row of 13 spiders that increased in size, together with a green farm and a blue farm placed at the top corners of the screen. When the experimenter clicked on each house, a flashing box appeared around the spiders that lived in the
selected farm. The experimenter subsequently pointed at two spiders, one at a time, asking the child “Does this spider live in the green farm or the blue farm?” The experimenter praised the child if he/she answered accurately (“Good job!”), and corrected the child otherwise (“No, that spider actually lives in the green farm!”). The second practice trial that followed was identical to the first, except that we used a row of frogs instead, and a new category boundary.

**Figure 4.1.** Fifteen pigs that can be categorized as living in the green farm or the blue farm.

**Cueing Phase.** After the two practice trials, children were presented with a row of 15 pigs. The experimenter then informed the child that she will have to figure out which farm each pig lived in. Fourteen category boundaries were possible, but only the 3rd to the 12th boundary were used in this experiment. This step was taken to ensure that there were at least a small number of pigs that lived in each farm. The child was unaware of this stipulation. For each participant, a boundary location was randomly generated, and this location was used for all test blocks. For example, if the 5th boundary was generated, pigs 1 – 5 lived in the green farm and pigs 6 – 15 lived on the blue farm for all test blocks.

An image of a storm then appeared. Each child was shown that the 1st pig and the 15th pig got scared and ran back to the green farm and the blue farm respectively. For all trials, the smallest and the biggest pigs always lived in the farm closest to them. The storm then cleared out quickly and the sun came up, so all the pigs reappeared. The children were reminded that these were the same pigs as before.

**Test Block.** Each test block consisted of two sampling trials and a classification test. In the sampling trials, children in the Selection condition selected which pigs to query, whereas children in the Yoked Reception condition received information on the pigs that were previously selected by a matched participant in the Selection condition. In the subsequent classification test, children were asked to classify the pigs as green-farm-dwellers or blue-farm dwellers. Every child completed a total of 4 test blocks.

**Test Block: Sampling Trials.** The sampling phase of each test block consisted of two sampling trials. To begin the sampling phase, an image of a storm appeared once again. In the Selection condition, the experimenter told the child that there was only time to tap on one pig,
and asked the child to choose one pig to “figure out where each pig lives”. The experimenter then clicked on the chosen pig, which moved to its associated farm as determined by the pre-set category boundary. Then, the storm cleared, so all the pigs reappeared. A slightly modified cover story regarding bad weather was repeated (e.g., some strong winds), and the child was allowed to choose a second pig to learn about. Note that all previously chosen pigs continued to be available as options. The key feature in the Selection condition was thus that the child was allowed to independently generate data about the pigs in order to learn about the category structure.

In the Yoked Reception condition, a PowerPoint presentation was prepared such that each child was shown a sequence of observations that was previously chosen by a child in the Selection condition. Within each test block, children in the Yoked Reception condition thus observed two pigs returning to their associated farms. The children were not told that these pigs had been selected by other children; instead, they were told that these pigs got scared off by the storm and ran back to their farm. To ensure that the children were able to track the provided information, each pig wiggled for 2 seconds before moving to the farm it lived in. The key feature in the Yoked Reception condition was therefore that the child could only observe, but not generate, data about the pigs to learn about their category structure. In addition, the sequence of observations viewed by each child in this condition was exactly matched to another child in the Selection condition.

**Test Block: Classification Test.** Immediately after the second pig within each test block moved to its associated farm, the children entered a classification phase. The experimenter first informed the child that the storm was almost here, so they had to take the rest of the pigs back to their farms. The child was then asked to point to all the pigs that lived in the green farm, as well as all the pigs that lived in the blue farm. If the child skipped the classification of some pigs, the experimenter pointed to each of these skipped pigs and asked, “Which farm does this pig live in?” The children’s answers allowed us to determine where they believed the boundary was located. After all the pigs had been classified, they disappeared and the experimenter told the child, “Phew, all the pigs are safe! But we don’t know if they went to their correct farms.” This instruction was given to remind the children that although the pigs did not meet with any harm, they may not have been classified into their respective farms correctly. The sun then came up and the same set of pigs reappeared, thus transitioning the child into a new test block.

**Coding.** In the Selection condition, we recorded the pigs that each child selected over the four test blocks (a total of 8 samples). We computed two scores for each sampling trial and one score for each test block. First, we measured the sampling distance, i.e., the distance between each of their selections and the true (pre-determined) category boundary. For example, if the child selected a pig that was adjacent to the category boundary (left or right), the sampling distance was 0. The sampling distances allowed us to examine how children were sampling across time. Second, these sampling distances were converted to bisection distances, i.e., the distance between the child’s selection and the optimal selection as determined by a binary bisection strategy within each sampling trial. Each child was then scored according to their average bisection distance over the 8 sampling trials. This average bisection distance was calculated by dividing the sum of the bisection distances for each of the sampling trials by the total number of trials, thus providing an indicator of how close the child approximated the optimal strategy of binary bisection. Third, for each child, we also obtained a classification
accuracy score for each test block. Each correctly classified pig was scored as 1 point, so the maximum score in each block was 15. The children’s scores were then converted into a percentage of classification accuracy.

In addition, an observer blind to the conditions coded 50% of the videos offline for the child’s overall level of attention on a scale from 1 to 5 (1: Strongly Disagree that child was paying attention; 2: Disagree that child was paying attention, 3: Neutral; 4: Agree that child was paying attention; 5: Strongly Agree that child was paying attention).

4.2.2 Results

The participants were attentive and the overall level of attention did not differ between the two conditions (Selection: $M = 4.92, SD = .29$; Yoked Reception: $M = 4.54, SD = .78$), $t(22) = 1.1, p = .28$. Preliminary analyses found no effects of gender or location of boundary on children’s accuracy on classification trials. Subsequent analyses were collapsed over these variables.

Selection condition: sampling trials. After learners have acquired some data in a category learning task, they can easily classify items that are far from the true category boundary, but are more uncertain about items that are near the boundary. For example, children were shown that the smallest pig lives in the green farm and the largest pig lives in the blue farm. Suppose that the true category boundary lies between the 9th and 10th pig. When a child chooses to click on the 6th pig and discovers that it lives on the green farm, she would be fairly confident that the 2nd pig also lives in the green farm, but far less confident about where the 7th pig lives. Following the analyses in Markant & Gureckis (2013), we thus examined children’s sampling distances, i.e., the distance between the children’s selections and the true category boundary, as a general measure of uncertainty-driven information selection.

As Figure 4.2 indicates, children in the Selection condition sampled closer to the true category boundary over time. Using the children’s average sampling distance over each of the 8 sampling trials, we performed a one-way repeated measures analysis of variance (ANOVA) with Trial (1–8) as a within-subjects factor. The effect of Trial was significant, $F(1, 7) = 3.748, p = .0017$, $\eta^2 = .13$.

We also compared children’s sampling distance with a random sampling strategy. The average sampling distance expected by a random sampling strategy (3.93 items from the category boundary) was computed by averaging the sampling distance of all possible sampling choices for the 10 different category boundaries used in this experiment. Planned comparisons showed that in the 1st trial, the average sampling distance in the Selection condition was significantly larger than expected by a random sampling strategy, $t(23) = 2.31, p = .03, d = .47$, as children often sampled close to the end points in the first sampling trial. The average sampling distance in the last test block was significantly smaller than expected by random-sampling: for the 7th sampling trial, $t(23) = 2.91, p = .008, d = .59$; and for the 8th sampling trial, $t(23) = 3.40, p = .002, d = .69$. All the other sampling distances (2nd through 6th sampling trials) did not differ from using a random-sampling strategy. Post-hoc tests revealed that there was a significant linear trend across all the sampling trials (1-8), $F(1, 23) = 16.938, p < .0001$, using Bonferroni adjusted levels of .007 per test (.05/7).
We also examined children’s bisection distances as an indicator of how well they approximated a binary bisection strategy, which is the optimal strategy in this task. For each child, we computed their average bisection distance by dividing the sum of the bisection distances for each of the sampling trials by the total number of trials. Therefore a child with a smaller score approximated the optimal strategy better than a child with a larger score. Our results showed that the children’s average bisection scores ($M = 3.53$, $SD = 1.34$) were significantly smaller than that expected by a random-sampling strategy ($M = 4.14$), which was obtained by simulating a model using a random-sampling strategy 10,000 times, $t(23) = 2.22, p = .036, d = .45$.

**Selection and Yoked Reception conditions: classification tests.** Figure 4.3 illustrates children’s classification accuracy within each test block. Chance performance ($M = 0.71$) was computed by averaging the classification accuracy associated with all possible boundary location guesses for the 10 different category boundaries used in this experiment. In Test Blocks 1 (Selection: $M = .81$, $SD = .13$; Yoked Reception: $M = .84$, $SD = .12$) and 2 (Selection: $M = .83$, $SD = .12$; Yoked Reception: $M = .82$, $SD = .14$), children in both conditions classified the pigs at a rate significantly better than chance, all $ps < .001$. While children’s classification accuracy remained above chance for Test Blocks 3 (Selection: $M = .89$, $SD = .12$; Yoked Reception: $M = .82$, $SD = .15$) and 4, (Selection: $M = .92$, $SD = .08$; Yoked Reception: $M = .83$, $SD = .13$), the two conditions began to diverge such that by Test Block 3, children in the Selection condition were performing marginally better than children in the Yoked Reception condition, $t(46) = 1.73$, $p = .09$. 
Using children’s average classification accuracy across the four test blocks, we then performed a 2x4 repeated measures analysis of variance (ANOVA) with Condition (Selection vs. Yoked Reception) as a between-subjects factor and Test Block (1–4) as a within-subjects factor. There was a main effect of Test Block, $F(3, 138) = 4.70, p = .0037, \eta^2 = .093$, as well as a significant interaction, $F(3, 138) = 6.07, p < .001, \eta^2 = .12$. No other main effects were found.

The presence of a significant interaction in the omnibus test suggests that the rate of learning differed between the two conditions. Post-hoc tests revealed that there was a significant linear trend ($F(1, 23) = 21.17, p < .001$) for the Selection condition across the four test blocks, indicating that children’s classification accuracy increased over time. In contrast, this trend was not found in the Yoked Reception condition, $F(1, 23) = .75, p = .79$; classification accuracy remained flat across the four test blocks.

We further examined the relationship between average bisection scores and classification accuracy. We found that the average bisection scores of children in the Selection condition was negatively correlated with their final classification accuracy, $r = -.46, p = .025$. That is, the more a child used the optimal bisection strategy (which resulted in a smaller average bisection distance score), the better she performed on the classification test. In contrast, this correlation was not found between the bisection scores and the final classification accuracy of the children in the Yoked Reception condition, $r = .029, p = .89$, even though the sequence of observations were identical between matched pairs.

**Figure 4.3.** Classification accuracy in the Selection (solid lines) and Yoked Reception (dashed lines) conditions. Dotted line indicates chance performance. Error bars show standard error.

### 4.2.3 Discussion

In Experiment 1, we found that 7-year-old children learned successfully under conditions of selection: they gathered data systematically by sampling closer to the true category boundary over time, and their classification accuracy in the Selection condition was significantly higher
than chance for each of the four test blocks. More strikingly, even though the two groups of children learnt about an identical set of items, the final classification accuracy in the Selection condition was reliably higher than that of the Yoked Reception condition, indicating that selection learning may have distinct advantages over reception learning.

Our results also show a clear relationship between children’s ability to approximate the optimal binary bisection strategy and classification performance: children who, on average, had lower bisection distances (i.e., closer to using the optimal strategy) performed better at classification at the end of the experiment. However, simply viewing the data generated by children who better approximated the binary bisection strategy had no bearing on classification accuracy: there was no correlation between bisection distance and final classification accuracy in the Yoked Reception condition.

In addition, overall attention level did not differ between the two conditions. Children appeared to be equally engaged in the task, yet selecting one’s own data based on the learner’s current hypothesized category boundary conferred a clear advantage for learning.

Given the success of 7-year-olds in this category learning task under conditions of selection, we were interested to examine whether younger children would also succeed on such a task. Our pilot testing revealed that 5-year-old children had a lot of difficulty in staying engaged with the procedure of Experiment 1, partly because they were not allowed to activate any of the animations by themselves and partly because they did not find the cover story compelling. Some children also had difficulties understanding the goal of the game even with the demonstration trials. We thus modified the procedure for Experiment 2, creating a card game instead, so that the 5-year-olds would be able to physically manipulate the stimuli involved in the experiment. We also modified the demonstration phase to include a pre-test, and the cover story to include predator and prey animals so children would be more motivated to put a boundary between them. Lastly, to reduce processing load, we used only 11 animals, instead of 15, in Experiment 2.

4.3 Experiment 2A: Self-Directed Learning in 5-year-olds

4.3.1 Method

Participants. Forty-eight English-speaking 5-year-olds (23 boys and 25 girls) with a mean age of 60.4 months (range = 45.9 to 70.9 months) were tested. All were recruited from schools and children’s museums in Berkeley, California, and its surrounding communities. An additional 9 children were tested but excluded due to failing the pre-test (N = 6), technical error (N = 2), and experimenter error (N = 1).

Materials. Experiment 2 was presented using laminated cards (1 inch x 1 inch), with each card displaying a house on its front side and an animal that lived in the house on its back side. There were four sets of cards. Each set consisted of 11 houses of varying sizes arranged from smallest to largest, among which smaller houses had prey animals living within (e.g., mice), and larger houses had predator animals living within (e.g., cats). Three sets of these cards were used in the demonstration phase (i.e., squirrels and dogs; worms and birds; butterflies and frogs). The houses displayed on these demonstration cards had an additional item to cue children as to the animal that lived within it without having to flip over the card (e.g., an acorn was used
to cue that a squirrel lived within; a bone was used to cue that a dog lived within). The last set of cards was used in the Testing Phase and did not contain such cues on the front side of the cards.

During the experiment, the cards were placed on a piece of green fabric with squares that were pre-drawn for positioning purposes. Children were asked to sequentially use four small fences (1.5 inch x 1 inch) of different colors to separate the prey from their predators. A small laptop was also used to play two sound effects: (1) a rooster crowing, representing that the rooster has woken up, and (2) crickets chirping, representing that the rooster is still asleep.

**Procedure.** As in Experiment 1, each child was assigned to a Selection condition or a Yoked Reception condition. Children in the Yoked Reception condition were individually matched to a randomly selected child in the Selection condition. The procedure for both conditions consisted of a demonstration phase and 4 test blocks. Each test block consisted of two sampling trials, followed by a classification test. The experiment lasted about 10 minutes.

**Demonstration Phase.** The demonstration phase of Experiment 2 consisted of two demonstration trials and a pre-test. The demonstration trials established that (1) there were two kinds of animals that lived within these houses that were ascending in size, (2) the prey and the predators were divided by an invisible category boundary based on the size of the houses, (3) the location of the boundary was different for each set of houses, and (4) the objective of the game was to place a fence right at the category boundary in order to prevent the predators from chasing the prey when the rooster’s crow wakes all the animals up.

In the first demonstration trial, the experimenter laid out the first set of cards and used the cues (i.e., acorns and bones) displayed beside the picture of each house to indicate the animal that lived within it. She explained that the animals (i.e., squirrels and dogs) were sleeping now, but when a rooster crows, all the animals would wake up, and the dogs would chase the squirrels. To keep the animals safe, she then placed a fence at the boundary between where the squirrels and dogs lived. She subsequently activated a sound effect of a rooster crowing, exclaiming that the rooster had woken up all the animals. The experimenter worked with the child to flip all the cards over, revealing that the fence had been effective at separating the prey from the predators. The second demonstration trial that followed was identical to the first, except that the animals involved were worms and birds, and the category boundary was at a different location.

After the two demonstration trials, children participated in a pre-test to determine whether they understood the objective and instructions of the game. The experimenter laid out the third set of cards (i.e., butterflies and frogs) and asked the child to place the fence to separate the two groups of animals. Note that the participants were not allowed to flip over the cards before placing the fence – the cues displayed on the front side of the cards were sufficient to determine where the category boundary was. Children who placed the fence correctly then proceeded into the Test Blocks.

**Test Block.** After the demonstration phase, children were presented with a new set of 11 cards (See Figure 4.4). Unlike those used in the demonstration phase, the front side of these cards displayed only houses, with no additional cues as to the animal that lived within each house. The experimenter highlighted the absence of cues and informed children that “cats live in the big houses and mice live in the small houses.” The children were also reminded that they were to put a fence between where the cats and mice live before the rooster crows.
10 category boundaries were possible, but only the 3rd, 6th and 8th boundary were used in this experiment to simplify the role of the experimenter. Children were unaware of this stipulation, and each child was randomly assigned to one of the three boundaries to be used for all test blocks. Each test block consisted of two sampling trials and a classification test. In the Selection condition, children selected which cards to flip over, whereas in the Yoked Reception condition, children were asked to flip over a pre-determined sequence of cards.

**Figure 4.4.** A child flipping a card over to reveal the cat that lives within it.

*Test Block: Sampling Trials.* The sampling phase of each test block consisted of two sampling trials. In the Selection condition, the experimenter explained that the child could look into the houses to figure out where the fence should be placed. However, every time that they looked into a house, they would make some noise that might wake up the rooster. As such, the child was encouraged to look inside as few houses as possible. The child was then asked to choose a house to look inside of. They flipped over the card independently, revealing the animal that lived within it as determined by the pre-set category boundary. The experimenter then reset the card, and the child was allowed to choose a second house to flip over. As in Experiment 1, children in the Selection condition were allowed to independently generate data about the houses in order to learn where the prey and predators lived.

In the Yoked Reception condition, the experimenter explained that “Sometimes, the animals get thirsty and go outside to get water. We can watch to see if any of the animals come out!” After a 3 second pause, the experimenter pointed to a specific card and said, “Oh look, this animal got thirsty and came out.” The child was asked to flip that card over to reveal the animal that lived within. The experimenter then reset the card, and used the same procedure to reveal
information about a second house. Therefore, children in the Yoked Reception condition could only observe, but not generate, data about the animals that lived within the row of houses to learn the associated category structure. Just as in Experiment 1, the sequence of observations viewed by each child in this condition was exactly matched to another child in the Selection condition.

**Test Block: Classification Test.** After the sampling phase of each test block, the children entered a classification phase. The experimenter asked the child, “Where should we put the fence in case the rooster wakes up?” After the child placed the fence, the experimenter and the child listened as to whether the rooster was awake. A sound effect of crickets chirping was then activated, representing that the rooster had not awoken yet. The experimenter then informed the child about a big gust of wind coming by, causing the fence to break. The experimenter removed the fence, and proceeded into the next test block. For each of the subsequent test blocks, the experimenter explained that she was able to build a new fence of a different color each time, which was then used for the subsequent classification tests.

**Coding.** The coding was similar to that of Experiment 1. For the sampling trials in the Selection condition, we computed both sampling distance and bisection distance. For all test blocks in both conditions, we computed classification accuracy. In addition, an observer blind to the conditions coded 50% of the videos offline for two attention measures: (1) the child’s overall level of attention, and (2) the duration of look-away, which is the amount of time each child spent not looking at the experimenter or the stimuli involved in the experiment. It was possible to use the latter measure in Experiment 2 as the experiment was done primarily in a lighted room such that the children’s eyes could be seen, in contrast to Experiment 1, where the experiment was done primarily in a dim space in a museum.

### 4.3.2 Results

Children were attentive and the overall level of attention did not differ between the two conditions (Selection: $M = 4.41, SD = .70$; Yoked Reception: $M = 4.63, SD = .64$), $t(22) = .82, p = .42$. The duration of time children spent looking away was also not different between the two conditions (Selection: $M = 19.6s, SD = 22.8$; Yoked Reception: $M = 23.05s, SD = 39.8$), $t(22) = .26, p = .80$. Preliminary analyses also found no effects of gender or location of boundary on children’s accuracy on classification trials. Subsequent analyses were collapsed over these variables.

**Selection Condition: sampling trials.** Replicating the results of Experiment 1, we found that 5-year-old children sampled closer to the true category boundary over time. Using the children’s average sampling distance over each of the 8 sampling trials, we performed a one-way repeated measures analysis of variance (ANOVA) with Trial (1–8) as a within-subjects factor. The effect of Trial was significant, $F(1, 7) = 5.94, p < .001, \eta^2 = .21$. Planned comparisons showed that the average sampling distance in the Selection condition was significantly larger than expected by a random sampling strategy ($M = 2.64$) in the 1st trial, $t(23) = 2.58, p = .017, d = .53$. In contrast, the average sampling distance in the last test block was significantly smaller than expected by random-sampling: for the 7th sampling trial, $t(23) = 2.71, p = .012, d = .55$; and for the 8th sampling trial, $t(23) = 4.04, p = .001, d = .52$. All the other sampling distances (2nd through 6th sampling trials) did not differ from using a random-sampling strategy. Post-hoc tests
revealed that there was a significant linear trend across the sampling trials (1-8), $F(1, 23) = 23.89, p < .0001$, using Bonferroni adjusted levels of .007 per test (.05/7).

The average bisection scores of children in Experiment 2 ($M = 2.27, SD = .80$) was also significantly smaller than expected by a random-sampling strategy ($M = 2.84, t(23) = 3.50, p = .0019, d = .72$).

**Figure 4.5.** Classification accuracy in the Selection (solid lines) and Yoked Reception (dashed lines) conditions. Dotted line indicates chance performance. Error bars show standard error.

**Selection Condition and Yoked Reception Condition: classification tests.** In Test Blocks 1 (Selection: $M = .80, SD = .17$; Yoked Reception: $M = .83, SD = .15$) and 2 (Selection: $M = .86, SD = .15$; Yoked Reception: $M = .84, SD = .17$), the five-year-olds in both conditions did not classify the animals at a rate significantly better than chance ($0.81$), all $p$s $> .05$, contrary to the results of Experiment 1 (Figure 4.5). However, by Test Block 3, only children in the Selection condition ($M = .91, SD = .10$) performed significantly better than chance, $t(23) = 5.19, p < .001$, while children in the Yoked Reception condition ($M = .84, SD = .15$) did not, $t(23) = 1.81, p = .083$. This asymmetry was also found in Test Block 4 (Selection: $M = .92, SD = .11$, Yoked Reception: $M = .84, SD = .15$), $t(23) = 5.15, p < .001$ for the Selection condition, and $t(23) = 1.62, p = .12$ for the Yoked Reception condition. By Test Block 4, the classification accuracy in the Selection condition was also significantly better than that in the Yoked Reception condition, $t(46) = 2.07, p = .045$.

Using children’s average classification accuracy across the four test blocks, we then performed a 2x4 repeated measures analysis of variance (ANOVA) with Condition (Selection vs. Yoked Reception) as a between-subjects factor and Test Block (1–4) as a within-subjects factor. There was a main effect of Test Block, $F(3, 138) = 4.83, p = .0031$. No other main effects or interaction was found. Planned comparisons also revealed that there was a significant linear trend ($F(1, 23) = 9.79, p = .005$) for the Selection condition across the four test blocks, indicating that children’s classification accuracy increased over time. In contrast, this trend was not found in the Yoked Reception condition, $F(1, 23) = .16, p = .70$; classification accuracy remained flat across the four test blocks.
A further examination of the relationship between average bisection scores and classification accuracy revealed that the scores of children in the Selection condition was negatively correlated with their final classification accuracy, \( r = -0.49, p = .014 \). That is, the closer a child’s selection was compared to the optimal selection, the better she performed on the classification test. However, this correlation was once again not found between the bisection scores and the final classification accuracy of the children in the Yoked Reception condition, \( r = -0.29, p = .17 \), even though the sequence of observations were identical between matched pairs.

### 4.3.3 Discussion

Our results in Experiment 2 with 5-year-old children were similar to those obtained in Experiment 1. Just like the 7-year-old children, the 5-year-olds gathered data in a systematic manner, and their final classification accuracy in the Selection condition was significantly higher than chance. Experiment 2 also replicated the selection advantage found in Experiment 1: the final classification accuracy in the Selection condition was reliably higher than that of the Yoked Reception condition. One important difference between the two age groups appears to be the rate of self-directed learning: 7-year-olds in the Selection condition were capable of learning from their selections from the very start, such that their classification accuracy was significantly higher than chance for all four test blocks, while the 5-year-olds in the Selection condition only performed better than chance after having made 6 selections (by the 3rd test block). Learning in the Yoked Reception condition also appeared to differ for the two age groups: 7-year-olds learned successfully based on the data provided directly to them, while the 5-year-olds’ classification performance remained at chance levels throughout the entire experiment.

Furthermore, we again found a clear relationship between the children’s ability to approximate the binary bisection strategy and classification performance in the Selection condition, which was not found in the Yoked Reception condition.

### 4.4 General Discussion

The present study examined whether 5- and 7-year-old children can engage in and benefit from selection learning. Using a category learning task, we demonstrate that children as young as 5 can learn successfully under conditions of selection. We also found that they can gather data in a systematic manner, and that selection learning has distinct advantages over reception learning: in a 10-minute study, selection showed an 8 – 10% learning advantage over yoked reception.

More specifically, our results indicate that children can learn successfully when they are allowed to make decisions about what information they wish to gather. The final classification accuracies of 5- and 7-year-old children in the Selection conditions were significantly higher than chance, suggesting that children are capable of learning from data that they generate by themselves. We also found a developmental difference: 7-year-olds in the Selection condition were capable of learning from their own selections from the very start, such that their classification accuracy was significantly higher than chance for all four test blocks, while 5-year-olds only performed better than chance after having made 6 selections (by the 3rd Test Block).
More impressively, we also found that children are able to gather data in a systematic way. As our results show, children sampled closer to the true category boundary over time. On average, they also bisected their current region of uncertainty more often than expected by chance. Thus the children’s information gathering was informed by uncertainty and feedback, leading them to sample items that were near the true category boundary. Such a strategy allows children to avoid generating redundant information, and to proceed efficiently in collecting data that is expected to help them learn effectively.

Most strikingly, children showed better learning under conditions of selection as compared to yoked reception over time. In both experiments, the final classification accuracy obtained by children in the Selection condition was reliably higher than that in the Yoked Reception condition, despite the fact that the matched pairs viewed identical sequences of data. The rate of learning also differed: Children’s classification accuracy in the Selection condition increased over time, whereas the classification accuracy for children in the Yoked Reception condition did not.

Another developmental difference between the groups is that yoked reception appeared to be the most detrimental to category learning for 5-year-old children. Although these 5-year-olds were presented with data directly, they performed at chance levels for the classification tests throughout the entire experiment. This performance was unlike that of 7-year-olds in the Yoked Reception condition who successfully learned to classify the items, albeit not as well as the 7-year-olds in the Selection condition.

Together, these findings suggest that children benefit from selection learning because they are able to gather data that optimally and systematically addresses their own regions of uncertainty. According to Markant and Gureckis (2013), learners are biased towards collecting data that specifically tests their own hypotheses (i.e., hypothesis-dependent sampling bias). As such, when selection learners are given the opportunity to collect their own information, the training experience that results is one that is uniquely optimized to that particular learner, whereas the same training experience would be a poor match to the yoked partner’s mental state. This asymmetry in the usefulness of the training experience may account for the differences observed in children’s category learning in the two conditions.

A number of recent developmental studies have shown the systematicity of children’s exploration and attentional strategies, but none have shown that these strategies have any consequences on children’s learning (Cook et al., 2011; Kidd et al., 2012; Legare et al., 2013; Nelson et al., 2014; Schulz & Bonawitz, 2007; Sim & Xu, 2014). The current study thus adds an important piece to the puzzle by demonstrating that when given the opportunity, children can gather data in a systematic manner to address their own uncertainty, and they employ this strategy iteratively, depending on feedback. Furthermore, this process is associated with superior performance in category learning, demonstrated by the correlations between the bisection distances and the classification accuracy of the children in the Selection condition, but not with the classification accuracy of matched participants in the Yoked Reception condition.

Our discussion so far offers a cognitive explanation for the selection advantage. Children performed better under conditions of selection because they generated data that was informative for them. One may speculate that there are other relevant psychological processes driving the selection advantage: enhanced memory encoding (Metcalfe & Kornell, 2005); deeper processing
of the problem structure (Sobel & Kushnir, 2006); attention and motivation (Corno & Mandinach, 1983), etc. While our results do not speak directly to the possible memory and processing factors, our additional video coding for both experiments indicates that attention was equivalent among children in the two conditions. This finding suggests that the differences found between the Selection and Yoked Reception conditions were unlikely to be due to attention factors. At the same time, we do not think that any of these factors run contrary to our arguments – after all, those processes could have certainly been recruited when children were deciding which items to learn about.

The current study also revealed a developmental difference with regards to the rate of self-directed learning, in that 7-year-olds appeared to learn successfully from their own selections from the very start, while 5-year-olds only performed better than chance after having made 6 selections (by the 3rd test block). This difference is all the more striking given that the procedure of Experiment 2 was simplified for the purposes of testing younger children. We thus speculate that the capacity for data generation and the ability to learn from self-generated data may change over the first few years of development. Consistent with this hypothesis, results presented in Sim and Xu (2015) indicate that while 3-year-olds were successful in acquiring causal generalizations through free play, 19-month-old infants were unable to do so, except when their play was facilitated by an adult experimenter or a parent. However, we note that the use of these different methods for examining selection/active learning in young children make any comparisons difficult. To more closely examine this developmental trajectory in future studies, one can extend the current method to younger children, or use a causal learning task (a la Sim & Xu, 2015) with older children.

Self-directed learning has been an influential and long-standing debate in education. Our results provide empirical support for the practice of encouraging students to engage in hypothesis testing and self-directed exploration to boost learning (Bonwell & Eison, 1991; Bruner, Jolly, & Sylva, 1976; Chi, 2009; Freeman et al., 2014; Vygotsky, 1967, 1978). The results may also inform specific education practices, particularly in the teaching of concepts that require the parsing of instances into different categories, e.g., positive numbers vs. negative numbers; living things vs. non-living things; or different states of matter (solid, liquid, and gas). In such cases, it may be beneficial for students to be allowed to focus on their own uncertainty over time, as opposed to being didactically given the definitions of specific categories, or having every child receive the same set of exemplars selected by a teacher. In addition to extending the current paradigm to more real life cases of category learning, future research will also investigate whether even younger children benefit from selection learning; whether selection learning confers any long-term advantages on memory and conceptual understanding, and what the limits are on the selection advantage (see Castro et al., 2008 and Markant & Gureckis, 2013).
Chapter 5

Five-year-old children identify the most informative questions

5.1 Introduction

Asking questions is a powerful learning tool. Children ask questions about a variety of topics numerous times a day. In a sample analyzed by Chouinard et al. (2007), 2- to 5-year-olds asked an average of 107 questions per hour while engaged in conversation with adults. Their inquiring behavior is purposeful, intended to resolve a knowledge gap or inconsistency, to seek explanations, and more generally to test and extend their developing understanding of the world (Carey, 1985; Chouinard, 2007; Frazier et al., 2009; Gopnik & Wellman, 2012; Harris, 2012; Piaget, 1954; Wellman, 2011).

Research to date has shown that young children ask domain-appropriate questions (Callanan & Oakes, 1992; Greif, Kemler Nelson, Keil, & Guterrez, 2006; Hickling & Wellman, 2001), have reasonable expectations about which responses count as answers to their questions (Frazier et al., 2009), and can use the answers they receive to solve problems (Chouinard et al., 2007; Legare et al., 2013). We also know that children’s questions are responsive to the statistics of their environment in that they preferentially consult reliable informants (Birch, Vauthier, & Bloom, 2008; Corriveau & Harris, 2009; Koenig & Harris, 2005; Mills, Legare, Bills, & Mejias, 2010; Mills, Legare, Grant, & Landrum, 2011) and privilege more informative cues (Nelson, Divjak, Martignon, Gu mundsdottir, & Meder, 2013).

How sophisticated is this question-asking capacity in young children? Previous studies have examined the development of children’s ability to ask questions by using variations of the Twenty Questions game, in which children have to identify a target object (or kind of objects) within a given set by asking as few questions as possible that can be answered by either “yes” or “no” (see Mosher & Hornsby, 1966; Nelson et al., 2013; Ruggeri & Feufel, 2015; Ruggeri & Lombrozo, 2015). In such studies, researchers measure children’s question-asking ability by analyzing their usage of constraint-seeking vs. hypothesis-scanning questions. Constraint-seeking questions target a feature shared by multiple hypotheses, such as “Was the boy late because of something he could not find?” They stand in contrast to hypothesis-scanning questions, which target a single hypothesis, such as “Was the boy late because his bike was broken?” Researchers have found that 7-year-olds predominantly use hypothesis-scanning questions and that, over development, children transition to using more constraint-seeking questions, until constraint-seeking becomes the dominant strategy in adulthood (Mosher & Hornsby, 1966; Ruggeri & Feufel, 2015; Ruggeri & Lombrozo, 2015).
However, Legare and colleagues (2013) have recently provided empirical evidence that preschoolers as young as 4-years-old are able to generate a majority of constraint-seeking questions. In their study, experimenters presented children with an array of cards with animals of different sizes, colors and patterns. For example, one trial consisted of 16 unique picture cards: 8 of the cards were birds and the other 8 were dogs. Within each set of 8 cards, the animals differed in size (small vs. large), color (red vs. yellow) and pattern (spotted vs. no spots). Children were then told that a duplicate of one of the 16 cards was hidden in a box, and they had to figure out which animal it was by asking yes-or-no questions. Children were also told that they could ask as many yes-or-no questions as they wanted to, but they could only give the solution once (e.g., “Is it the small red spotted dog?”). Their findings indicated that most of the questions generated by even 4-year-olds were constraint-seeking questions, as compared to confirmatory (i.e., questions that are redundant with previous questions) or ineffective questions (i.e., questions that do not discriminate among different options).

However, the study design in Legare et al. (2013) did not allow for a direct comparison between children’s usage of constraint-seeking and hypothesis-scanning questions. As mentioned, children were told that they could only give the solution once, so any hypothesis-scanning question, such as “Is it the small red spotted dog?”, would be construed as the child giving a solution as to which animal was hidden in the special box, leading the experimenter to end the trial after this question. This procedure meant that children could ask at most one hypothesis-scanning question within each trial. As such, while it is clear that even 4-year-olds are able to generate more constraint-seeking questions as compared to confirmatory or ineffective questions, it remains unknown whether 4-year-olds’ generation of questions is better characterized by a constraint-seeking approach or a hypothesis-scanning approach. In addition, the study design also did not allow researchers to examine the quality of constraint-seeking questions that children generated. Every constraint-seeking question that could be generated in this task (i.e., based on type of animal, size, color, or pattern) was equally useful: each question always eliminated half of the cards on the table.

Traditionally, constraint-seeking questions are considered to be more effective than hypothesis-scanning questions because they are able to rule out multiple hypotheses at each step of the search process (Mosher & Hornsby, 1966). However, constraint-seeking questions are not always the most effective – the informativeness of each question type varies depending on the problem being considered, e.g., the number of hypotheses available and their likelihoods (Ruggeri & Lombrozo, 2015; see also Todd, Gigerenzer, & the ABC Research Group, 2012). For example, with only two equally likely candidate hypotheses, hypothesis-scanning questions are just as informative as constraint-seeking questions. Moreover, when the alternative hypotheses considered are not all equally likely, a hypothesis-scanning question that targets a single high-probability hypothesis (e.g., one that has a 70% probability of being correct) can be more informative than a constraint-seeking question that targets several hypotheses with a small summed probability (e.g., 30%). Furthermore, not all constraint-seeking questions are equally effective: For example, a constraint-seeking question that partitions the hypothesis space evenly is on average more informative than a constraint-seeking question that partitions the same space unevenly.
In this paper we consider for the first time how the traditional distinction between constraint-seeking and hypothesis-scanning questions maps onto the more formal distinction between more and less informative questions, as measured by their expected information gain. Expected information gain (see Chin, Payne, Fu, Morrow, & Stine-Morrow, 2015; Nelson, McKenzie, Cottrell, & Sejnowski, 2010; Oaksford & Chater, 1994; Steyvers, Tenenbaum, Wagenmakers, & Blum, 2003) measures the reduction of entropy—that is, the uncertainty as to which hypothesis is correct—upon asking a certain question (see SI). Within this framework, the best questions are the ones that maximize the reduction of entropy, allowing one to move from a state of uncertainty (e.g., “Why was the boy late to school?”) closer to a state of certainty (e.g., “The boy was late to school because he woke up late.”) with the fewest number of questions. Recent studies have used information gain to measure the effectiveness of 7- to 10-year-olds and adults’ questions (Nelson et al. 2013; Ruggeri & Feufel, 2015; Ruggeri et al. 2015), but to our knowledge, information gain has not been used as a formal measure to capture preschoolers’ learning behavior. In particular, in the present study we investigate whether children change their reliance on different question types (constraint-seeking vs. hypothesis-scanning) depending on their informativeness as measured by expected information gain.

Previous research showed that by age 7 children still have difficulties generating effective constraint-seeking questions from scratch (e.g., “Is the boy late because of something he couldn’t find on his way to school?”), and they more often rely on hypothesis-scanning questions (Mosher & Hornsby, 1966; Ruggeri & Feufel, 2015; Ruggeri & Lombrozo, 2015). Generating constraint-seeking questions from scratch depends on children’s verbal knowledge, categorization skills, and previous experience. For example, one needs to identify features that can be used to group hypotheses into different categories, categorize objects correctly according to those features, and label those categories. Indeed, the developmental change in the effectiveness of children’s questions has been explained by an increasing ability to generate object-general features that can be used to cluster similar objects into categories (e.g., quadrupeds vs. non-quadrupeds, see Ruggeri & Feufel, 2015). The ability to make accurate judgments of effectiveness may therefore emerge earlier than the ability to generate effective questions from scratch. For this reason, we chose to focus on the accuracy of children’s judgments of questions, rather than their generation of questions, to examine the computational machinery needed to support effective question-asking behavior.

In what follows, we present four experiments in which 5-year-old children are given a simple causal inference task about why a monster, Toma, was late to school. In the first three experiments, we test the hypothesis that 5-year-olds are able to select the most effective question across a variety of scenarios. In particular, we hypothesize that children rely on different types of questions (constraint-seeking vs. hypothesis-scanning) based on their expected information gain in a scenario, rather than based on the probability of positive feedback (Experiment 1B) or the salience associated with the single most frequent hypothesis (Experiment 1C). In Experiment 2, we examine the possibility that children are adaptive learners, revising their judgments of effectiveness of different question types iteratively by taking into account how the hypothesis space changes due to feedback.
5.2 Experiment 1A: Selecting Effective Questions

5.2.1 Method

Participants. Participants were 60 5-year-olds (36 female, $M_{age} = 62.4$ months; $SD = 7.9$ months) recruited from local children’s museums and schools. Five additional children were excluded from the analyses for failing to respond to the test question ($N = 2$), or due to parental interference ($N = 3$). Participants were randomly assigned to one of two conditions: Uniform or Skewed.

Design and procedure. Participants were presented with a storybook, displayed on a computer screen. Each page of the storybook contained an illustration, as well as a short text that the experimenter read aloud. The story introduced Toma, a monster from Planet Apres, who is often late to school. Children were presented with the reasons why Toma was late to school over several days (see Table 5.B1), with each day represented on a different page of the storybook (e.g., “On Day 1, Toma was late because he could not find his jacket”, see Figure 5.1).

Figure 5.1. Example page of a storybook from the first series of experiments presenting the reasons why Toma was late to school over several days.

Subsequently, children were told that Toma was late to school again. His friends, Dax and Wug, wanted to find out why Toma was late to school again, so each of them asked Toma a question. One of the monsters (Dax or Wug, counterbalanced) asked a constraint-seeking question (i.e., a question that targets multiple hypotheses: “Was Toma late because he could not
find something?”), whereas the other monster asked a hypothesis-scanning question (i.e., a question that targets a single hypothesis: “Was Toma late because his bicycle was broken?”).

**Figure 5.2.** Displays presented at test in the Uniform (Figure 5.2A) and Skewed (Figure 5.2B) conditions of Experiment 1A.
At test, children were reminded of the reasons why Toma was late on previous days. This information was displayed on the same screen as the monsters’ questions (see Figures 5.2A and 5.2B). Children were then asked to indicate which of the two friends, Dax or Wug, would find out first why Toma was late to school again. We accepted both verbal responses (e.g., saying the monster’s name or color) and points towards either monster.

**Uniform Condition.** In the Uniform condition, the storybook presented six instances (i.e., six days) of Toma being late to school, each day for a different reason. Three of the reasons (jacket, books, or shoes) could be targeted together by a constraint-seeking question (i.e., “Was Toma late because of something he could not find?”). The information gain for the constraint-seeking question asked by one of the monsters was exactly 1, while the information gain for the hypothesis-scanning question asked by the other monster was 0.66 (Table 5.1, and see SI for an example of calculating information gain).

**Skewed Condition.** In the Skewed condition, the storybook presented eight instances (i.e., eight days) of Toma being late to school. Altogether, there were four different hypotheses. On five out of eight days, Toma was late to school because he woke up late, and on the other three days he was late for three different reasons. These three reasons could be grouped together by a constraint-seeking question: “Was Toma late because of something he could not find?” The information gain for the constraint-seeking question asked by one monster was now 0.81, while the information gain for the hypothesis-scanning question asked by the other monster was now 0.94 (Table 5.1).

Table 5.1

<table>
<thead>
<tr>
<th>Experiment/Condition</th>
<th>Associated Information Gain</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>CS question</td>
<td>HS question</td>
</tr>
<tr>
<td>1A – Uniform Condition</td>
<td>1</td>
<td>0.66</td>
</tr>
<tr>
<td>1A – Skewed Condition</td>
<td>0.81</td>
<td>0.94</td>
</tr>
<tr>
<td>1B</td>
<td>0.81</td>
<td>0.95</td>
</tr>
<tr>
<td>1C</td>
<td>0.95</td>
<td>0.88</td>
</tr>
</tbody>
</table>

**Note.** Bolded numbers indicate the question with a higher IG.

### 5.2.2 Results and discussion

An alpha level of 0.05 was used in all statistical analyses. Preliminary analyses found no effects of gender or the order in which the two monster friends (i.e., Dax and Wug) were presented. Subsequent analyses were collapsed over these variables.

---

1 To ensure that the information gain of the hypothesis-scanning question was higher than that of the constraint-seeking question in the Skewed condition, it was necessary to present more instances in this condition (8 days) than in the Uniform condition (6 day), but overall a smaller number of distinct hypotheses.
In the Uniform condition, 70% of the children in the Uniform condition selected the monster asking the constraint-seeking question, exact binomial \( p \) (two-tailed) = 0.042. In contrast, in the Skewed condition, 73% of the children selected the monster asking the hypothesis-scanning question, exact binomial \( p \) (two-tailed) = 0.016. A chi-square test confirmed the difference between these two distributions, \( \chi^2(2, N = 60) = 11.28, p < 0.001 \).

These results show that 5-year-old children are sensitive to the statistics of the environment. They predicted that in the Uniform condition the monster who asked the constraint-seeking question would converge to the solution faster as compared to the monster who asked the hypothesis-scanning question, and the reverse in the Skewed condition.

How did children compare the effectiveness of the two monsters’ questions? One intriguing possibility is that children based their judgments on the information gain associated with each question. However, an alternative possibility is that children might have simply selected the question (e.g., waking up late) with the highest probability of receiving positive feedback (i.e., a “yes” response). The design of Experiment 1A does not allow us to distinguish between these two interpretations: Both the constraint-seeking question in the Uniform condition and the hypothesis-scanning question in the Skewed condition have higher information gain, but they also have a higher probability of receiving positive feedback. We test this alternative explanation in Experiment 1B.

5.3 Experiment 1B: Controlling for the Probability of Positive Feedback

5.3.1 Method

Participants. Participants were 54 5-year-olds (29 female, \( M_{age} = 64.7 \) months; \( SD = 9.6 \) months) recruited at local museums and schools. Twelve additional children were excluded from the analyses for failing to respond to the test question (\( N = 5 \)), experimenter error (\( N = 2 \)), or parental interference (\( N = 5 \)). None of the children participated in Experiment 1A.

Design and procedure. In Experiment 1B, we tested children in a modified Skewed condition. Each child was presented with one of two storybooks. The two storybooks featured different stimuli in order to reduce potential effects related to children’s idiosyncratic preferences. Children were read a storybook that presented eight instances (i.e., eight days) of Toma being late to school. On five out of eight days, Toma was late to school for the same reason (e.g., he could not find his shoes), and on the other three days he was late for three different reasons (i.e., four different hypotheses in total; see Table 5.B1). Like Experiment 1A, one of the monster friends (Dax or Wug, counterbalanced) then asked a constraint-seeking question, while the other asked a hypothesis-scanning question, to find out why Toma was late to school again. In both storybooks, the constraint-seeking question targeted both the most frequent hypothesis and one of the other hypothesis (e.g., “Was Toma late because of something he could not find on his way to school?”). This question had a lower information gain of 0.81 when compared to the hypothesis-scanning question that targeted only the most frequent hypothesis (e.g., “Was Toma late because he could not find his shoes?”), which had an information gain of
0.95 (Table 5.1). However, the constraint-seeking question had a higher probability of resulting in positive feedback \((p = 0.75)\) as compared to the hypothesis-scanning question \((p = 0.63)\).

5.3.2 Results and Discussion

An alpha level of 0.05 was used in all statistical analyses. Preliminary analyses found no effects of gender or the order in which the two monster friends (i.e., Dax and Wug) were presented. Subsequent analyses were collapsed over these variables.

When predicting who would find out first why Toma was late to school, 70% of the children selected the monster asking the hypothesis-scanning question, exact binomial \(p\) (two-tailed) = 0.002. A chi-square test showed no significant difference between the distributions obtained for the two different sets of storybooks, \(\chi^2(2, N = 54) = 0.01, p = 0.95\).

The results of Experiment 1B rule out the alternative interpretation that children in Experiment 1A judged the questions’ effectiveness according to the probability of receiving positive feedback. With the use of two storybooks featuring different stimuli, it is also unlikely that our results were driven by children’s idiosyncratic preferences. However, in both Experiment 1A and 1B, children might have used frequency as a salient cue for identifying the most effective question, selecting the question that targeted the single most frequent hypothesis (e.g., waking up late). Experiment 1C tests this alternative interpretation.

5.4 Experiment 1C: Controlling for Salience

5.4.1 Method

**Participants.** Participants were 54 5-year-olds (24 female, \(M_{\text{age}} = 65.6\) months; \(SD = 8.5\) months) recruited at local museums and schools. Six additional children were excluded from the analyses because they failed to answer the test question (N = 3) or experimenter error (N = 3). None of the children participated in Experiments 1A or 1B.

**Design and procedure.** In Experiment 1C, we tested children in a modified Skewed condition. Once again, each child was presented with one of two storybooks. The two storybooks featured different stimuli in order to reduce potential effects related to children’s idiosyncratic preferences. Each child was read a storybook that presented ten instances (i.e., ten days) of Toma being late to school. On three out of ten days, Toma was late to school for the same reason (e.g., he woke up late), and on the other seven days, he was late for seven different reasons (i.e., eight different hypotheses in total; see Table 5.B1). As before, one monster friend (Dax or Wug, counterbalanced) asked a constraint-seeking question and another asked a hypothesis-scanning question to find out why Toma was late to school again. In both storybooks, the constraint-seeking question (e.g., “Was Toma because he could not find something on his way to school?”) had a higher information gain of 0.95. In contrast, the hypothesis-scanning question that targeted the single most frequent hypothesis (e.g., “Was Toma late because he woke up late?”) had a lower information gain of 0.88 (Table 5.1).
5.4.2 Results and Discussion

An alpha level of 0.05 was used in all statistical analyses. Preliminary analyses found no effects of gender or the order in which the two monster friends (i.e., Dax and Wug) were presented. Subsequent analyses were collapsed over these variables.

72% of the children selected the monster asking the constraint-seeking question, exact binomial $p$ (two-tailed) < 0.001. A chi-square test showed no significant difference between the distributions obtained for the two different storybooks, $\chi^2(2, N = 54) = 0.66, p = 0.41$.

The results of Experiment 1C rule out the interpretation that children in the Skewed conditions of Experiments 1A and 1B selected the hypothesis-scanning question simply because it targeted the single most frequent hypothesis. With the use of two sets of storybooks, it is also unlikely that the results of Experiment 1C were driven by children’s idiosyncratic preferences.

5.5 Discussion of Experiments 1A, 1B, and 1C

Experiments 1A-1C examined whether 5-year-old children were able to make predictions based on the informativeness of the presented questions. Across three experiments, we found that preschoolers judged the quality of the given questions in a way that is consistent with information gain, selecting the monster asking the question with higher information gain, regardless of whether it was a constraint-seeking or hypothesis-scanning question.

This claim is supported by our results showing that children in our task appeared not to rely on simpler strategies. First, although constraint-seeking questions are usually considered superior to hypothesis-scanning questions, children reliably judged a hypothesis-scanning question as more effective when the distribution of hypotheses resulted in the latter having a higher information gain (Experiment 1A). Second, children did not simply judge questions according to the probability of receiving positive feedback, although this strategy would require a considerably simpler computation than that of information gain (Experiment 1B). Finally, children did not rely on a heuristic based on frequency – they did not judge the question targeting the single most frequent hypothesis as more effective (Experiment 1C).

In Experiments 1A, 1B, and 1C, children were presented with only the first question that the agents (i.e., Dax and Wug) asked and were asked to identify which agent they thought would find out first why Toma was late to school. In other words, we asked children to choose the best first question to ask, and established that 5-year-old children can make accurate one-shot judgments of the effectiveness of presented questions. In Experiment 2, we ask whether children are adaptive learners, that is, whether they can identify the most effective question iteratively, and whether they can revise their judgments depending on how the hypothesis space changes due to feedback.
5.6 Experiment 2: Identifying Effective Questions Iteratively

5.6.1 Method

Participants. Participants were 60 5-year-olds (29 female, $M_{age} = 68.9$ months; $SD = 9.3$ months) recruited at local museums and schools. None of the children participated in Experiments 1A-1C.

Design and procedure. In Experiment 2, we tested children in a modified version of Experiment 1A. Children were read a shortened version of the storybook used in the first series of experiments, in which the reasons for Toma being late to school (see Table 5.B2) were presented all on one page (see Figure 5.3). Children were asked to count with the experimenter the number of times Toma had been late for each of the reasons presented.

Children were then told that Toma was late to school again, and that his friend, Wug, wanted to find out why. Participants were subsequently presented with two alternative questions that Wug could ask (a constraint-seeking and a hypothesis-scanning question) and had to indicate the question they thought Wug should ask. As in Experiment 1A, one of the two questions presented had a higher information gain than the other. The game continued only if children correctly selected the question with the higher information gain.

Children who selected the question with the higher information gain were subsequently presented with Toma’s answer to the selected question, which was always “no”. They were then shown two new follow-up questions that Wug could ask to find out why Toma was late to school—one constraint-seeking and one hypothesis-scanning question. On the same page, children were also shown an updated representation of the hypothesis-space (“These are now the reasons why Toma could be late for school, right?”), which excluded the hypotheses ruled out by Toma’s “no” feedback to the first question selected. Children were again asked to indicate which question they thought Wug should ask.

Children were randomly assigned to four possible conditions, across which we manipulated the question type (constraint-seeking or hypothesis-scanning) that was more informative in the original and revised hypothesis space.

Table 5.2

<table>
<thead>
<tr>
<th>Condition</th>
<th>Original hypothesis space</th>
<th>Revised hypothesis space</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>CS question</td>
<td>HS question</td>
</tr>
<tr>
<td>1</td>
<td>1.00</td>
<td>0.59</td>
</tr>
<tr>
<td>2</td>
<td>0.99</td>
<td>0.94</td>
</tr>
<tr>
<td>3</td>
<td>0.75</td>
<td>0.86</td>
</tr>
<tr>
<td>4</td>
<td>0.94</td>
<td>0.99</td>
</tr>
</tbody>
</table>

Note. Bolded numbers indicate the question with a higher IG in each hypothesis space.
Figure 5.3. Example page of a storybook from Experiment 2 presenting the reasons why Toma was late to school over several days.

5.6.2 Results and Discussion

An alpha level of 0.05 was used in all statistical analyses. Preliminary analyses found no effects of gender or the order in which the two follow-up questions (i.e., constraint-seeking question first or hypothesis-scanning question first) were presented. Subsequent analyses were collapsed over these variables.

Across the four conditions, when asked which question Wug should ask in the original hypothesis space, 67% of the children selected the question with higher information gain, exact binomial $p$ (two-tailed) = 0.013. A chi-square test showed no difference between the four conditions in the original hypothesis space, $\chi^2(3, N = 60) = 5.40, p = 0.145$. We therefore replicated the results found in Experiments 1A-1C, showing that children’s judgments were robust across different distributions and types of hypotheses.

Overall, we found that 47% of the children selected the question with higher information gain in both the original and the revised hypothesis space. This is significantly different from chance (25%), exact binomial $p$ (two-tailed) < 0.001. A chi-square test also showed no significant differences between the conditions in which the question type of the more informative question changed between the original and revised hypothesis space (e.g., where a constraint-
seeking question was more informative in the original hypothesis space, but a hypothesis-scanning question became more informative in the revised hypothesis space) and the conditions in which the question type of the more informative question remained the same between the original and revised hypothesis space, $\chi^2(1, N = 40) = 1.55, p = 0.214$.

These results suggest that 5-year-old children are adaptive learners: They can judge the effectiveness of presented questions iteratively, rather than being limited to one-shot judgments of the most informative first question. Furthermore, they selected questions based on its informativeness over its type (i.e., whether the question was constraint-seeking vs. hypothesis-scanning), revising their judgments of the effectiveness of different question types depending on the current hypothesis space.

### 5.7 General Discussion

Across four experiments, we show that 5-year-old children already have the computational foundations for developing successful question-asking strategies. The results of Experiments 1A – 1C indicate that children are able to select the most effective first question between two presented alternatives. Experiment 2 shows that this capacity is not limited to one-shot judgments of effectiveness: Children adapt their reliance on different question types (constraint-seeking vs. hypothesis-scanning), reassessing their judgment of effectiveness based on the hypothesis space currently under consideration.

Our results also suggest that children’s judgments and behaviors are well captured by the formal measure of expected information gain: 5-year-old children judge the effectiveness of questions according to how well they are expected to reduce the learner’s uncertainty about the true solution in the scenario considered.

It remains to be seen if this continues to be the case when a larger and more complex set of information gain comparisons is tested. Specifically, would children still reliably select the more effective question as measured by information gain, when the difference is very small? In such situations, the influence of other factors may become more apparent: the cost of computation in terms of time and resources; the frequencies of the different hypotheses; prior information about the agents’ attitudes and intentions (e.g., knowing from previous interactions that Toma is a very forgetful monster may encourage certain questions), etc. We note that we did not find that children’s performance reflected the varying levels of difference in information gain between the two given questions. In particular, although in some of the presented problems the difference in information gain between the two given questions was rather small, we still found that children selected the more effective question, as measured by information gain. We speculate that our sample sizes might not have provided enough power to detect such differences, and it would be worthwhile in future research to examine this quantitative relationship more closely.

Other measures and/or information search strategies, which may be more psychologically plausible than information gain, may be able to account for the data we observed. As noted in Nelson (2005), it is not trivial to choose a formal measure that best explains people’s choice of actions in active learning scenarios. More research is necessary to provide evidence that information gain adequately captures children’s judgments in the domain of question-asking.
behavior and to identify possible heuristics children may implement to approximate information gain calculations. Also, there may be other factors contributing to a question’s goodness, besides its informativeness. For example, questions that are formulated at a higher level of abstraction (e.g., “Does it have to do with the car and the bicycle?” vs. “Does it have to do with means of transportation?”) may enable answers to be more easily encoded (e.g., Cimpian & Erickson, 2012), and thus prove to be more useful when acquiring new knowledge.

It is an open question whether preschoolers are able to generate different types of questions depending on their informativeness, as do older children (Ruggeri & Lombozo, 2015; Ruggeri, Lombozo, Griffiths, & Xu, 2015). We could adapt Legare et al. (2013)’s methods to allow children to ask more hypothesis-scanning questions if they choose to, and we could manipulate the hypothesis space to make some constraint-seeking questions more informative than others.

In sum, by eliminating the need for children to generate questions from scratch, we demonstrate that 5-years-old children are sensitive to the relative effectiveness of different questions. Our results show that the computational machinery to support effective question-asking is already present by five years of age. Future research will investigate whether young children are able to generate their own questions based on their effectiveness, and how learners implement heuristics to approximate information gain computations.

5.8 Appendix A

Experiment 1A – Uniform condition

Information gain of each question can be computed as:

\[ IG = H_{prior} - H_{posterior} \]  
Eq. (A.1)

where:

\[ H = - \sum_h p(h) \log_2 p(h) \]  
Eq. (A.2)

The prior entropy \( H_{prior} \) defines the status of uncertainty preceding every action:

\[ H_{prior} = - \sum_h p(h) \log_2 p(h) \]  
Eq. (A.3)

where \( h \) are the candidate hypotheses, and \( p \) is the probability associated with each hypothesis. Because there are 6 equally likely hypotheses presented:

\[ H_{prior} = - \sum_h \frac{1}{6} \log_2 \frac{1}{6} (h) = 2.59 \]

The predictive posterior entropy \( H_{posterior} \) refers to the predicted status of uncertainty after the question is asked and the answer is received. Because there are two possible answers to each question (yes/no):

\[ H_{posterior} = p(x_{yes}|X)H(x_{yes}) + p(x_{no}|X)H(x_{no}) \]  
Eq. (A.4)
**Hypothesis-scanning question.** For a hypothesis-scanning question, the probability of getting a ‘yes’ answer is 1/6, whereas the probability of getting a ‘no’ answer is 5/6:

\[
H_{\text{posterior}} = \frac{1}{6} H(x_{\text{yes}}) + \frac{5}{6} H(x_{\text{no}})
\]

Using Eq. (A.2):

\[
H(x_{\text{yes}}) = 0
\]
\[
H(x_{\text{no}}) = 2.32
\]

Therefore:

\[
H_{\text{posterior}} = \frac{1}{6} (0) + \frac{5}{6} (2.32) = 1.93
\]

To obtain the information gain for the hypothesis-scanning question, we use Eq. (A.1):

\[
IG = 2.59 - 1.93 = 0.66
\]

**Constraint-seeking question.** The constraint-seeking question in the Uniform condition of Experiment 1 targets three of the six hypotheses, therefore the probability of getting a ‘yes’ or a ‘no’ answer is 3/6:

\[
H_{\text{posterior}} = \frac{3}{6} H(x_{\text{yes}}) + \frac{3}{6} H(x_{\text{no}})
\]

Using Eq. (A.2):

\[
H(x_{\text{yes}}) = 1.59
\]
\[
H(x_{\text{no}}) = 1.59
\]

Therefore:

\[
H_{\text{posterior}} = \frac{3}{6} (1.59) + \frac{3}{6} (1.59) = 1.59
\]

To obtain the information gain for the hypothesis-scanning question, we use Eq. (A.1):

\[
IG = 2.59 - 1.59 = 1
\]

**Experiment 1A – Skewed condition**

In the Skewed condition, there are four hypotheses presented. One hypothesis \((h_{\text{freq}})\) occurs on five out of eight instances, while the other three hypotheses \((h_{\text{infreq}})\) each occur once. Using Eq. (A.2):

\[
H_{\text{prior}} = - \left[ \frac{5}{8} (h_{\text{freq}}) \log_2 \frac{5}{8} (h_{\text{freq}}) + 3 \left( \frac{1}{8} (h_{\text{infreq}}) \log_2 \frac{1}{8} (h_{\text{infreq}}) \right) \right] = 1.55
\]
**Hypothesis-scanning question.** For the hypothesis-scanning question, the probability of getting a ‘yes’ answer is 5/8, whereas the probability of getting a ‘no’ answer is 3/8. Using Eq. (A.4):

\[
H_{\text{posterior}} = \frac{5}{8} H(x_{\text{yes}}) + \frac{1}{8} H(x_{\text{no}})
\]

Using Eq. (A.2):

\[
H(x_{\text{yes}}) = 0 \\
H(x_{\text{no}}) = 1.59
\]

Therefore:

\[
H_{\text{posterior}} = \frac{5}{8} (0) + \frac{3}{8} (1.59) = 0.59
\]

To obtain the information gain for the hypothesis-scanning question, we use Eq. (A.1):

\[
IG = 1.55 - 0.59 = 0.94
\]

**Constraint-seeking question.** The constraint-seeking question targets two of the three infrequent hypotheses, and the probability of getting a ‘yes’ answer is 1/4, whereas the probability of getting a ‘no’ answer is 3/4:

\[
H_{\text{posterior}} = \frac{1}{4} H(x_{\text{yes}}) + \frac{3}{4} H(x_{\text{no}})
\]

using Eq. (A.2):

\[
H(x_{\text{yes}}) = 1 \\
H(x_{\text{no}}) = 0.65
\]

Therefore:

\[
H_{\text{posterior}} = \frac{1}{4} (1) + \frac{3}{4} (0.65) = 0.74
\]

To obtain the information gain for the hypothesis-scanning question, we use Eq. (A.1):

\[
IG = 1.55 - 0.74 = 0.81
\]
### 5.9 Appendix B

**Table 5.B1**

*Frequencies of the Hypotheses Presented in the Experiment 1, Showing the Specific Hypotheses Targeted by Each Constraint-seeking and Hypothesis-scanning Question*

<table>
<thead>
<tr>
<th>Hypotheses</th>
<th>Expt. 1A Uniform</th>
<th>Skewed Book 1</th>
<th>Expt. 1B Skewed Book 1</th>
<th>Expt. 1B Skewed Book 2</th>
<th>Expt. 1C Skewed Book 1</th>
<th>Expt. 1C Skewed Book 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Couldn’t find jacket</td>
<td>1^c</td>
<td>1^c</td>
<td>1^c</td>
<td>1</td>
<td>1^c</td>
<td>-</td>
</tr>
<tr>
<td>Couldn’t find shoes</td>
<td>1^c</td>
<td>1^c</td>
<td>5^c,h</td>
<td>-</td>
<td>1^c</td>
<td>-</td>
</tr>
<tr>
<td>Couldn’t find books</td>
<td>1^c</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>1^c</td>
<td>-</td>
</tr>
<tr>
<td>Couldn’t find lunchbox</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>1^c</td>
<td>1</td>
</tr>
<tr>
<td>Bicycle was broken</td>
<td>1^h</td>
<td>1</td>
<td>1</td>
<td>5^c,h</td>
<td>1</td>
<td>3^h</td>
</tr>
<tr>
<td>Watching TV</td>
<td>1</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>Spilt milk on clothes</td>
<td>1</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>Woke up late</td>
<td>-</td>
<td>5^h</td>
<td>1</td>
<td>-</td>
<td>3^h</td>
<td>-</td>
</tr>
<tr>
<td>Go to dentist</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>1^c</td>
</tr>
<tr>
<td>Go to doctor</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>1^c</td>
</tr>
<tr>
<td>Go to grandma’s house</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>1^c</td>
</tr>
<tr>
<td>Pick up the dog from the vet</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>1^c</td>
</tr>
<tr>
<td>Car was broken</td>
<td>-</td>
<td>-</td>
<td>1^c</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Had to make his bed</td>
<td>-</td>
<td>-</td>
<td>1</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
</tbody>
</table>

^c Hypotheses targeted by the constraint-seeking question.

^h Hypothesis targeted by the hypothesis-scanning question.
Table 5.B2

*Frequencies of the Hypotheses Presented in the Experiment Two, Showing the Specific Hypotheses Targeted by Each Constraint-seeking and Hypothesis-scanning Question*

<table>
<thead>
<tr>
<th>Hypothesis</th>
<th>Original Hypothesis Space</th>
<th>Revised Hypothesis Space</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Condition 1</td>
<td>2</td>
</tr>
<tr>
<td>Couldn’t find jacket</td>
<td>1&lt;sup&gt;c&lt;/sup&gt;</td>
<td>2&lt;sup&gt;c&lt;/sup&gt;</td>
</tr>
<tr>
<td>Couldn’t find shoes</td>
<td>1&lt;sup&gt;c&lt;/sup&gt;</td>
<td>2&lt;sup&gt;c&lt;/sup&gt;</td>
</tr>
<tr>
<td>Couldn’t find books</td>
<td>1&lt;sup&gt;c&lt;/sup&gt;</td>
<td>2&lt;sup&gt;c&lt;/sup&gt;</td>
</tr>
<tr>
<td>Couldn’t find lunchbox</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Bicycle was broken</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>Watching TV</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Spilt milk on clothes</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Woke up late</td>
<td>10&lt;sup&gt;h&lt;/sup&gt;</td>
<td>3&lt;sup&gt;h&lt;/sup&gt;</td>
</tr>
<tr>
<td>Go to dentist</td>
<td>-</td>
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<tr>
<td>Go to doctor</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Go to grandma’s house</td>
<td>-</td>
<td>-</td>
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<tr>
<td>Pick up dog</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Car was broken</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Make his bed</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Fed his dog</td>
<td>-</td>
<td>1</td>
</tr>
<tr>
<td>Fed his cat</td>
<td>-</td>
<td>1</td>
</tr>
<tr>
<td>Couldn’t find backpack</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Prepare lunch</td>
<td>-</td>
<td>-</td>
</tr>
</tbody>
</table>

<sup>c</sup> Hypotheses targeted by the constraint-seeking question.

<sup>h</sup> Hypothesis targeted by the hypothesis-scanning question.
Chapter 6

Learning Higher-Order Generalizations through Free Play: Evidence from Two- and Three-Year-Old Children

6.1 Introduction

Young children are actively engaged in their environment. From as early on as infancy, they interact with other agents, they manipulate objects, and they explore their environment (Piaget, Cook, & Norton, 1952; Vygotsky, 1978). With the development of language, they begin to ask questions (Chouinard et al., 2007), and become relentless in seeking explanations for events that they observe (Frazier et al., 2009). Clearly, young children have some control over information incoming from the world: recent research has demonstrated that young learners allocate their attention and exploration in a non-random manner (Bonawitz, van Schijndel, Friel, & Schulz, 2012; Cook et al., 2011; Gerken et al., 2011; Kidd et al., 2012; Legare, 2012; Schulz & Bonawitz, 2007; Schulz, Gopnik, & Glymour, 2007; Stahl & Feigenson, 2015).

Yet to date, the role of self-generated evidence in children’s learning remains unclear. On the one hand, research in the field of education has demonstrated that play promotes academic skills such as literacy, language and math (Bergen & Mauer, 2000; Bruner, 1961; Hirsh-Pasek, Golinkoff, Berk, & Singer, 2008; Roskos, Christie, Widman, & Holding, 2010; Sarama & Clements, 2009a; Seo & Ginsburg, 2004; Singer, Hirsh-Pasek, & Golinkoff, 2009), but children in these studies were not required to generate evidence during play to fulfill a learning goal. A recent study by Fisher, Hirsh-Pasek, Newcombe, and Golinkoff (2013) has made significant headway in mitigating this gap in the education literature, demonstrating that for geometric concepts (e.g., the definitional properties of a triangle), 4- and 5-year-old children may benefit from learning within a playful context with guidance from an experimenter, but not from free play alone (see further discussion in the General Discussion).

On the other hand, previous studies in the field of cognitive development that have children generate data do not typically include outcome measures for learning, so it is an open question what children learn from the evidence that they generate by themselves (Cook et al., 2011; Legare, 2012; Schulz & Bonawitz, 2007). Two exceptions are Bonawitz et al. (2012) and Schulz et al. (2007), who examined the relationship between exploration and eventual learning accuracy in 5- to 7-year-olds. In one experiment, Bonawitz et al. (2012) tested 6- and 7-year-old children who had incorrect beliefs about how objects balance. These children were considered “Center Theorists,” as they believed that blocks would balance at the geometric center, rather than the center of mass. Half of these children were shown a block that balances only at the center of mass (Center of Mass condition). They were then given the opportunity to freely
interact with that block, after which the experimenter returned the block to its balance position and asked the child to explain why this was the case. Results showed that a majority of these children later successfully made a correct prediction that a new block should balance at its center of mass, and not the geometric center. In contrast, this effect was not found for the other half of the children who were shown, who freely explored, and who provided an explanation for a block that balances at the geometric center (Geometric Center condition).

Schulz et al. (2007) examined whether 5-year-olds could, through the course of free play, generate interventions that would help them learn the causal structure of the system they were interacting with. The causal system was a machine consisting of two gears that spin simultaneously when a switch was flipped on. Several different causal structures are thus possible, e.g. causal chain (e.g. the switch causes gear A to spin, and gear A causes gear B to spin), or common cause (e.g. the switch causes gears A and B to spin, independently of each other), etc. To learn the correct causal structure, children had to generate interventions such as flipping the switch on/off after placing the gears on individual “pegs” extending out of the machine. Their results suggested that the 5-year-olds could generate the necessary set of interventions when they played with the system in dyads, but it is unclear that children who played with the system on their own (i.e., singly) were actually performing better than chance at test. Only 6 out of 12 children in this condition (as compared to 12 out of 12 children in the complementary dyad condition) generated the complete set of evidence necessary to learn the correct causal structure. Furthermore, only a minority of the children (7 out of 18, combining the two conditions) who generated the complete evidence were actually successful at learning, i.e., selecting the correct causal structure at test. With infants, previous studies have only examined what infants choose to pay attention to (Kidd et al., 2012), and that upon detecting a violation, infants manipulate objects that have behaved in a surprising manner as if to confirm their previous observations (Stahl & Feigenson, 2015).

As such, there is a lack of empirical evidence that younger children can successfully acquire the correct generalizations through free play alone. In the current experiments, we therefore ask 1) whether 2- to 3-year-old children can use self-generated evidence to support the formation of higher-order generalizations, and 2) how their performance would compare to that in a didactic condition.

Like Schulz et al. (2007), we chose to examine these research questions in the causal domain. Causal learning is particularly important, as learning about our world is to learn the causal relations among the objects and events within it. It is the basis of all theory formation and change (Gopnik, Sobel, Schulz, & Glymour, 2001; Gopnik & Wellman, 2012; Gopnik, 2012). There is extensive evidence that children are good at causal learning: from an early age, they understand important causal relations—they can make causal predictions and give causal explanations (see Gopnik, Glymour, Sobel, & Schulz, 2004; Gopnik & Wellman, 2012; Griffiths & Tenenbaum, 2009 for reviews). More recently, studies have demonstrated that even infants and toddlers can engage in causal learning with just small amounts of training evidence (Gweon, Tenenbaum, & Schulz, 2010; Sobel & Kirkham, 2006; Walker & Gopnik, 2013). Furthermore, children’s real-world environment is filled with many examples of causal systems, and they have numerous opportunities to interact with such systems, e.g. they play with toys that have buttons and levers, they turn on light switches and remote controls, etc. Given the rich environmental
input, there is reason to believe that even younger children, 2- and 3-year-olds, may be able to successfully learn from self-generated evidence in simpler causal learning tasks as compared to the more complex causal systems presented in Schulz et al. (2007). However, to date, there has been no systematic study investigating whether and how much learning can be accomplished through free play in the causal domain.

To examine learning through free play, we asked: can children learn higher-order generalizations based on self-generated evidence? Making appropriate generalizations is a difficult task, but young children are remarkably proficient at doing so (e.g., Chomsky, 1980; Gelman & Wellman, 1991; Gopnik & Sobel, 2000; Wang & Baillargeon, 2005; Welder & Graham, 2006). They often make these generalizations at multiple levels of abstraction as well: Not only do they make first-order generalizations (e.g. dogs like to eat bones; rabbits like to eat vegetables), but they also make second, third, or fourth-order generalizations (e.g. each kind of animal has a favored food; each kind of animal has its own unique traits), which are particularly important for building large conceptual structures and domain level knowledge. This capacity to acquire generalizations at multiple levels through training appears to be domain-general: children do so in word learning (Smith, Jones, Landau, Gershkoff-Stowe, & Samuelson, 2002), categorization (Dewar & Xu, 2010; Macario et al., 1990), and causal learning (Walker & Gopnik, 2013), etc. Based on these findings, we hypothesized that young children can form higher-order causal generalizations from evidence that they independently generate during the course of free play. However, given that previous studies have not examined the effectiveness of free play vs. didactic learning in a domain of knowledge that is familiar to young children, it is an open empirical question whether children’s performance would differ between these learning contexts.

In addition, previous studies that have examined children’s learning through play have found successful learning after free play specifically in situations where children’s observations conflict with their current beliefs. For example, in Bonawitz et al. (2012), 6- and 7-year-olds were more likely to revise their beliefs about how objects balance only in conditions where they observed the block to be balancing at a location that differed from their initial predictions. Would children also be motivated to explore during free play, and to learn from self-generated data, even when they do not have strong initial beliefs about the task? Is observing a surprising event necessary to trigger learning from the evidence that children generate by themselves? These are questions that remain unexplored in the literature.

To investigate these questions, we designed a causal learning task in which 2- to 3-year-old children were presented with three different categories of “blicket” machines (c.f. Gopnik, Sobel, Schulz, & Glymour, 2001) and three blocks of different shapes and colors. For half of the children, the machines were activated using a shape rule: a shape-match block had to be used to activate the machine, while for the other half, the machines were activated using a color rule: a color-match block had to be used to activate the machine.

In Experiment 1, we tested children in a didactic learning condition. Like most other experiments, we gave children the data directly, and examined what they had learnt from the training data. To do this, an experimenter demonstrated the activations of three categories of

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2 We thank Liz Bonawitz for this suggestion.
machines by using the appropriate block to activate each machine. We then asked the children to make both first-order generalizations, where they had to choose from a new set of blocks to activate a previously seen machine, and second-order generalizations, where they had to choose from a new set of blocks to activate a novel machine. We then tested children in two different versions of free play in Experiments 2 and 3, in order to compare children’s performance with that in the didactic condition. Finally, in Experiment 4, we measured children’s baseline performance in these generalization tests.

6.2 Experiment 1: Learning by Didactic Instruction

6.2.1 Method

Participants. Thirty-two English-speaking 2- and 3-year-olds (12 boys and 20 girls) with a mean age of 35.8 months (range = 31.1 to 42.3 months) were tested. All were recruited from Berkeley, California, and its surrounding communities. The sample was representative of the ethnic diversity in these communities. An additional 2 children were tested but excluded due to refusal to make a choice at test (N = 1), or experimenter error (N = 1).

Materials. Four categories of toy machines were used in this experiment, with two identical machines in each category. The categories differed in shape and color, i.e. machines in Category 1 were blue and rectangular; machines in Category 2 were red and triangular; machines in Category 3 were green and circular; and machines in Category 4 were orange and L-shaped (each approximately 30 cm x 10 cm x 5 cm). Each set of machines produced a unique sound when activated (Figure 6.1). This effect was achieved by hiding a doorbell in each machine that was activated by an experimenter with a remote control device.

A variety of small blocks (approximate 4 cm x 2 cm x 1 cm) with different shapes and colors were used to activate these machines. Some of these blocks matched the toy machines in shape but not color (shape-match blocks), some matched the machines in color but not shape (color-match blocks), and others did not match the machines in shape or color (distracter blocks). Three white trays with separators were also used to easily present the activator blocks during the learning phase and the test phase.

Procedure. Children were tested individually in the laboratory. The parents were also present in the testing room, but sat about 80 cm behind the children throughout the experiment, in order to not influence their actions and choices. Children were introduced to the machines and blocks under the pretext of the experimenter showing them her toys.

The experiment consisted of two phases: a learning phase and a test phase. To begin the learning phase, the experimenter presented a white tray containing three blocks differing in shape and color. The child was free to play with these blocks for about 20 seconds. After this exploration, the blocks were returned onto the tray and pulled close to the experimenter, but remained visible to the child.
Figure 6.1. Schematic diagram of materials and procedure for children presented with the machines which were activated according to a shape-match rule.

The experimenter then presented the first toy machine (e.g. blue rectangular machine), and activated the machine with one of the three blocks by placing it on top of the machine (e.g. red rectangular block, if the machines were being activated by a shape rule; blue triangular block, if the machines were being activated by a color rule). Upon the machine’s activation, the experimenter drew attention to the event by saying, “Look! The block made the machine go; it made it go!” The experimenter next showed the child another machine that was identical to the first one, and activated it using the same block. This first set of two machines was then cleared from the table. The experimenter repeated this procedure with two other sets of training machines, activating them with their respective shape-match or color-match blocks.

A total of six machines were presented during the learning phase, and each child saw each machine being activated only once. The three categories of machines chosen as the training set were randomized, leaving the fourth category of machines for the test phase (i.e. each
category could be used in the training or the test phase). The order of presentation for the categories of training machines was also counterbalanced. The duration of this phase was about 4 minutes.

A test phase immediately followed the learning phase. The test phase consisted of a first-order generalization test and a second-order generalization test (see Figure 6.1). In the first-order test, each child was presented with a familiar machine, which is a machine that was previously presented in the learning phase. Then, the child was provided with 3 novel choice blocks in a white tray: a shape-match block, which is similar to the target machine in shape but not color, a color-match block, which is similar to the target machine in color but not shape, and a distracter block, which differed from the target in both color and shape. The experimenter requested the child to hand her a block that made the target machine go, “Can you give me the block that makes this machine go?”

In the second-order test, each child was presented with a novel machine, which is a machine that was not previously presented in the learning phase. The child was again asked to activate the machine by choosing among 3 novel choice blocks: a shape-match block, a color-match block, and a distracter block. The presentation order of the three choice blocks were counterbalanced for both test trials. The duration of the test phase was about 1 minute.

**Coding.** The children’s responses in the test trials were scored for accuracy. For the children exposed to the shape rule during the learning phase, choosing a shape-match block was scored as 1 point. Correspondingly, for children exposed to the color rule, choosing a color-match block was scored as 1 point. The maximum score for each child was 2 points, as there were 2 test trials in total. The children’s scores were then converted into percentage of accuracy. A second coder recoded all of the children’s responses offline, and the level of agreement between the coders was 100%.

### 6.2.2 Results

An alpha level of 0.05 was used in all statistical analyses. As Figure 6.2 shows, children performed accurately on the test trials, frequently selecting the correct block to activate the machines. 69% of the children chose the correct activator block in the first-order generalization test, exact binomial \( p \) (two-tailed) < .0001. 75% of the children chose the correct activator block in the second-order generalization test, exact binomial \( p \) (two-tailed) < .0001.

Using children’s responses on the two test trials, we also performed a repeated measures logistic regression. Our results indicate that there were no effects of gender, age, trial order (first test trial vs. second test trial), presentation order of the training machines (e.g. whether machines from the different categories were presented first, second or third during the training phase), and rule type (shape rule vs. color rule) on children’s accuracy on the test trials. Critically, there was no difference between children’s performance on the first-order and second-order generalization trials, Wald Chi-Square = .49, \( p = .48 \).
6.2.3 Discussion

These results converge with and extend the results found in many previous training studies examining learning and generalization in young children. With a short training session and a small amount of instructive evidence, two- and three-year-old children in Experiment 1 learned first-order and second-order generalizations about the causal structure of the machines, picking out the correct activator blocks according to the rule that they were exposed to within a didactic learning context.

6.3 Experiment 2: Learning by Free Play

In Experiment 2, we investigated whether children would successfully acquire first-order and second-order generalizations based on evidence generated by themselves during the course of free play.

6.3.1 Method

Participants. Twenty-four English-speaking 2- to 3-year-olds (10 boys and 14 girls) with a mean age of 36.1 months (range = 30.3 to 42.3 months) were tested. All were recruited from Berkeley, California, and its surrounding communities. The sample was representative of the ethnic diversity in these communities. An additional 3 children were tested but excluded due to

Figure 6.2. Percentage accuracy in the four experiments. Dashed line indicates chance performance. Error bars represent 95% CI.
refusal to make a choice at test (N = 1), no attempts to make any activations (N=1), or experimenter error (N = 1).

**Materials and Procedure.** The procedure for Experiment 2 consisted of three phases: a familiarization phase, a learning phase, and a test phase. To begin the familiarization phase, the experimenter presented the child with a cross-shaped yellow machine, together with its activator block. This block matched the machine both in shape and color. The familiarization phase served to introduce the child to the sound-making function of these novel machines. This phase was not necessary in Experiment 1, since the machines’ function would be introduced in the learning phase. The experimenter then activated the machine, drawing attention to the event by saying, “Look! The block made the machine go. It made it go!” The child was then given the activator block, and was allowed to activate the machine freely. The experimenter ensured that each child saw at least two activations of this familiarization machine.

The learning phase followed, and it began by the experimenter exclaiming, “Oh no! I just remembered that I have some work to do. While I’m doing my work, you can play with some of my toys!” The experimenter then laid out three plastic bins, each consisting of two identical machines together with their corresponding activator block (e.g., if the machines were being activated by a color rule, then the blue activator block was placed in the same plastic bin as the blue machines). The experimenter subsequently moved to a table and pretended to work, telling the child, “You can go ahead and play!” All three activator blocks and the three categories of machines were simultaneously available to the child, who was then given 5 minutes to play freely with the machines and blocks. After 5 minutes, the experimenter announced that she was done with her work and that it was time to put the toys away. The test phase that immediately followed was identical to that in Experiment 1.

**Coding.** The coding scheme used in Experiment 2 was identical to that of Experiment 1.

### 6.3.2 Results

Due to the free play nature of Experiment 2, individual children’s behaviors varied in the following dimensions: the number of activations for each category of machines (M = 5.07, SD = 5.12; recall that children in Experiment 1 each saw 2 activations per category of machines), and the number of times that “negative evidence” was generated, as defined by the number of times the child placed an activator block on a machine from a different bin (M = 3.7, SD = 5.11). 79% of the children activated every category of machines that was presented during the free play phase (i.e., the set of activations presented in Experiment 1), and 42% of the children generated more evidence than what was presented to children in Experiment 1.

Children performed accurately under free play in Experiment 2, often selecting the correct block to activate the machines. 71% of the children chose the correct activator block in the first-order generalization test, exact binomial $p$ (two-tailed) = .0002. 75% of the children chose the correct activator block in the second-order generalization test, exact binomial $p$ (two-tailed) < .0001.

Using children’s responses on the two test trials in Experiment 2, we performed a repeated measures logistic regression. Our results indicate that there were no effects of gender, age, trial order (first test trial vs. second test trial), presentation order of the training machines...
(e.g. whether machines from the different categories were presented first, second or third during
the training phase), and rule type (shape rule vs. color rule) on children’s accuracy on the test
trials. Critically, there was no difference between children’s performance on the first-order and
second-order generalization trials ($M = .75$, $SD = .44$), Wald Chi-Square = .13, $p = .71$ (Figure
6.2).

6.3.3 Discussion

The results of Experiment 2 demonstrate that children can successfully acquire higher-
order generalizations in the causal domain during the course of free play. However, the way to
generate useful evidence in this experiment was quite transparent to the children – all they had to
do was to place each block on top of the machines that were placed in the very same bin. Would
children also learn successfully when the evidence generation process is more obscure?

6.4 Experiment 3: Learning by Naturalistic Free Play

In Experiment 3, we increased the difficulty of the free play task by handing the activator
blocks directly to the child, such that the blocks were no longer presented together with their
associated machines.

6.4.1 Method

Participants. Thirty-two English-speaking 2- to 3-year-olds (17 boys and 15 girls) with a
mean age of 36.4 months (range = 30.3 to 44.4 months) were tested. All were recruited from
Berkeley, California, and its surrounding communities. The sample was representative of the
ethnic diversity in these communities. An additional 3 children were tested but excluded due to
parental intervention (N = 1), no attempts to make any activations (N=1), or experimenter error
(N = 1).

Materials and Procedure. The materials and procedure used in Experiment 3 were
identical to those of Experiment 2, except that after laying out the machines in 3 separate bins in
the Free Play phase, the experimenter handed the three activator blocks directly to the child.

Coding. Children’s responses in the test trials of Experiment 3 were scored in the same
way as that of Experiments 1 and 2.

6.4.2 Results

Just as in Experiment 2, individual children’s behaviors varied in the following
dimensions: the number of activations for each category of machines ($M = 18.81$, $SD = 16.08$),
and the number of times that “negative evidence” was generated, as defined by the number of
times the child placed an activator block on a machine that leads to a non-activation ($M = 13.06$,
$SD = 15.51$). 68% of the children activated every category of machines that was presented during
the free play phase (i.e., the set of activations presented in Experiment 1); this group of children
also all generated more evidence than the data presented to children in Experiment 1. Regression
analyses found that the above variables were not significant predictors of children’s overall accuracy at test: 1) the number of successful activations, $\beta = .043, t(28) = 1.28, p = .21$, 2) the number of “negative evidence” children generated, $\beta = -.05, t(28) = -1.31, p = .20$, and 3) the number of categories activated, $\beta = -.052, t(28) = -1.04, p = .31$.

Children also performed accurately under free play in Experiment 3, selecting the correct block to activate the machines. 69% of the children chose the correct activator block in the first-order generalization test, exact binomial $p$ (two-tailed) < .0001. 69% of the children chose the correct activator block in the second-order generalization test, exact binomial $p$ (two-tailed) < .0001. A repeated measures logistic regression indicated that there were no effects of gender, age, trial order (first test trial vs. second test trial), presentation order of the training machines (e.g. whether machines from the different categories were presented first, second or third during the training phase), and rule type (shape rule vs. color rule) on children’s accuracy on the test trials. Critically, there was no difference between children’s performance on the first-order and second-order generalization trials, Wald Chi-Square = .0, $p = 1$.

An overall comparison also revealed that children’s performance on the test trials did not differ across the three experiments. A repeated measures logistic regression with Experiment (1 vs. 2 vs. 3) as a between-subjects variable did not indicate Experiment to be a significant predictor of children’s accuracy on the two test trials, Wald Chi-Square = .14, $p = .71$.

### 6.4.3 Discussion

The results of Experiment 3 replicated those of Experiment 2. Two- to 3-year-old children, in the course of free play, can generate the necessary data for their own learning; they were able to use the self-generated evidence to acquire higher-order generalizations, even when the data generation process was less transparent. The accuracy of their learning did not differ whether they were trained by an experimenter (Experiment 1), or allowed to play with the toys freely by themselves (Experiments 2 and 3).

### 6.5 Experiment 4: Baseline

In Experiment 4, we tested another group of children in a baseline condition to address the possibility that children would be similarly successful at the generalization tests without any prior training or free play.

#### 6.5.1 Method

**Participants.** Twelve English-speaking 2- to 3-year-olds (7 boys and 5 girls) with a mean age of 39.2 months (range = 30.0 to 44.6 months) were tested. All were recruited from Berkeley, California, and its surrounding communities. The sample was representative of the ethnic diversity in these communities. An additional 2 children were tested but excluded due to parental interference ($N = 1$) or experimenter error ($N = 1$).

**Materials and Procedure.** The procedure of Experiment 4 consisted only of the test phase of Experiments 1 – 3. The test phase consisted of two test trials. For each test trial, the
experimenter presented the child with one machine and said, “This machine makes a sound.” She then provided the child with 3 choice blocks: a shape-match block, a color-match block, and a distractor, and asked, “Which block makes this machine go?”

Coding. Children’s responses in the test trials of Experiment 4 were scored in the same way as that of Experiments 1 – 3.

6.5.2 Results

As Figure 6.2 shows, 33% of the children chose the correct activator block in the first-order generalization test, exact binomial \( p \) (two-tailed) = 1. 42% of the children chose the correct activator block in the second-order generalization test, exact binomial \( p \) (two-tailed) = .55.

A repeated measures logistic regression indicated that there were no effects of gender, age, trial order (first test trial vs. second test trial), presentation order of the training machines (e.g. whether machines from the different categories were presented first, second or third during the training phase), and rule type (shape rule vs. color rule) on children’s accuracy on the test trials. Critically, there was no difference between children’s performance on the first-order and second-order generalization trials, Wald Chi-Square = .21, \( p = .65 \).

An overall comparison across the four experiments with Experiment (1 vs. 2 vs. 3 vs. 4) as a between-subjects variable indicated Experiment to now be a significant predictor of children’s accuracy on the two test trials, Wald Chi-Square = 4.22, \( p = .04 \).

6.5.3 Discussion

Children’s accuracy in Experiment 4 differed from those in Experiments 1 – 3. This finding demonstrated that without any prior training or free play, children were not successful at the generalization tests.

6.6 General Discussion

The present study examined whether 2- and 3-year-old children can form higher-order generalizations in the causal domain based on self-generated evidence during the course of free play, or experimenter-generated evidence within a didactic learning context. The results demonstrate that children can do so: In Experiments 1 – 3, children as young as two-and-a-half rapidly made first-order and second-order generalizations about how the machines and the activator blocks interacted with one another, and they extended these generalizations appropriately to novel toy machines. Furthermore, the accuracy of children’s generalizations was comparable across the three experiments, indicating that young children are equally effective in learning from both types of evidence. Experiment 4 provided a baseline measure, demonstrating that without prior training or free play, children performed at chance levels on the generalization tests.

These results make two important new contributions to the literature. First, children showed robust learning during free play in Experiments 2 and 3. They consistently chose the correct blocks to activate the machines presented in both the first-order and second-order
generalization tests. This success suggests that even in the absence of explicit instructions, children can, in the course of free play, generate the relevant evidence themselves in order to form appropriate generalizations. This learning condition is much closer to what children encounter in the real world, where preschoolers are often allowed to play freely, and engage with whatever aspects of the environment they find interesting and appealing. Furthermore, we found that the two- and three-year-old children were motivated to explore during the free play period, despite not entering the task with strong initial beliefs about how the toy machines would work. It was also not necessary for the children to first observe a confounded or surprising event (a la Bonawitz et al., 2012; Schulz & Bonawitz, 2007) to explore in a way that would successfully support future learning. Our data therefore provide evidence that 1) young children are motivated to understand what rules govern the behavior of the objects around them, and that 2) they may have some rudimentary capacity to systematically generate the relevant evidence to support such learning.

Second, children’s success in these experiments constitutes the first demonstration that children can acquire generalizations at multiple levels in the causal domain. Our finding that children performed similarly in the first-order and second-order generalization tests is consistent with previous computational work with hierarchical Bayesian models demonstrating that abstract knowledge can be acquired very quickly with sparse data. At times, higher-order learning may even proceed faster than the learning of lower-level details, in what is known as the “blessing of abstraction”, because while specific data points provide evidence for lower-level generalizations, the entire set of data points provides evidence for higher-order generalizations (Kemp et al., 2007; Perfors et al., 2011; Tenenbaum et al., 2011). Previously, developmental research based on this framework has largely focused on word learning and categorization, leaving open the question of whether this capacity is limited to only a few specific domains. Given that causal knowledge constitutes the foundation of intuitive theories, and that these theories are present in multiple domains (Carey, 1985, 2009; Gopnik & Meltzoff, 1997), there is reason to believe that the ability to acquire generalizations at multiple levels of abstraction is a domain-general one. This finding therefore lends empirical support to the “learning to learn” view (Griffiths & Tenenbaum, 2009; Kemp et al., 2007; Tenenbaum et al., 2007), which argues that early input provides the basis for developing inductive constraints and biases, and that subsequent learning is guided by these learned constraints.

The findings of the present study may appear inconsistent with previous work in the field of education suggesting that free play (also known as “pure discovery” or “unassisted discovery”), as an instructional method, leaves little to be desired (Alfieri, Brooks, Aldrich, & Tenenbaum, 2010; Chien et al., 2010; Kirschner, Sweller, & Clark, 2006; Mayer, 2004; Weisberg, Hirsh-Pasek, & Golinkoff, 2013). In a recent study, Fisher et al. (2013) demonstrated that when 4- to 5-year-olds were allowed to engage in free play, they failed to extract key concepts associated with the definitional properties of specific shapes (e.g., all triangles have three sides and three angles) despite being provided with enriched materials. In contrast, children who engaged in guided play, where an adult experimenter was present to scaffold their learning (e.g., guiding children to “discover” the definitional properties of triangles as having three sides and three angles), showed a robust improvement in their shape knowledge with relatively little decline over a 1-week period.
However, it should be noted that many of these education studies focus on academic content—domains such as literacy, mathematics, and science. While these domains are appropriate for investigating the effectiveness of different instructional methods in preschool and school settings, they may not be the best candidates for revealing a natural capacity for young children to learn from evidence that they independently generate through free play. Reasoning about causal relations, on the other hand, is fundamental and central to building intuitive theories without formal instruction, and consequently, causal knowledge may thus be a domain in which young children can effectively learn about through free play. In contrast, it is highly unlikely that young children can discover mathematical concepts when they play with math-related objects by themselves (Fisher et al., 2013; Sarama & Clements, 2009b). In addition, “pure discovery” or “unassisted discovery” tasks within education are likely to be more complex and open-ended, resulting in a much larger set of possible hypotheses that children would have to sift through, as compared to our free play task in Experiments 2 and 3, where the hypothesis space is relatively constrained and children were somewhat limited in their choice of actions. We suggest that a child’s capacity to learn from self-generated evidence is likely to be strongly influenced by both the domain and the complexity of the learning task. Experiment 3 begins to address this issue, but we acknowledge that further work would be necessary to bring our free play context closer to free play in children’s real world environment. Future work should also more closely examine the cognitive and task factors that influence the success of these different learning contexts.

Our findings also raise many questions for future research. Our work leaves open the issue of why children performed just as well under free play as compared to a didactic learning context. Our analyses of children’s choice of actions during the free play period in Experiment 3 did not reveal any relationship between children’s generalization accuracy and 1) the number of successful activations, 2) the number of unsuccessful activations (i.e., negative evidence: observing that a block does not make the machine go), and 3) the number of categories of machines successfully activated. We speculate that one important reason for children’s success under free play in Experiments 2 and 3 is that they successfully generated a superset of the evidence observed by children in Experiment 1, possibly through a trial and error approach. Given that the set of evidence presented in Experiment 1 was instructive in forming the appropriate higher-order generalizations, it is perhaps unsurprising that children in Experiments 2 and 3 were likewise able to perform well at test. Future work will more closely examine the systematicity of children’s actions during the free play period of Experiments 2 and 3, with one promising direction being information gain analyses. Information gain is a formal measure that quantifies how much uncertainty is reduced, with respect to the true hypothesis (e.g., in our studies, the shape or color rule that governs how the machines work), when a particular action is taken (Nelson, 2005). To engage effectively in hypothesis-testing, learners should systematically choose actions that lead to the highest expected information gain. Such analyses may shed light on the underlying processes that drive children’s actions during free play, and give insight about how engaging in free play may lead to learning and generalization, as demonstrated in the present study. At the same time, we note that such an analysis would be neither straightforward nor comprehensive. It is not clear that every action that children take during free play is about hypothesis-testing. For example, two- and three-year-old children in our study often repeated their actions during free play, especially after successfully activating a machine. Repetitions
provide no new information when it comes to learning about a deterministic system, but they do generate a lot of joy and excitement. When it comes to free play, there thus appears to be at least two competing goals on the learner’s part: 1) to learn about the system that one is interacting with, and 2) to enjoy that very interaction. Given these different goals, it remains to be seen how children’s actions during free play can be properly analyzed (see Coenen, Rehder, & Gureckis, 2015 for discussion of similar issues in adults).

Are there any developmental changes in the capacity to learn through self-generated evidence in the causal domain? Preliminary results with 19-month-olds suggest ‘yes’ (Sim & Xu, 2015). When 19-month-old toddlers were tested in procedures similar to Experiments 1 and 2, we found a different picture; these toddlers produced chance performance whether they were directly provided with instructive evidence or given a free play opportunity. When their play was facilitated by an experimenter or a parent, similar to the manipulation used in Fisher et al. (2013), we found instead that these toddlers were able to acquire the appropriate higher-order generalizations like the two- and three-year-olds in the present study. It is noteworthy that these preliminary results align very well with Fisher et al. (2013): guided play was beneficial to children’s learning, while free play was not. At the same time, the striking difference found between the two age groups despite the use of similar procedures begs the question of why this would be the case. Was the task too open-ended for the 19-month-old infants, who presumably have less well-formed prior beliefs to constrain the hypothesis space, as compared to the 3-year-olds? Or do they not understand what constitutes evidence and how to generate it through their own actions? As Weisberg, Hirsh-Pasek, Golinkoff, and McCandliss (2014) explains, guided play is an example of an adult-structured *mise en place* (i.e., an environment that prepares and nudges children towards engaging in particular types of actions), and having such a *mise* may be especially important for younger children who have poorer proactive control mechanisms as compared to their older counterparts. More research is necessary to better understand how this capacity may develop and interact with contextual factors over early childhood.

In summary, the present study provides strong evidence that young children are self-directed learners, and they can effectively engage different aspects of their environment to support their own learning. Future research will investigate the optimality of the active learning that children partake in, as well as its limits, to shed light on how early learning occurs in the real world.
Chapter 7

Conclusions

7.1 Conclusions and Implications of Empirical Work

The goal of this dissertation was to examine how self-directed activity might be relevant to children’s development, specifically to their cognitive development. In other words, I asked whether children can be considered to be active learners – learners who are able to successfully acquire knowledge about their world through self-directed activity. Through this work, we identified four component processes that allow young learners to engage in active learning effectively during early childhood.

First, young learners detect situations where more learning is necessary. As real life is neither made up of well-specified and transparent learning goals, nor are adults constantly directing children to acquire certain pieces of information, it is crucial to demonstrate that young children are able to independently identify learning goals, or at least situations where more learning is necessary, in order to make the case that active learning is relevant to development. In Chapter 2, we showed that infant looking times appear to be driven by the evidence an observed event provides for a set of alternative hypotheses over the currently favored hypothesis. That is, infants look longer when alternative hypotheses are warranted by unexpected evidence, indicating that by 8 months, infants can successfully detect situations where their current understanding is inaccurate or incomplete, and there may be a better explanation for the events that they observed. Such inferential reasoning is consistent with principles of Bayesian reasoning, and its presence early on in development may allow infant learners to take their first steps towards developing an accurate view of the world.

Second, they attend to or approach potential source(s) of information. Once learning opportunities are identified, learners would need to attend to or approach the source of information for learning to actually occur. And unexpected events are exceptional opportunities for learning, as the world is behaving differently from one’s representation of it. In Chapter 3, we demonstrate a strong correspondence between infants’ looking times and approach behaviors, which indicates that 13-month-old infants preferentially explore sources of unexpected events. Such spontaneous exploration may provide information relevant for theory revision, allowing infants to play an active role in driving their own development.
Third, they are able to *generate* evidence that is relevant to the learning goal. In Chapter 4, our results indicate that the information gathering that 5- and 7-year-old children engage in was systematically driven by uncertainty. We further found that the systematicity of the children’s data generation process was correlated with their learning performance. In addition, we demonstrated that learning from selection, where each child was allowed to make decisions about the data they wished to experience, was superior to yoked reception, where each child was instead presented with a sequence of items that was previously selected by a child in the selection condition.

What mechanisms enable young children to independently and systematically generate evidence to support their own learning? In Chapter 5, we examined whether 5-year-old children were able to select the most effective questions, and we found that children’s judgments were well-captured by the formal measure of expected information gain. In other words, the 5-year-olds judged the effectiveness of questions in a way that was consistent with evaluating how well each question was expected to reduce the learners’ uncertainty about the true hypothesis. This work suggests that the computational foundations for developing effective information search strategies may be in place by five years of age, allowing preschoolers to successfully generate evidence to support their own learning.

Finally, young learners actually *learn* from self-generated data; they incorporate observed outcomes into their knowledge. Chapter 6 demonstrates that two- and three-year-old children can successfully learn higher-order generalizations based on self-generated evidence through the course of free play. Furthermore, the children’s generalization performance was comparable after free play and didactic instruction.

Although our experiments demonstrated that these components are present by early childhood, we also found that the components may emerge at different time points during development. It appears that the ability to successfully detect new learning opportunities is present in 8-month-old infants, and there is selectivity in approach and exploration behaviors in 13-month-old infants. I consider these two components as the foundational blocks of active learning, and it is clear that these foundations are laid very early on in development. However, when it comes to systematically generating data to support one’s learning, this component does not seem to be present in 2- and 3-year-old children (Chapter 6), who perhaps use a trial and error approach for data generation. But the systematicity certainly seems to emerge by 5 years of age, as shown in Chapters 4 and 5. Despite that, children can already successfully learn from self-generated evidence before their 3rd birthday. We also observed that there is continued development in the link between data generation and learning over childhood: 7-year-olds appear to learn more easily from self-generated evidence as compared to 5-year-olds.

Overall, some components of active learning are present early, but others may emerge over development. These capacities also continue to develop, and correspondingly, we see that children become better at directing their own learning over time.

### 7.2 Remaining questions and future directions

In this work, my collaborators and I tested children of different age groups using a variety of methods, allowing us to make a few broad observations about the developmental trajectory of
active learning. However, this aspect of active learning is still very much underexplored. We have begun studies to address this gap in the literature. In ongoing work with 19-month-old infants using the free play paradigm explored in Chapter 6, we found that unlike two- and three-year-olds, these infants failed to acquire the correct generalizations about the blocks and toy machines when they were allowed to generate data to support their own learning. This finding suggests that the capacity for active learning might initially be limited to detecting new learning opportunities and to attention and approach behaviors, while the capacities for independently generating evidence and learning from self-generated data may emerge only after 3 years of age.

How do children go from being able to engage in active learning only in the sense of focusing their attention on a subset of available data in early infancy, to being able to engage in active learning that requires them to generate their own data in toddlerhood? We speculate that conditions where a child’s free play is facilitated by adults might hold the key to this puzzle. In our work with the 19-month-olds, we actually found that when their play was facilitated by either an adult experimenter or their own parent, the infants now successfully formed first- and second-order generalizations about how the blocks and toy machines interacted with one another, and they extended these generalizations when asked to activate a new machine (Sim & Xu, 2015). In the course of such collaborative play, not only is the learning about the task at hand boosted (as our results indicate), children may also be provided with an opportunity to observe and pick up on appropriate strategies for generating the evidence necessary to support one’s own learning. This knowledge would prepare them well to reap the benefits of self-directed learning later on in development (Bruner, 1961; Castro et al., 2008; Markant & Gureckis, 2013; Sim & Xu, 2014).

Another future direction is to explore how the complexity of the learning goal may interact with the effectiveness of different learning conditions. In Chapter 6, we demonstrated that children’s generalization performance was equivalent between the didactic instruction and free play conditions. However, there may be instances where free play is more effective than training, and other instances where training may be more effective than free play. For example, it is highly unlikely that young children can discover mathematical concepts when they play with math-related objects by themselves (Fisher et al., 2013; Sarama & Clements, 2009b). In addition, tasks within education are likely to be more complex and open-ended, resulting in a much larger set of possible hypotheses that children would have to sift through in order to arrive at the correct solution. As such, one might find didactic instruction to be more effective (or more efficient) at imparting these forms of knowledge, as compared to free play. Yet this may not always be the case. In a follow-up study on the work presented in Chapter 6, we increased the complexity of the rule that children had to learn: instead of having the machines activated by a single shape-match block, the machines now had to be activated by two and only two blocks that matched the machines in shape. Our preliminary results show that children were more successful in a free play condition as compared to a didactic instruction condition. As such, more research is necessary to understand the cognitive factors and task features that influence the success of these different learning contexts.
The current work also leaves one important area unaddressed: How should we understand children’s free play? Is there systematicity to it, such that the data generated through free play enables children to achieve specific learning goals? Investigating this question would not be a straightforward task, especially given the difficulty in identifying children’s learning goals at every moment. For example, we may set up an experiment where the goal for the child is to learn how to categorize exemplars through free play, but the child may instead be more interested, at that very moment, in how the paper cards being used to represent those exemplars move when dropped from a certain height. Their learning goals may not remain stable throughout the free play period either—it is certainly possible that children may quickly move on to investigating how resistant those paper cards are to tearing. In fact, children may even have multiple learning goals at once, or no learning goals at all! In addition, it is not clear that every action that children may take during free play is directed towards accomplishing some learning goal. When it comes to free play, there thus appears to be at least two competing goals on the learner’s part: 1) to learn about the system that one is interacting with, and 2) to enjoy that very interaction. An account of children’s real life free play behaviors remain very much elusive, and future experiments could begin to explore this aspect of self-directed activity.

7.3 Concluding Remarks

The empirical work reviewed in this dissertation provides evidence for the relevance of exploratory activity to cognitive development, lending itself to the perspective that children are active learners. At the same time, it identifies four components that contributes to active learning in early childhood: First, young learners detect when there is something to be learnt. Second, they preferentially attend to or approach the potential source of information. Third, young learners generate evidence that is relevant to the learning goal. And finally, they actually learn from the self-generated data, incorporating the observed outcomes into one’s knowledge.

This work presents a different way of thinking about active learning within development. Previous experiments on active learning have all turned the knob on one or more of these different components, and that is potentially why definitions of active learning have become so divergent over the last two decades. By pulling these components together, we bring together these different definitions and recognize that active learning is not just one unitary process, but several different processes working in concert. With such a view, we might also be able to move towards a better understanding of children may come to drive their own development.
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