

THE NATURE, DEVELOPMENT AND IMPLICATIONS OF THE 'CURSE OF
KNOWLEDGE' IN CHILDHOOD

by

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Abstract

Our ability to reason about the perspectives of others is associated with many positive life outcomes (e.g., better interpersonal relationships). Unfortunately, when reasoning about the perspectives of others, we are often biased by our own knowledge (i.e., the curse of knowledge). The curse of knowledge is a cognitive bias that limits our ability to reason about less-knowledgeable perspectives. It leads to overestimations of what others know and clouds our judgements about their beliefs. Critically, this bias is prevalent across various contexts, and it affects our social reasoning across the lifespan. Previous research demonstrated the effects of the bias on children's social reasoning, however there are several critical theoretical questions that remain unanswered. For instance, is the bias universal among children? I find support for the universality of the bias, in Chapter 2, by showing that even children of a nomadic pastoralist tribe, the Turkana, show the bias. Furthermore, in Chapter 3, I examine how age-related changes in the bias can affect young children's performance on a widely used measure of Theory of Mind—a false belief task. I examine this question in two experiments; one showing that younger children are more accurate at reasoning about other perspectives when the curse of knowledge is minimized. The other experiment showing that children's inferences are biased by their knowledge but that minimizing the bias does not improve performance. I discuss potential reasons for the discrepancy between experiments. Finally, I examine two accounts on the mechanisms underlying the curse of knowledge. In Chapter 4, I provide evidence that fluency misattribution (i.e., the tendency to attribute the fluency in processing a stimulus to an inaccurate source) is necessary for the bias to occur. Chapter 5 summarizes my findings, discusses their implications, and highlights avenues for future research.

Lay Summary

The curse of knowledge refers to the difficulty in ignoring one's knowledge when reasoning about a more naïve perspective. This phenomenon clouds one's ability to understand other individuals' perspectives, and it is reported across various settings, including legal, medical, academic, educational and interpersonal settings. In this work, I find that the curse of knowledge does not only occur among Western children, but rather it is a universal phenomenon that is independent of children's cultural upbringing. I also find that the curse of knowledge affects children's ability to realize that another person has a mistaken belief, such that children assume that the person would share their knowledge about an event outcome. Lastly, I find that children do not become swayed by their own knowledge anytime they learn new information, but rather they only find it difficult to ignore their knowledge when they learn information that seems familiar.

Preface

This dissertation is original independent research conducted by me, the author, Siba Ghrear, under the supervision of Dr. Susan Birch. In accordance with the UBC Department of Psychology 2020-2021 Graduate Student Handbook, I elected to ‘incorporate one or more manuscripts into my dissertation’ (i.e., Chapters 2, 3, & 4) of which I am the primary author. Below, I acknowledge the roles of my co-authors and indicate where the work, or parts of the work, was published. In addition to the three “research chapters”, I included substantial introductory and concluding chapters. Research conducted was approved by the Behavioural Research Ethics Board at UBC, [Certificate #: H10-01272].

A version of Chapter 1 is published, citation: Ghrear, S., Birch, S. A. J., & Bernstein, D. M. (2016). Outcome Knowledge and False Belief. *Frontiers in Psychology*, 7, 118. <https://doi.org/10.3389/fpsyg.2016.00118>. I was the lead author, and I wrote the manuscript under the guidance of Birch and Bernstein.

A version of Chapter 2 is published, citation: Cultural variations in the curse of knowledge: The curse of knowledge bias in children from a nomadic pastoralist culture in Kenya. Ghrear, Siba; Chudek, Maciej; Fung, Klint; Mathew, Sarah; Birch, Susan A. J.; *Journal of Cognition and Culture*, Vol 19(3-4), Aug. 2019 pp. 366-384. Birch designed the study, Chudek and Mathew organized the logistics of data collection, Fung and I analyzed and interpreted the data, and I wrote the manuscript.

A version of Chapter 3 is published, citation: Ghrear S, Baimel A, Haddock T, Birch SAJ (2021) Are the classic false belief tasks cursed? Young children are just as likely as older children to pass a false belief task when they are not required to overcome the curse of knowledge. *PLoS ONE* 16(2): e0244141. <https://doi.org/10.1371/journal.pone.0244141>. Birch and I designed the experiments. Haddock and I collected data. Baimel and I analyzed and interpreted the data, and I wrote the manuscript.

A version of Chapter 4 is published, citation: Ghrear, S., Fung, K., Haddock, T., & Birch, S. A. J. (2021). Only Familiar Information is a “Curse”: Children’s Ability to Predict What Their Peers Know. *Child Development*, 92(1), 54–75. <https://doi.org/10.1111/cdev.13437>. Birch and I designed the experiments. Haddock and I collected data. Fung and I analyzed and interpreted the data, and I wrote the manuscript.

Note that parts of the three latter publications were also used in Chapter 1, to provide a clear introduction of my dissertation.

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Dedication

I consider myself very lucky to have so many people in my life that I want to dedicate my dissertation to, but first I want to dedicate this to my family. I dedicate this to my parents who secured a better future for my siblings and I by making the decision to immigrate to Canada in 2000. It was at a time where my parents were doing particularly well in Syria. We had family, friends and a stable income, but my parents were able to foresee the potential instability in Syria, and they sacrificed everything for the sake of our futures. I am so lucky to have them. My mother was there for me everyday, she even supported me when she did not understand what I was doing. My father always made me feel capable as I was growing up, he made me feel like I had so much potential.

I want to dedicate this to my brother who consistently inspires me with his brilliant mind and his love for science. Every day, I am filled with joy and pride to witness his path in this world, and the kindness and love that he carries for the people around him. I dedicate this to my sister who inspires me everyday with her strength, resilience and wisdom. She fills me with love, and I am so proud to call her my little sister. Her support throughout graduate school made everything feel a little easier.

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Chapter 1: Introduction

Theory of mind, which is the capacity to reason about other people's perspectives, is essential for social wellbeing and efficient communication (see Birch et al., 2017; Korucu et al., 2017; Lagattuta, Hjortsvang, et al., 2014; Nilsen & Fecica, 2011). There are vast individual differences in this capacity, and more accurate perspective taking is associated with many positive outcomes (e.g., fewer relationship problems, higher academic achievement, more prosocial behavior; Birch et al., 2017; Caputi et al., 2012; Haddock et al., 2017; Lalonde & Chandler, 1995; Lecce et al., 2011, 2017; Repacholi et al., 2003; Smith & Rose, 2011). An important component of reasoning about other perspectives is the ability to make inferences about what others know. This ability allows us to communicate effectively, predict other individuals' thoughts, beliefs, and actions, and it allows us to make sense of another individual's point of view.

Given the importance of this ability, it is not surprising that humans can make inferences about other individuals' knowledge as early as toddlerhood (see Brosseau-Liard, 2017; Tomasello & Haberl, 2003). At 18 months of age, children are able to infer whether an adult is aware of the hiding location of an object based on whether they witnessed a researcher hiding the object or not (Buttelmann et al., 2009). By 3 years of age, children accurately infer that an individual who has seen a hidden object would know the identity of this object (Pillow, 1989; Pratt & Bryant, 1990). At 5 years of age, children can also reason about what they knew in the past and what they know now (Caza et al., 2016). Children are also able to make sophisticated decisions when inferring the knowledge states of others, such as choosing to imitate a competent versus an incompetent individual (Burdett et al., 2016), choosing to learn from a previously accurate versus inaccurate individual (Brosseau-Liard et al., 2014), choosing to learn from an

expert versus a novice (Sobel & Corriveau, 2010), and choosing to learn from a certain informant versus an uncertain informant (Brosseau-Liard & Poulin-Dubois, 2014).

The ability to make inferences about the knowledge of others is, however, susceptible to bias (i.e., a systematic distortion in thinking; Haselton et al., 2015). When individuals know a piece of information, they tend to overestimate the likelihood that others will also know this information, compared to individuals who do not know (see Guilbault et al., 2004). For example, a child who knows the hiding location of his candy may overestimate how likely his siblings are to know the hiding location as well. This bias, coined ‘the curse of knowledge’ (e.g., Camerer et al., 1989), refers to the tendency to be biased by one’s current knowledge (or belief) when reasoning about a less informed perspective. This bias has received several names in the literature depending on the particular discipline of the researcher or the specific manifestation of the bias being investigated, including: ‘hindsight bias’ (Bernstein et al., 2016), ‘creeping determinism’ (see Fischhoff, 1975), the ‘knew-it-all-along effect’ (e.g., Wood, 1978), ‘adult egocentrism’ (e.g., Royzman et al., 2003), ‘reality bias’ (e.g., Mitchell & Taylor, 1999) and ‘curse of belief’ (Ichikawa & Steup, 2001). In each case the bias described is consistent with the tendency to be influenced by one’s current knowledge when reasoning about a more naïve perspective. Therefore, hereafter, I will refer to this bias as ‘the curse of knowledge’ or sometimes simply as ‘the bias’.

A wealth of cognitive and social psychological research has investigated the pervasive nature of the curse of knowledge and its effects on social cognition and memory. The curse of knowledge is robust and widespread. It persists after explicitly warning participants about it and giving cash incentives to avoid it (Camerer et al., 1989; Damen et al., 2018; Pohl & Hell, 1996). It occurs across a variety of paradigms and information types (Blank et al., 2003; Bryant &

Brockway, 1997; Tykocinski et al., 2002), and has been documented in many applied settings, including business, education, and politics, as well as in academic writing and legal, political, and medical decision-making (e.g., Harley, 2007; Pinker, 2014).

1.1 Goals of the current research

Despite the large body of research that has demonstrated the impact that the curse of knowledge has on our ability to reason about others' perspectives, there are still several critical open questions regarding its origins and its implications for social reasoning and decision-making. Is the bias universal? What is the causal mechanism, or mechanisms, underlying the bias? What role does it play in children's Theory of Mind and social reasoning? In the program of research that forms my dissertation, I aim to address these questions to expand our understanding of the curse of knowledge. Specifically, my dissertation is composed of three empirical papers that addresses related, but independent, research questions rather than testing a single framework or hypothesis. These empirical papers include the following goals: 1. To investigate previous claims that the curse of knowledge is universal by examining the bias among non-Western children, in a culture that is maximally distinct from previously tested populations (Chapter 2). 2. To examine the effect of the curse of knowledge on children's false belief reasoning (i.e., the ability to reason about mistaken beliefs), which is argued to be the essential hallmark of Theory of Mind (Chapter 3). 3. To compare the efficacy of the fluency misattribution account and the inhibitory control account of the curse of knowledge to explain the bias in children's social reasoning (Chapter 4). 4. To examine a manifestation of the curse of knowledge that has not been tested among children; that is, to examine the effect of the bias on children's estimates of how widely known certain information is among their peers (Chapters 2 and 4).

In the following sections of this chapter, I outline the importance and rationale for each of my research questions and how the research in each chapter of my dissertation addresses my research goals. To that end, I begin with an overview of the origin of the curse of knowledge by discussing the view that the bias is a byproduct of our human learning system. According to this view, the curse of knowledge occurs when we, as humans, learn new information, and it is therefore believed to be a consequence of a universal learning system (Henriksen & Kaplan, 2003; Hoffrage et al., 2000). Subsequently, I review the literature on the three different manifestations of the bias, provide examples of each, and highlight a gap in the literature that my research aims to address. Next, I provide an overview of the links between children's Theory of Mind and their social competence, and I discuss how the most widely used Theory of Mind measure may be confounded by the curse of knowledge. Finally, I review two main accounts of the curse of knowledge: the fluency misattribution account and the inhibitory control account. In the following empirical chapters (Chapters 2 to 4) which were written as 'stand-alone' manuscripts for independent publications, I again outline the rationale of the research questions, and discuss the relevant literatures in more detail. In the final chapter of my dissertation (Chapter 5), I discuss and integrate the main findings of my empirical manuscripts. I also discuss the implications of these findings on the existing literature on children's social cognition, and I discuss the limitations of my work as well as important avenues for future work.

1.2 The curse of knowledge as a byproduct of an adaptive learning system

The curse of knowledge is argued to be a consequence of a universal feature of the human mind. Researchers have proposed that the curse of knowledge is a by-product of an adaptive learning system (Haselton et al., 2009, 2015; Henriksen & Kaplan, 2003; Hoffrage et al., 2000). Even though the curse of knowledge itself is maladaptive as it limits our ability to

understand other perspectives, this bias is believed to be an outcome of an otherwise adaptive learning system that helps us keep track of our changing environments. Our learning system prioritizes our most recently learned information and places less emphasis on outdated information or our previously held naïve perspectives (Henriksen & Kaplan, 2003; Hoffrage et al., 2000). In other words, we, as humans, live in a world that is constantly changing, and it is important for our survival to ‘keep in mind’ the most recent information about our environment. For example, if a food source has changed locations, it is more important for us to remember the new location of that food source, rather than remembering all the other locations where it used to be.

As one can imagine, this ability to track and prioritize new knowledge is critical for our daily functioning, however it appears to lead to the curse of knowledge. Researchers suggest that when we acquire new knowledge our learning system integrates the information into our existing knowledge structures so swiftly and fluently that it becomes difficult to recall or consider a more naïve perspective. Indeed, we find it extremely difficult (if not impossible) to intentionally ‘unknow’ something (e.g., to suppress information; see Golding et al., 1994), even when we try to imagine a more naïve perspective. According to this argument, the downside of our adaptive learning system is the resulting curse of knowledge, where we find it difficult to completely disregard our current knowledge when reasoning about other perspectives.

If this argument is true, then the curse of knowledge should affect individuals’ social inferences cross-culturally, and it should occur early in development (i.e., childhood), since learning occurs from the very beginning of development. Although research has reported the effect of the curse of knowledge among non-Western adults (e.g., von der Beck et al., 2017), research has yet to examine the bias among non-Western children. In Chapter 2, I address this

gap by examining the bias among Turkana children. The Turkana is a nomadic pastoralist society situated in Northwest Kenya. The Turkana children lead a very different lifestyle, compared to Western children; for example, most Turkana children do not attend formal schooling and do not have access to electricity. In their daily lives, Turkana children usually help with chores around the homestead, such as fetching water, and looking after the livestock. Given how distinct the cultural and social upbringing of Turkana children is compared to previously studied populations, examining the curse of knowledge among this population provides a solid test of the universality of the curse of knowledge.

In the following section, I discuss the manifestations of the curse of knowledge that have been reported in the literature, including the effect of the bias on one's ability to recall their previously held, more naïve, perspective (Manifestation 1), the effect of the bias on one's ability to infer another individual's naïve perspective (Manifestation 2), and the effect of the bias on one's ability to estimate how widely known information is among others (Manifestation 3). I discuss examples of each manifestation of the curse of knowledge and how it was reported among children and adults.

1.2.1 Three Manifestations of the Curse of Knowledge

1.2.1.1 Manifestation 1: The bias in recalling one's earlier perspective.

To investigate the first manifestation (also known as, 'hindsight bias' or 'the knew-it-all-along' effect) among adults, researchers typically use what has been referred to as the 'memory design' (Pohl, 2007) where they ask participants to answer a set of questions. Later, participants learn the correct answers and must recall their original answers. Participants' recollections of their original answers tend to be biased toward the newly learned answers. For example, Fischhoff and Beyth (1975) asked participants to predict the likelihood of a set of possible

outcomes of Nixon's future visit to the USSR (e.g., a joint space program between USA and the USSR). Upon learning the outcomes of Nixon's visit, participants were asked to recall their earlier predictions of the likelihood of the different outcomes. Participants' recollections were biased toward their newfound knowledge of the outcome. Specifically, participants were more likely to erroneously recall having predicted the actual outcome. In that study, and many others like it (see Blank et al., 2007; Pohl, 2007), participants' current knowledge biased their recollections of what they had previously thought.

Similarly, Taylor and colleagues (1994) found that when young children learned new information (e.g., the color chartreuse) they claimed they knew it all along. They were unable to differentiate between knowledge that they learned a long time ago (e.g., the color red), and knowledge that they learned that day (see also Sutherland & Cimpian, 2015). Importantly, young children were not just trying to 'look smart', they thought that others would know this information too. In fact, young children sometimes claim that peers, adults, and even babies will share their knowledge (Taylor et al., 1991).

1.2.1.2 Manifestation 2: The bias in judging another perspective.

To examine the second manifestation among adults, researchers typically use the 'hypothetical design' where they present participants with answers to a set of questions and then ask how another individual, who had not learned the answer, would respond (Pohl, 2007). For example, Fischhoff (1975) provided participants with descriptions of an historical event involving the war between the British and the Gurkha. Some participants did not learn the war's outcome, whereas others learned that 'The British and the Gurkha reached a military stalemate.' Subsequently, participants considered several possible outcomes, including the actual outcome. For each of these outcomes, participants estimated how likely it would be for a naïve peer to

predict that outcome. Compared to participants who did not learn the outcome, participants who knew the outcome estimated that a naïve peer would be more likely to predict that outcome of the war. In this study, and many others like it, participants' current knowledge biased their ability to infer what someone else would think (for reviews see, Ghrear et al., 2016; Hoffrage et al., 2000).

In the first developmental demonstration of this manifestation, Birch and Bloom (2003) investigated children's ability to infer another's knowledge of the contents of different boxes when the children were informed of the boxes' contents versus when they were uninformed. Three- to five-year-old children saw sets of boxes, each containing "a special thing inside." Children learned that Percy, a puppet, had seen what was inside one set, but not the other. On half the trials, children saw what was inside both sets of boxes; on remaining trials, children did not see what was inside either set, resulting in a 2 x 2 cross between Percy's knowledge (or ignorance) and the child's knowledge (or ignorance). When children knew the boxes' contents, they overestimated Percy's knowledge compared to when they did not know the boxes' contents. That is, knowledge of the boxes' contents led to overestimations about what Percy knew. The magnitude of the bias decreased significantly between three to five years of age. These age-related changes lend support to claims that the curse of knowledge may contribute to young children's difficulty with false belief reasoning—the so-called litmus test of children's Theory of Mind, or their ability to recognize that the mind can misrepresent reality (e.g., Bernstein et al., 2004; Birch, 2005; Royzman, Cassidy, & Baron, 2003), a point I discuss in Chapter 3.

Similarly, in a visual perspective taking task, Bernstein and colleagues (2007) found that knowledgeable children and adults were more likely to overestimate another individual's knowledge. In this procedure, referred to as the Visual Hindsight Bias Measure (VHB Measure;

Harley et al., 2004), 3- to 5-year-old children and adults saw degraded images of common objects that gradually clarified on a computer. In a baseline condition, participants identified an image as it clarified. In a hindsight condition, participants first saw a clear version of the image, and then estimated when another individual, a puppet named Ernie, would identify the image as it clarified. The researchers found that after seeing the clear images of the objects, participants overestimated how early, in the course of clarification, Ernie would be able to identify the degraded images. In a follow-up study examining participants from 3 to 95 years of age, Bernstein and colleagues (2011) found that the curse of knowledge followed a U-shaped trajectory across the lifespan, with young children and older adults exhibiting a greater curse of knowledge than older children and younger adults (for related research see, Bernstein et al., 2004; Bernstein et al., 2007; Lagattuta et al., 2014; Massaro et al., 2014; Pohl et al., 2018).

1.2.1.3 Manifestation 3: The bias in judging how widely known information is.

In research examining how the curse of knowledge affects people's judgments of how widely-known information is among a group of people, adult participants are presented with questions and asked to estimate how many of their peers will know the answers to each of the questions (e.g., Birch et al., 2017). Importantly, participants are told the answers to some of the questions but not others. Participants tend to estimate that more peers would know the answers to the questions that they themselves know the answers to, compared to the questions that they do not know the answers to. Critically, when participants do not know the answers, they are more accurate in estimating how widely known the information is among their peers, compared to when they do know the answers; hence, the curse of *knowledge* (see also Heine & Lehman, 1996; Nickerson et al., 1987; Pohl et al., 2002). This manifestation of the curse of knowledge has not yet been tested among children. Chapter 2 and 4 of my dissertation examine the effect of the

curse of knowledge on children's ability to reason about a group's perspective, to gain a comprehensive understanding of the bias in childhood.

1.3. The importance of Theory of Mind and the role of the curse of knowledge

As mentioned above, Theory of Mind is essential for our social wellbeing. This capacity involves the ability to recognize that others have minds, and to reason about the contents of those minds. Theory of Mind is critical for understanding human action (Frith & Frith, 2005), it also provides the foundation for cultural learning (Henrich, 2004; Herrmann et al., 2007), reduces prejudice (Shih et al., 2009), and fosters prosocial behavior (Bzdok et al., 2012; Caputi et al., 2012; Etel & Slaughter, 2019; Imuta et al., 2016). The developmental origin of this capacity in humans has intrigued philosophers and researchers for decades. When do children understand that others have minds that represent, and therefore can *mis*represent, reality? In some of the earliest work investigating this question researchers used what became known as the Sally-Anne Task (a.k.a., the Classic False Belief task, Maxi Task, or Change-of-Location task; Wimmer & Perner, 1983). In this task, children observe a scenario where a protagonist (e.g., Sally) hides an object in one location and leaves the scene. In Sally's absence, another character (e.g., Anne) hides the object in a different location. Then, children are asked where Sally will first look for the object when she returns. The logic is as follows: if children can predict that Sally will look for the object where she originally hid it—because she will not know what transpired in her absence and thus will hold a *false* belief—then they understand that a mind can misrepresent reality (Dennett, 1978; Gopnik & Astington, 1988; Perner et al., 1987; Wimmer & Perner, 1983). Across hundreds of replications of this task, and others like it (Gopnik & Astington, 1988; Liu et al., 2008; Perner et al., 1987; Wellman et al., 2001), 3-year-olds consistently failed by indicating that Sally will first look for the object where it was moved (i.e., where they know it to be),

whereas by around 4.5 to 5 years of age, children tended to correctly infer Sally's false belief (see Wellman et al., 2001, for meta-analysis).

Notably, Theory of Mind is a multifaceted capacity that is much broader than simply passing the Sally-Anne task, or reasoning about false beliefs. A Theory of Mind involves a suite of abilities including reasoning about emotions, intentions, desires, knowledge, and much more (for review, see Birch et al., 2017; Hughes & Devine, 2015; Hughes & Leekam, 2004). To examine the developmental trajectory of different Theory of Mind abilities, researchers use various measures such as the Reading the Mind in the Eyes (Baron-Cohen et al., 2001), Belief-Desire Reasoning task (Leslie et al., 2004), Strange Stories (Frith & Happe, 1994), among many others (e.g., Beaudoin et al., 2020; Dumontheil et al., 2010; Legg et al., 2017; Repacholi & Gopnik, 1997; Woodward, 1998). Together, these measures have revealed that there is important development in Theory of Mind both before and after children succeed at the Sally-Anne task.

Despite the existence of several important Theory of Mind measures, the classic Sally-Anne task is still by far the most widely used measure in the literature, especially for preschool-age children. As of 2021, the original work by Wimmer and Perner has been cited 7,816 times (Wimmer & Perner, 1983), and Wellman and Liu's Theory of Mind Scale (which includes a variant of the false belief task) has been cited 2,012 times (Wellman & Liu, 2004). For better or worse, the Sally-Anne task remains the gold standard in developmental and cognitive science providing the primary foundation for various fields of research relating to the understanding of the mind such as its neuroanatomical loci (Rothmayr et al., 2011; Sommer et al., 2007), its role in Autism Spectrum Disorder (e.g., Baron-Cohen, 2002; Durrleman et al., 2016; Joseph & Flusberg, 2004), and ADHD (see Pineda-Alhucema et al., 2018), its various cognitive and environmental correlates (e.g., Charman et al., 2002; Hughes et al., 2005; see, Milligan et al.,

2007; Perner & Lang, 1999; Sabbagh et al, 2006), and its ability to predict a whole host of social outcomes such as one's tendency to exhibit empathy and prosocial behavior (see Imuta et al., 2016), the degree of one's peer relationship problems (e.g., Capage & Watson, 2001; De Rosnay et al., 2014; Peterson et al., 2016), and one's academic success (e.g., Lecce et al., 2011, 2014). Given that performance on this task is such a widely used predictor of various social outcomes, and it forms the bedrock on which several fields of research were built, it is imperative to understand precisely what it is measuring and which aspects of cognition account for the developmental differences.

Nonetheless, there is widespread disagreement about what the Sally-Anne task is measuring. Particularly, researchers disagree on the reasons why 3-year-olds tend to fail this task, while 4.5 and 5-year-olds tend to pass. One dominant view suggests that 3-year-olds do not yet understand how the mind works; that is, they do not understand that the mind can represent, and misrepresent reality, based on one's experience (e.g., Gopnik, 1993a; Perner, 1991; Wellman et al., 2001). Another dominant view suggests that 3-year-olds' poor performance on the Sally-Anne task is a reflection of their premature cognitive abilities (e.g., working memory, language skills, inhibitory control), rather than a reflection of their concept of how a mind works (e.g., Bloom & German, 2000; Fodor, 1992; He et al., 2012). That is, by this latter view 3-year-olds can reason about false beliefs, however they have not yet developed the cognitive capacities to pass the Sally-Anne task. Under this interpretation, researchers have put forth various accounts that outline the specific cognitive limitations that lead 3-year-olds to fail the Sally-Anne task, and the cognitive development that allows 5-year-olds to pass the tasks (Apperly & Butterfill, 2009b; Astington & Jenkins, 1999; Benson et al., 2012; see Fabricius & Khalil, 2003). Indeed, researchers found that 3-year-olds' difficulty in passing the Sally-Anne task is linked to the

cognitive demands associated with the task (e.g., linguistic demands, executive functioning, attentional biases; Atance et al., 2012; de Villiers & de Villiers, 2012; He et al., 2012; Rubio-Fernández & Geurts, 2016; Setoh et al., 2016; Siegal & Beattie, 1991). Consistent with this, some research suggests that when cognitive demands are sufficiently reduced even toddlers and infants can reason about false beliefs (e.g., Buttelmann et al., 2009, 2014; Luo, 2011; Onishi & Baillargeon, 2005).

Researchers also suggest that 3-year-olds' difficulty in passing the Sally-Anne task may be a result of their difficulty in overcoming the curse of knowledge (Birch, 2005; Birch & Bernstein, 2007; Birch & Bloom, 2003; Royzman, Cassidy, College, et al., 2003). In the Sally-Anne task, children are asked to reason about Sally's naïve perspective after learning specific outcome information (i.e., the current location of the ball). In other words, children are asked to overcome the curse of knowledge to infer Sally's false belief (i.e., Sally thinks the ball is in its original location, the basket). The ability to overcome the curse of knowledge is especially difficult for younger children, who are more vulnerable to the bias (e.g., Bernstein et al., 2011; Birch & Bloom, 2003). This age-related change in the curse of knowledge may at least partially explain the widely reported finding that 3-year-olds find it difficult to reason about false beliefs.

In chapter 3, I directly examine the effect of outcome information on children's ability to reason about another individual's false belief. To do this, I design a variant of the Sally-Anne task involving four boxes rather than two. Children are told a similar scenario to the Sally-Anne scenario, where a protagonist hides a target object that is later moved by another character. In some trials, children are told the current location of the object, while in other trials, children are not told where the object was moved (i.e., it could be in any one of the remaining three boxes). I hypothesize that if the curse of knowledge impairs young children's false belief reasoning, then

young children would perform more accurately on the trials where they are *not* told the current location of the object, versus the trials where they are told that information. In other words, I suspect that the age-related change in children's performance on the classic Sally-Anne task reflects the developmental change in the course of knowledge rather than the developmental change in false belief reasoning.

1.4 The mechanism behind the bias: fluency misattribution versus inhibitory control

In Chapter 4, I investigate the effect of fluency misattribution on the course of knowledge, and I discuss the implications of my findings for the inhibitory control account of the bias. The fluency misattribution and inhibitory control accounts are viewed as separate causal explanations for the bias. The fluency misattribution account views the cause of the bias as the tendency to misinterpret the ease, or fluency, with which previously encountered information comes to mind (Harley et al., 2004). In contrast, the inhibitory control account views the cause of the bias as a failure to inhibit the contents of one's knowledge. Indeed, the inhibitory control account suggests that the course of knowledge affects our social judgements, because we, as humans, find it difficult to suppress or inhibit our own privileged knowledge and reason about naïve perspectives (e.g., see Bayen et al., 2007). For example, if you know the location of the Trevi Fountain, you must inhibit that information (Rome, Italy) to make accurate inferences about others' knowledge. According to this account, an inability to adequately inhibit your knowledge contributes to the bias. Consistent with this, researchers found that children's performance on inhibitory control tasks is correlated with their performance on course of knowledge tasks (e.g., Lagattuta et al., 2010; Nilsen & Graham, 2012); such that children who were able to inhibit a prepotent (or dominant) response were also more likely to show a smaller magnitude of the course of knowledge. Indeed, inhibitory control is also predictive of children's ability to pass the Sally-

Anne task (e.g., Carlson et al., 2004; Carlson & Moses, 2001; Chasiotis et al., 2006), such that children who can override the tendency to give a response that is consistent with reality, can accurately infer Sally's false belief. Inhibitory control, as a cognitive skill, develops across childhood and deteriorates with older age (e.g., Groß & Bayen, 2015), consistent with the age-related changes associated with the curse of knowledge where young children and aging adults are more affected by the bias (e.g., Bernstein et al., 2011).

The fluency misattribution account of the curse of knowledge suggests that the bias occurs upon learning information that is fluent (i.e., easy to process), and we mistake this fluency as indicative of this information being obvious to others (Bernstein et al., 2018; Birch et al., 2017; Harley et al., 2004). In other words, we misattribute the subjective ease with which a piece of information comes to our mind as indicative of how easy or foreseeable the information is to others. According to a fluency misattribution account, the curse of knowledge is not due to a failure to inhibit the content of one's knowledge, but rather a tendency to misinterpret the ease with which the information comes to mind (or the feelings of fluency associated with the information). For example, if one knows that the Trevi Fountain is in Rome, one must be able to discount the ease with which this information comes to mind to make accurate inferences about others' knowledge. Indeed, research with adults suggests that the more fluency adults experience in processing certain information, the more likely they are to show the curse of knowledge in their social reasoning (e.g., Harley et al., 2004).

Although fluency misattribution results in errors in a variety of judgments in perceptual, conceptual, and social reasoning (e.g., Begg et al., 1985; Fazio et al., 2019; Whittlesea et al., 1990), it stems from an otherwise useful heuristic, namely fluency *attribution* (Whittlesea, 1993). Information that has been processed before (due to prior exposure) is processed more fluently, so

we, as humans, use our metacognitive awareness of that fluency as a cue to previous exposure (Jacoby & Whitehouse, 1989; Whittlesea, 1993). As such, fluency attribution serves an important memory function by signaling that we have previously encountered the information. In line with this, researchers have shown that when participants process information fluently they attribute those feelings of fluency to prior exposure (e.g., Jacoby & Dallas, 1981). Fluency *mis*attribution occurs when the subjective feeling of fluency is attributed to an incorrect source. Consistent with this, research suggests that the inability to recall the source of one's information leads to fluency misattribution (e.g., Jacoby et al., 1989). That is, fluency misattribution appears to be a type of source memory error, where one does not recognize how the information became fluent and misattributes this fluency to an inaccurate source. If fluency misattribution is a type of source memory error, that may partially explain why the curse of knowledge is greater in younger children than older children, given that source memory is more error prone in younger children (Earhart & Roberts, 2014; Gopnik & Graf, 1988).

Despite a large body of evidence showing the effect of fluency misattribution on social judgements among adults (e.g., Bernstein et al., 2018; Birch et al., 2017), this effect has not been examined among children. I fill that gap by investigating the effect of fluency misattribution on children's judgement of what their peers know. Particularly, I examine the effect of fluency, stemming from familiarity (i.e., previous exposure to a topic), on the occurrence of the curse of knowledge among children. That is, can fluency, resulting from familiarity, lead to misattributions about how widely known certain information is among peers? I examine this question by manipulating the familiarity of the information provided to children and examining whether it leads to a curse of knowledge effect among children. For example, I present children with familiar information (e.g., the *eagle* can fly the highest), versus unfamiliar information (e.g.,

the *Ruppel's vulture* can fly the highest), and I test the hypothesis that children would only be cursed by their knowledge upon learning familiar information. The logic here is that if the fluency misattribution account of the curse of knowledge is accurate, then children would only show the curse of knowledge effect upon learning familiar, and therefore fluent, information. On the other hand, if children show the curse of knowledge regardless of the familiarity of the information they learn, then the fluency misattribution account is not a valid explanation of the curse of knowledge.

To date, the fluency misattribution and inhibitory control accounts have been discussed as separate explanations for the curse of knowledge. The fluency misattribution account on the bias has been exclusively examined among adults (Bernstein et al., 2018; Bernstein & Harley, 2007; Higham et al., 2017), whereas the developmental literature has mainly examined the inhibitory control account in attempting to understand the bias in children (Bernstein et al., 2011; Lagattuta et al., 2014; Nilsen & Graham, 2009, 2012). Although I hypothesize that fluency misattribution would be the causal mechanism underlying the bias, I also believe there is value in considering how inhibitory control can come into play. I suspect that once the curse of knowledge occurs, as a result of fluency misattribution, inhibitory control can help lessen the magnitude of the bias. In other words, the bias may not occur unless individuals mistake the fluency in processing information as indicative of how widely known that information is, however, once the bias occurs it is possible to reduce the bias through inhibitory control— an important point I return to in Chapter 4 and in the General Discussion.

Chapter 2: The Curse of Knowledge among Turkana children

Although the curse of knowledge leads to errors in our social judgements, researchers propose that it is a byproduct of an innate and adaptive learning system that prioritizes efficient learning (Haselton et al., 2009; Henriksen & Kaplan, 2003; Hoffrage et al., 2000). Note, by innate, I mean it does not require specific environmental learning to develop. Upon learning new information, one's understanding of the world is swiftly updated, making it difficult to accurately recall, or imagine, a naïve perspective. As one can imagine, this learning system is critical for everyday functioning in our changing environment. In our daily lives, events are constantly unfolding, and it is important to keep track of what is happening in the current moment, in order to respond and navigate our environment strategically. The downside of this otherwise adaptive learning system is that it is difficult to fully disengage from our current knowledge when reasoning about a naïve perspective.

If the curse of knowledge is truly an outcome of our innate human learning system, then we would expect that this bias would affect individuals' social inferences cross-culturally. In other words, the curse of knowledge should be universal to humans regardless of their social environment, or culture. In support of this, there is evidence that the curse of knowledge occurs cross-culturally among adults (e.g., Choi & Nisbett, 2000; Heine & Lehman, 1996; Pohl et al., 2002; von der Beck et al., 2017; Yama et al., 2010). Researchers have demonstrated the robustness of the curse of knowledge among adult participants from North America, East Asia, Europe, and Australia, though there are mixed results on whether the magnitude of the bias varies from one culture to another (see, Choi & Nisbett, 2000; Heine & Lehman, 1996; Pohl et al., 2002; Yama et al., 2010).

Notably, however, the curse of knowledge has not yet been studied in children from a non-Western culture. Theoretically, it is important to examine whether the curse of knowledge affects children's social reasoning cross-culturally, as this research would lend credence to the claim that this bias is a universal feature of the human mind. Specifically, if the curse of knowledge affects non-Western children's social reasoning, then we have evidence to suggest that this bias is not tied to some unique aspect of Western children's environments, but rather it occurs cross-culturally among children. Indeed, one could argue that Western children show a curse of knowledge because there are features in their society (e.g., formal schooling, media) that allow for shared experiences and knowledge. That is, it would make sense for a Western child to overestimate how many of their peers would share their knowledge, given that their peers go to school where they attend similar classes and learn the same information. It is also likely that the child's peers would watch the same T.V. shows and movies and possibly play the same online games. Therefore, it is important to examine the bias in other cultures, or societies, where children are not influenced by the same societal practices. Note that while revealing the curse of knowledge among non-Western children would support the view that this bias is a universal feature of the mind, it cannot confirm that it is an outcome of an evolutionarily adapted human learning system, *per se*. Nonetheless, if the bias is not evident in non-Western children's social reasoning, then we have evidence to suggest that the bias is *not* a part of an evolutionarily adapted human learning system, because it would not be shared by all humans.

There are two reasons to examine the curse of knowledge cross-culturally among children. First, the view that the bias is a by-product of our human learning system suggests that the curse of knowledge should affect our social reasoning as soon as we begin learning, and we begin learning from the very beginning of development (Bower, 1989). Accordingly, this view is

not only contingent on the finding that the curse of knowledge is evident cross-culturally among adults, but it is also contingent on the assumption that it is evident cross-culturally among children. Second, by examining the curse of knowledge cross-culturally among children, I can confirm that the curse of knowledge is not a result of cultural learning or socialization that occurs in adulthood. For instance, one can argue that adults tend to overestimate the likelihood that others would share their knowledge, because over time they learn that a lot of knowledge is shared (e.g., knowledge of cultural norms, knowledge of where to get food or where to go when one's sick, etc.) It is therefore important to confirm that the curse of knowledge exists cross-culturally among children, to ensure that the bias is not a result of cultural learning.

2.1 Hypothesis and Goals of the current research

In line with the view that the curse of knowledge is a byproduct of an innate human learning system (Haselton et al., 2009, 2015; Henriksen & Kaplan, 2003; Hoffrage et al., 2000), as well as the evidence suggesting that this bias occurs cross-culturally and across the lifespan (Bernstein et al., 2011; Groß & Bayen, 2015a, 2015b; Groß & Pachur, 2019; Heine & Lehman, 1996; Jung, 2020; Lagattuta et al., 2014; Pohl et al., 2018; Yama et al., 2010), I hypothesize that children would be biased by their knowledge, regardless of their specific cultural upbringing. That is not to suggest that there are no cultural differences in the magnitude of the curse of knowledge. It is possible that certain cultural experiences can attenuate the magnitude of the curse of knowledge in individuals' social reasoning. It is also possible that the developmental trajectory of the curse of knowledge varies depending on children's cultural experiences. For instance, the reported age-related change in the curse of knowledge among Western children (e.g., Bernstein et al., 2011; Birch & Bloom, 2007; Lagattuta et al., 2014) could be a result of Western specific cultural experiences, such as the teaching of certain topics in the Western

education system. For instance, children's cognitive skills may improve as children progress through the school system and learn topics such as advanced math and history; the learning of these topics may improve children's capacity to overcome the curse of knowledge. Accordingly, as a secondary question, I aim to examine whether the developmental trajectory of the curse of knowledge is specific to Western cultures, or if it is present in non-Western cultures as well.

To investigate the universality of the curse of knowledge, my colleagues and I examine the existence of this bias in Turkana children, who have a very different cultural and social upbringing compared to Western children. The Turkana are a nomadic Nilo-Saharan pastoralist society in East Africa, who have minimal influence from the Western world. The Turkana children's upbringing is distinct from the upbringing of Western children in many important ways. For one thing, the Turkana children's school attendance is low; indeed, in our current sample only 17% of children attended school, and they only attended school for one or two years. Also, most Turkana children do not have access to electricity, and therefore they do not have access to television, or iPads and video games. Given how distinct they are from previously tested populations, we reasoned that the Turkana population would be a great population to test whether the curse of knowledge is universal among humans, or whether the bias is a cultural artifact of Western environments. Below, I provide more information about the Turkana society for the reader to gain a more comprehensive on their cultural and social background.

2.2 *The Turkana*

The Turkana are situated in the Turkana county in Northwest Kenya and comprised of approximately one million people. They occupy a semi-arid savanna habitat in northwest Kenya herding cattle, camel, sheep, goat and donkey (see, Mathew & Boyd, 2011). A homestead is comprised of the male household head, his wives, and children, and possibly other relatives

living under his care. Turkana settlements are a collection of a few dozen homesteads. Settlements periodically break apart in the dry season as households, especially young adult males of the household, form temporary mobile camps that migrate to access fresh pastures and dry season water wells. Political organization is decentralized, and it is built around patrilineal clan groupings, and around age-sets—cohorts of men born within a 5 to 6-year period (Gulliver, 1958). Senior age-sets have authority over junior sets, but no single individual is vested with coercive authority. Post marital residence tends to be patrilocal, and marriage is polygynous.

The Turkana rely on the pastoral sector to make a living; however, they have limited market integration. Their subsistence is primarily based on livestock products such as milk, meat and blood, as well as supplies of maize flour, legumes, sugar, and oil that they purchase from town (Little & Leslie, 1999; McCabe, 2004). To purchase such supplies, they sell charcoal that they produce, or they sell one or more stock animals from their herd. Interestingly, in the northern segments of Turkana county, where my colleagues and I conducted the current study, there is a large refugee camp run by the UNHCR. The Turkana sell their stock and purchase supplies from the refugee camp regularly. Accordingly, the presence of the refugee camp has increased the market transactions that the Turkana in this area engage in.

The Turkana are a natural fertility population, and most births occur at home. The Turkana are only weakly influenced by the Kenyan nation state. Most Turkana in the pastoral sector have minimal access to medical care as dispensaries are sparse, and hospitals and clinics are in the town centers. There are no tarmac roads except for one highway that goes through Turkana county into South Sudan. Disputes and violations are typically handled by a gathering of the Turkana elders, and there is little reliance on the courts or police of the Kenyan state to maintain order.

The Turkana language is related to the Karimojong language group of Nilo-Saharan languages, and most Turkana who have not been to school do not speak Kiswahili, the national language of Kenya. Christianity began to spread in this region over the last half century, and most Turkana will identify as Christians. There is considerable variation in adherence and practice, with some primarily practicing the local spiritual system, others incorporating both, and a minority adopting Christian beliefs at the exclusion of local spiritual beliefs.

School attendance rates among the pastoralist Turkana is low, but not nil. A typical pattern is for a family to send a 1 or 2 of their children to school and have most of the children remain in the pastoral sector as the children are critical for labor. Several boys are needed for herding as the different stock animals need to graze in different pastures. Girls are needed to help with household tasks that women do, including fetching water, processing food, building and repairing the huts, charcoal production, taking care of younger siblings, and watering the livestock at the wells. The value of children for herding is the main reason for the low rates of school attendance. Even children who are sent to school are frequently taken back for herding as the needs of the household changes (for more information on the Turkana culture see, Mathew, 2017; Mathew & Boyd, 2011).

2.3 *Present Study*

In the present study, my colleagues and I examine whether the curse of knowledge affects Turkana children's estimates of how widely known information is among their peers. Using a design similar to that used with adults (e.g., Birch et al., 2017), we present children with eight factual questions and teach children the answers to half of the questions. After each question, we ask children to estimate how many of their peers would know the answer (e.g., how many Turkana children, out of ten, will know how many Turkana towns there are?); henceforth, I will

refer to their estimates as ‘peer estimates’. My colleagues and I predict that the Turkana children would make higher peer estimates for questions that they were taught, compared to questions that they were not taught. That is, upon learning the answers to the questions, I predict that the Turkana children would show the classic curse of knowledge effect and overestimate how widely known the information is among their peers. I also predict that, like Western children, Turkana children would show a decline in the magnitude of the curse of knowledge with age.

2.4 *Method*

2.4.1 *Participants*

Forty Turkana children belonging to the Ngiyapakuno territorial section of the Turkana were recruited from a northern area of the Turkana County, Kenya (see Figure 2.1, for an image of the site where the study was conducted). Participants’ ages ranged from approximately 2 to 10 years of age (Mean = 5 years, 6 months; SD = 2 years, 3 months). The Turkana do not usually note their date of birth using the Western calendar. The Turkana calendar names each season (which can span a year or more depending on droughts) and does not always differentiate between children who are one year apart. Therefore, the participants’ ages were estimated by consensus judgements of local research assistants based on the appearance and developmental stage of the child as well as by incorporating local knowledge of who is older among two children. All participants were of Turkana descent, and 50% of them were girls. Post-hoc power analyses revealed that there was a 74% chance of finding a main effect of the curse of knowledge, if it existed.



Figure 2.1. An image of the site where my colleagues and I examined the curse of knowledge among Turkana children.

2.4.2 Materials and Design

Participants were presented with a total of eight factual questions (see Table 2.1). The eight questions were divided into two sets of four (henceforth Set A vs. Set B), and they were roughly matched based on content (e.g., both Set A and Set B contained a question on technology and a question about human biology) and difficulty. Half of the children were taught the answers to Set A questions in the Teaching Phase of the experiment (e.g., ‘How many bones are in the body?’; Question Set A), and half of the children were taught the answers to Set B questions in the Teaching Phase (e.g., ‘How old is the oldest person alive?’; Question Set B). The questions were presented in a predetermined computer randomized order for each participant, for both the Teaching and Peer Estimates Phases.

The questions and the written script used by the researchers during the experiment were first created in English, then transcribed by a local research assistant to the Turkana language,

and finally transcribed back to English to ensure the intended meaning remained intact in the translation process. Ten small paper tokens with stick figures drawn on them were presented to participants to assist the children in providing their answers.

2.4.2 Procedure

A local research assistant administered the experiment with each child individually in a quiet location in their Turkana village in the child's native Turkana language. The experiment involved asking the children to estimate how many of their peers would know the answers to different factual questions (e.g., how many Turkana children, out of ten, will know how long snakes live?), so the researcher first administered a short pre-test to gauge the child's numerical and verbal abilities. In this pre-test, the researcher held up eight paper tokens and asked the child how many tokens he or she was holding. If the child responded correctly then he or she was allowed to provide verbal answers to the later questions, otherwise the child was asked to use paper tokens to indicate his or her answers non-verbally. The research assistant then administered three training trials demonstrating how to use the tokens to make a peer estimate. One training trial demonstrated how to use the tokens if they thought it was true of all of their peers. For example, the researcher asked, "how many children can sit?" Then, the researcher demonstrated that since all children can sit, they would choose all ten of the ten paper tokens. Two additional training trials were used to demonstrate how to use the tokens to indicate a small number of peers and how to use the tokens to indicate a medium number of peers.

In the Teaching Phase, the research assistant taught the child one set of facts (e.g., the answers for Set A questions) in an engaging manner through an animated discussion. Subsequently, in the Peer Estimate Phase, the researcher presented the child with all eight factual questions. After each question, the researcher asked the child to estimate how many out of ten

peers would know the answer. Depending on the child's success on the pre-test (see above), they either indicated the peer estimates using paper tokens or responded verbally. At the end of the study session, each child was thanked for his or her participation and rewarded with a piece of candy.

2.5. Results

A series of multilevel regression analyses were conducted using the lme4 package (Bates et al., 2007) in the R environment (R core development team, 2016) to examine whether age, gender, or Gender \times Age moderated the curse of knowledge. For all analyses, participants' peer estimates, nested within each participant, were entered as the dependent variables. Maximum likelihood was used for estimation. Intercepts were random.

To examine the effect of the curse of knowledge on peer estimates, I coded peer estimates for factual questions that were taught as known ($= .5$), and I coded the peer estimates for the factual questions that were not taught as unknown ($= -.5$). I began our analyses by examining whether gender had any effect on the curse of knowledge. To test this, I compared a full model, which included known versus unknown items, gender (boy = $-.5$, girl = $.5$), and Known versus Unknown \times Gender, with a reduced model without the interaction term. Results suggested that the full model provided a better fit compared to the reduced model, $\chi^2(1) = 12.52$, $p < .001$. In the full model, the intercept, that is, the overall mean number of estimates was 3.38 peers, $\beta = 3.38$, $SE = .28$, $t(39) = 12.17$, $p < .001$. There was a main effect of the curse of knowledge (Cohen's $d = 0.37$), that is, peer estimates were higher by $.50$ for known versus unknown items, $\beta = .50$, $SE = .21$, $t(39) = 2.35$, $p = .024$ (see Table 2.1, for an overview of mean peer estimates for each factual question presented by condition). Participant gender did not significantly predict peer estimates in general, $\beta = -.59$, $SE = .55$, $t(39) = -1.07$, $p = .29$. However, there was a

significant moderation of the curse of knowledge by gender, $\beta = 1.60$, $SE = .43$, $t(39) = 3.73$, $p < .001$ (see Figure 2.2, for a visual illustration). That is, girls did not exhibit a curse of knowledge in their peer estimates whereas boys were cursed by their knowledge. Of course, it is possible that the effect size of the curse of knowledge is much smaller among Turkana girls, compared to Turkana boys. Considering that 20 girls participated in our current sample, we had 36% chance of detecting a small effect of the bias, if it existed.

Table 2.1

Turkana Children's Mean Peer Estimates for Factual Questions that were Known vs. Unknown

Set	Factual Question	Mean Peer Estimates for Known Items	Mean Peer Estimates for Unknown items
A	How many Turkana towns are there?	4	2.71
	How do you make candy?	3.58	2.19
	How long does a snake live?	5.16	2.81
	How many bones are in the body?	3.63	2.67
Set A Average		4.09	2.60
B	How do we make oil/petrol for cars?	2.67	3.32
	How many babies does an elephant have?	3.29	2
	How old is the oldest person alive?	3	4.89
	What is the most northern Turkana town?	3.95	4.26
Set B Average		3.23	3.62

Note. I compared children's peer estimates for the known items (i.e., items that were taught) in Set A with the unknown items in Set B, and vice versa to create within subjects scores using all 8 of the child's peer estimates across items. An alternative way to compute the magnitude of the bias, to control for item difficulty or variability, is to create a difference score for each item by subtracting the sample mean peer estimate when the item was Unknown from each participant's peer estimate when the item was Known. The analyses using this alternative approach revealed the same overall pattern of results, except a Set effect was also observed whereby the questions in Set A produced the more robust curse of knowledge effect (see page 137 for a discussion).

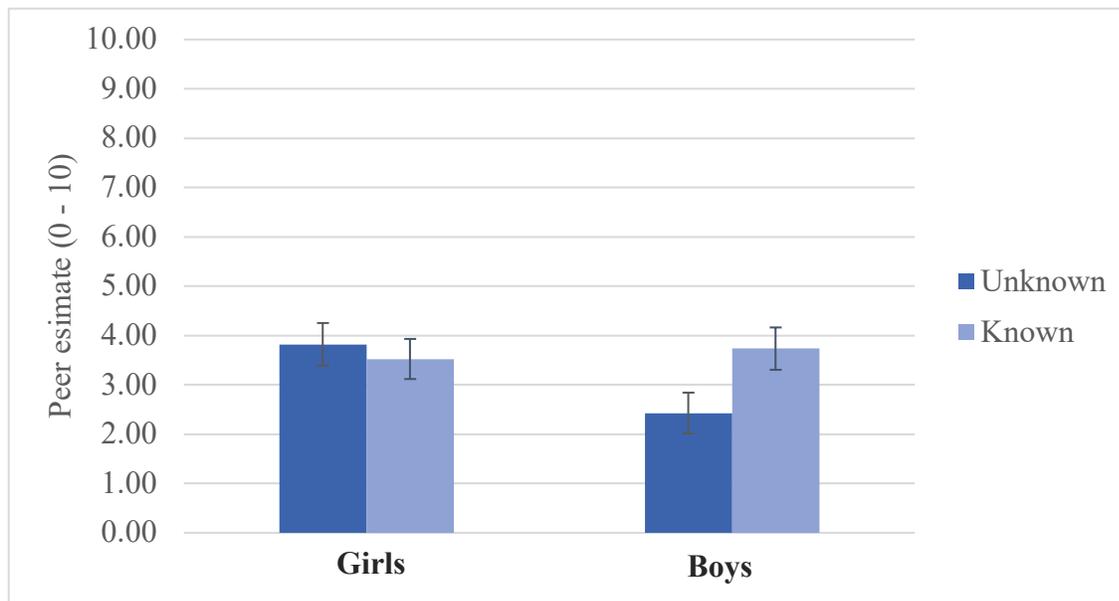


Figure 2.2. Turkana children’s mean peer estimates by condition (known vs. unknown items) and gender (boys vs. girls). Note. Error bars indicate the standard error of the means for peer estimates.

Next, I examined whether age moderated the curse of knowledge effect on peer estimates. A full model, which included known versus unknown items, centred age, and Known versus Unknown \times Age, was compared to a reduced model without the interaction term. The full model was not significantly different from the reduced model, $\chi^2(1) = 0.30, p = .59$, suggesting that age did not moderate the effect of the bias on peer estimates (see Figure 2.3, for a visual demonstration of peer estimates by age and condition). To further test the main effect of age on peer estimates across conditions in general, a full model, which included known versus unknown items and centred age was compared to a reduced model without centred age. Results were not significant, $\chi^2(1) = 0.80, p = .37$, suggesting that age did not predict peer estimates.

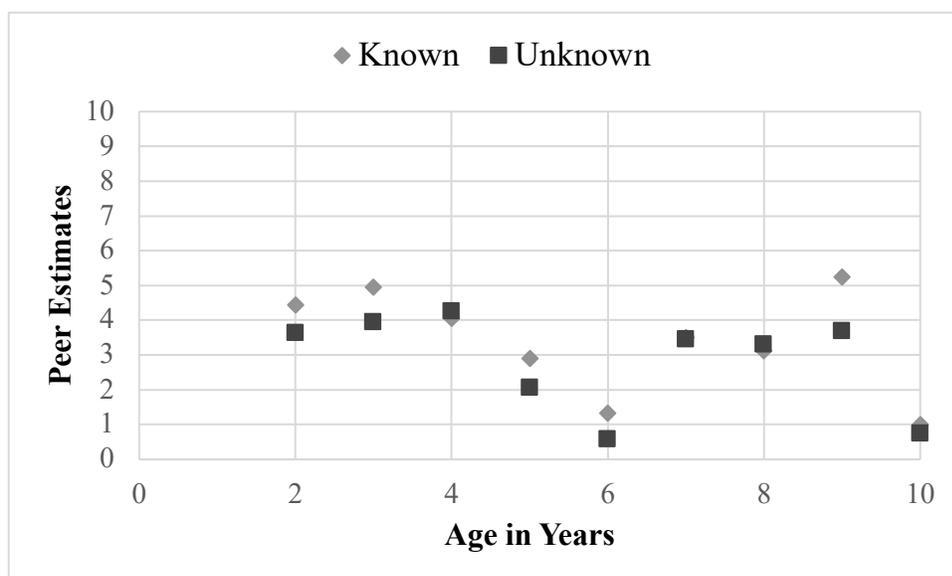


Figure 2.3. Turkana children's mean peer estimates by condition (known vs. unknown facts) and age (2 – 10 years). Note, the numbers of children for each age group are not equal.

Lastly, I examined whether gender moderated the effect of age on the curse of knowledge. I reasoned that it might be the case that age only predicted the curse of knowledge effect among boys, given that only boys showed the bias. Accordingly, I tested whether Gender \times Age moderated the effect of the curse of knowledge on peer estimates. A full model, which included known versus unknown, gender, centred age, and all two- and three-way interactions between the variables, was compared to a reduced model without the three-way interaction term and was also not significant, $\chi^2(1) = .44, p = .51$, suggesting that Gender \times Age did not moderate the bias on peer estimates.

I conducted power analysis on the non-significant effect of age using guidelines provided by Hox and colleagues (2010, pg. 239 - 240). I found that there was power of approximately .90 to detect a medium effect (i.e., explaining .1 of the between-participant variance) in the sample if such an effect existed. Note, I suspected a medium effect size of age on the curse of knowledge bias because previous research reported a medium effect size (e.g., Birch & Bloom, 2003;

Lagattuta et al., 2014). Nonetheless one may argue that our analyses were based on a sample size of 20 (rather than 40), given that half of the sample (the girls) did not show the bias. To address this concern, I ran the power analyses again assuming a sample of 20, and I found that there was power of approximately .34 to find the effect, if it existed. Further research is needed to confirm whether age is predictive of the curse of knowledge among Turkana children.

2.6. Discussion

The current experiment provides the first evidence of the curse of knowledge in a non-Western population of children, specifically the Turkana people of Kenya, Africa. In our experiment, Turkana children were asked to estimate how many of their peers (i.e., other Turkana children) would know the answers to various factual questions. Critically, children were taught the answers to half of the questions moments before they were asked to make peer estimates. Overall, my colleagues and I found that the Turkana children were biased by their newfound knowledge when reasoning about what their peers were likely to know. That is, they were significantly more likely to estimate that their peers would know the answers to the questions they had just learned, compared to the questions they themselves did not know.

The Turkana represent a population considerably different from the Western Educated Industrialized Rich Democratic (WEIRD) populations (Henrich et al., 2010). As described earlier, the Turkana are minimally integrated to the market economy. They are a pastoral nomadic tribe who have minimal access to the media, as well as minimal Western influence. Furthermore, the Turkana have very limited formal education. Evidence of the curse of knowledge in this population adds great weight to previous claims that this bias is a fundamental, universal, cognitive bias in human reasoning (e.g., Henriksen & Kaplan, 2003; Pohl et al., 2002) and is consistent with the notion that it is a by-product of an adaptive learning system. However,

that does not mean that the curse of knowledge is unaffected by environmental factors. Quite the contrary; it appears that environmental and cultural factors play a significant role in the magnitude of the bias, its developmental trajectory, and in how the bias manifests itself in various socio-cultural interactions. Notably, data from the Turkana sample revealed two important differences from previous findings with Western children. First, I found a significant gender difference, whereby only the boys (50% of the sample) exhibited a significant curse of knowledge when reasoning about the knowledge of their peers. Second, there was no evidence of a developmental decline in the magnitude of the curse of knowledge.

What do these cultural differences tell us? First, I will discuss the gender difference in the curse of knowledge. I did not expect that the Turkana girls would not show an effect of the bias in their social judgements, given that gender differences had not previously been found in research on the curse of knowledge. Nonetheless, a gender difference whereby females are more accurate than males at reasoning about the mental states of others is consistent with a body of work using other mental state reasoning, or social perspective taking, tasks (e.g., Baron-Cohen, 2002; Baron-Cohen et al., 2005; Baron-Cohen & Wheelwright, 2004). Accordingly, the tendency for the Turkana girls not to be biased by their own knowledge may be a part of a more general cross-cultural pattern where females are somewhat better at social reasoning on average than males.

I also suspect that cultural differences in the division of labor for Turkana boys, compared to girls, accounts for at least some of the gender difference observed in the curse of knowledge among the Turkana children. Turkana girls tend to spend more time within their communities interacting with other individuals compared to Turkana boys. For example, Turkana girls are often responsible for taking care of their younger siblings. On the other hand,

from early on, boys tend to be involved in more solitary tasks such as herding animals. This gender difference in their social interactions may afford girls more opportunities than boys to learn about the knowledge states of others, providing input that would assist them in overcoming the curse of knowledge in their social reasoning. Note, for instance, that while Turkana girls take care of their younger siblings, they may be routinely recognizing, and reasoning about, their siblings' more naïve perspectives. That is, they may have more practice overcoming the curse of knowledge to take the perspectives of their younger siblings.

It is also possible that what is gained by the Turkana girls in their cultural interactions is not necessarily social skills, per se, but other cognitive skills, such as improved inhibitory control or greater source memory, both of which are believed to play a role in overcoming the curse of knowledge in Western children (e.g., Birch & Bernstein, 2007; Taylor et al., 1994). For example, it could be that the Turkana girls were better at recognizing the source of their knowledge (i.e., that they had just learned the answers themselves) due to gender differences that exist in episodic memory (e.g., Grysman & Hudson, 2013; Herlitz et al., 1997), and as a result were less prone to fluency misattribution and were better at recognizing that others would not share that knowledge. Research examining gender differences in inhibitory control, source memory, critical thinking, and other social perspective taking tasks in Turkana children versus Western children could be fruitful avenues for future research.

In addition, future research would benefit from examining the conditions under which Turkana girls are susceptible to the curse of knowledge in their social reasoning. If the bias is truly a fundamental part of human cognition as the evidence suggests, then one would expect everyone to exhibit the bias under certain conditions. For example, I suspect that lengthening the time period between teaching the children the facts and asking them to make their peer estimates

would result in a greater curse of knowledge effect, one even the girls may have difficulty overcoming. The longer the information is known, the more entrenched and fluent that information becomes, and the harder it is to recall the source of one's knowledge and recognize the privileged nature of that information (e.g., Birch et al., 2017).

What do the cultural differences in the developmental trajectory of the curse of knowledge bias tell us? Before discussing the effect of age on the curse of knowledge, it is important to remind the reader that children's ages in the current sample were estimated, as the Turkana do not use the Western calendar to note birth dates. Accordingly, the finding that age does not moderate the bias should be interpreted with some caution. Also, it is possible that I did not find the effect of age in Turkana children's social estimates because the sample size was too small. Given that there was no evidence of the bias among Turkana girls assessing the effect of age relied on half of our sample.

While this question deserves further investigation, it is important to consider the possibility that age truly does not affect the magnitude of the bias among Turkana children. One possible explanation for this may be that most Turkana children do not attend formal education that is taught using a curriculum. For example, it is possible that the teaching of some topics (e.g., math) in Western education contributes to the improvement of children's cognitive capacity to overcome the bias. Indeed, there is a stark difference in school attendance in the Western world compared to the Turkana. For instance, according to the National Center for Education statistics in 2015, around 90% of American children are enrolled in kindergarten by 5 years of age. Comparatively, only 17.5% of children in the current Turkana sample completed 1 or 2 years of formal education. The topics taught in Western education could contribute to a decrease in the magnitude of the curse of knowledge in any number of ways, such as through

increases in critical thinking, inhibitory control, or source memory, to name a few. It is also possible that other environmental differences between the two cultures (e.g., differences in informal education, family and sibling interactions, language, etc.) contribute to these cultural differences in the course of knowledge. Pinpointing the causative agents that account for these cultural differences will be important avenues for future research now that the existence of cultural differences in the course of knowledge have been established.

It is important to acknowledge, however, that there is more than one way to interpret developmental data across cultures. The above explanations are under the assumption that the course of knowledge decreases at an earlier age among Western children. However, because these test questions (e.g., How many Turkana towns are there?) could not be used with a Western sample, it is difficult to say with certainty how the developmental trajectory of the course of knowledge would compare across the two cultures. It is possible that even the youngest Turkana children exhibit a course of knowledge similar in magnitude to older Western children. That is, without a Western comparison group using the same test questions we cannot rule out the interesting possibility that Turkana children are less cursed earlier in development than Western children. Future research would benefit from a replication of this study using test questions that would be appropriate for both Turkana children and Western children. For now, the only strong conclusion we can draw from the age-related data is that there is not a developmental decline in the magnitude of the curse of knowledge in Turkana children between ages 2 and 10, as has been observed in Western children.

This is not the first study to discuss cultural differences in the course of knowledge. Some researchers reported cross-cultural differences in the magnitude of the bias among adults, such that individuals from Eastern cultures (e.g., South Korea, Japan), showed a bigger magnitude of

the curse of knowledge in their social inferences, compared to individuals from Western cultures (e.g., Canada, Germany; Choi & Nisbett, 2000; Yama et al., 2010). Choi and Nisbett (2000) suggest that Eastern people tend to understand the world in a holistic mindset, where every given object is influenced by all the different aspects of its environment, as well as its interactions with the environment. This mindset gives Eastern people a deeper understanding of complexity, which in turn leads to less surprise (and perhaps more fluency) when learning new information, compared to Western people, and so Eastern individuals tend to integrate new information seamlessly into their knowledge of the world (Choi & Nisbett, 2000). The researchers suggest that Western people, on the other hand, tend to show more surprise when they learn new information, and they tend to think more analytically about the information. Accordingly, Western individuals are less biased by their newfound knowledge when reasoning about other perspectives. This view, however, is contradicted by other researchers who found that Eastern individuals are equally biased by their knowledge as Western individuals (Jung, 2020; Pohl et al., 2002), or that Eastern individuals are less biased by their knowledge compared to Western individuals (Heine & Lehman, 1996), warranting further research in this area.

2.6.1 Summary

The current experiment examined the curse of knowledge in the Turkana people of Kenya, Africa. To my knowledge, this is the first experiment to examine, and find, evidence of the curse of knowledge in children from a Non-Western sample lending the critically needed developmental support to previous arguments that this bias is a fundamental, universal, part of human cognition. While the current study reports evidence of the universality of the bias in children's estimates of what others know (Manifestation 3), the other manifestations of the curse of knowledge should also be universal, considering that they are all manifestations of the same

underlying bias. Interestingly, I found two important cultural differences in the bias in the Turkana children compared to Western children, demonstrating the key role that culture plays in the presentation, or magnitude, of this bias. First, only Turkana boys were significantly biased by their knowledge when gauging what their peers would know. Second, unlike Western children there was no developmental decline in the magnitude of the bias with age. That is, the older Turkana children, specifically boys, were as biased by their knowledge when reasoning about the knowledge of their peers as the younger children in the sample. Although not without limitations (as noted above), these data offer new insights into the origin and cognitive mechanisms involved in social perspective taking and provide the impetus and groundwork for much-needed future research in this area.

Chapter 3: The Effect of the Curse of Knowledge on False Belief Reasoning

For decades, researchers and philosophers have been intrigued by the development of Theory of Mind (i.e., the ability to reason about the mental states of others; see Birch et al., 2017). That is, when do children begin to recognize that others have minds that can represent or misrepresent reality? The capacity to reason about the mental states of others forms the bedrock of social competence, as it allows us to understand other individuals' behavior (Frith & Frith, 2005), it facilitates social and cultural learning (Henrich, 2004; Herrmann et al., 2007), it leads to better academic achievement (see Lagattuta, Hjortsvang, et al., 2014), and it promotes prosocial behavior (Bzdok et al., 2012; Caputi et al., 2012; Etel & Slaughter, 2019; Imuta et al., 2016). As of yet, the most widely used measure to examine this capacity among children is the Sally-Anne Task (a.k.a., the Classic False Belief task, Maxi Task, or Change-of-Location task; Wimmer & Perner, 1983). This task examines children's Theory of Mind by presenting them with a scenario where they observe a protagonist (Sally) hide a target object (e.g., ball) in one of two locations (e.g., a basket), and in her absence, another character (Anne) moves her ball into the other location (e.g., a box). Children are then asked where Sally will look for her ball upon her return. Across hundreds of studies, researchers suggest that at 3 years of age children fail this task by indicating that Sally will look for her ball in its current location (i.e., the box), and at 4.5 to 5 years of age children pass the task by indicating that Sally will look for the object in its original location (i.e., the box, where she originally hid it; Gopnik & Astington, 1988; Liu et al., 2008; Perner et al., 1987; see Wellman et al., 2001)

There is widespread disagreement among researchers about what the Sally-Anne task is measuring, or *why* 3-year-olds fail this classic task (Poulin-Dubois et al., 2018; Setoh et al., 2016; Wang & Leslie, 2016). One interpretation, sometimes referred to as the 'Fundamental

Change' view, is that 3-year-olds do not yet comprehend that others have minds that can misrepresent reality (Gopnik, 1993b; Perner, 1991; Wellman et al., 2001) and between the ages of 3 and 5 children undergo a fundamental change in the way they conceive of the mind. Another interpretation, sometimes referred to as the 'Processing Demands' view is that 3-year-olds' more general cognitive abilities (e.g., working memory, inhibitory control, language) are not sufficiently developed to allow them to succeed. Under this latter interpretation, it is not that 3-year-olds lack the concept of false beliefs (Bloom & German, 2000; Fodor, 1992; He et al., 2012), but that developmental limitations in cognitive processing abilities undermine their performance. Several variants of this latter interpretation have been put forth emphasizing different cognitive processes but on the whole these researchers argue that if these processing demands were sufficiently reduced, even very young children could successfully reason about false beliefs (for more accounts, see Apperly & Butterfill, 2009; Benson et al., 2012).

Several studies have shown that when the Sally-Anne task is modified in ways to improve children's processing of the task, young children's false belief performance tends to improve. For example, Siegal and Beattie (1991) changed the false belief question to where will the protagonist look for the target object first? As opposed to, where will the protagonist look for the target object. They found that this clarification improved children's false belief performance. Researchers also found that when children were active in a false belief scenario, where they were deceiving the protagonist by hiding the target object, young children were more likely to accurately infer the protagonist's false belief (Chandler et al., 1989; Hala & Chandler, 1996; see Sullivan & Winner, 1993). Also, as early as 18 months old, toddlers demonstrate an understanding of false beliefs when actively helping an adult achieve a goal (Buttelmann et al., 2009, 2014). Furthermore, research suggests that young children's difficulty with the Sally-

Anne task is related to cognitive demands such as explicit response generation (e.g., He et al., 2012), executive functioning (Devine & Hughes, 2014, for meta-analysis; Fiske et al., 2014), attentional biases (e.g., Rubio-Fernández & Geurts, 2013, 2016), and language comprehension (e.g., de Villiers & de Villiers, 2012).

Initially, infant research also appeared to provide support for the view that the Classic Sally-Anne task is unnecessarily cognitively demanding for young children (Baillargeon et al., 2010; Luo, 2011; Onishi & Baillargeon, 2005; Scott & Baillargeon, 2017). Onishi and Baillargeon were first to demonstrate infants' apparent ability to reason about false belief using an experimental paradigm measuring whether infants looked longer (evidencing surprise) at one of two visual scenes (i.e., one in which an actor behaved in line with a true belief and one with an actor behaving in line with a false belief; Onishi & Baillargeon, 2005). This task placed fewer cognitive demands on participants. For example, the paradigm did not require any understanding of language, it did not require that the participant make a prediction about one's future behavior, nor did it require the participant to intentionally choose a response. In the absence of these demands, even 15-month-olds seemed to understand that people can hold false beliefs (see also, Surian et al., 2007; Träuble et al., 2010). However, the robustness of those findings have been called into question following several failed replication attempts from multiple labs (Kulke et al., 2019; Powell et al., 2018; Schuwerk et al., 2018). Moreover, even if the initial findings hold true, it was not infants' false belief reasoning that was shown to predict a host of social outcomes (e.g., De Rosnay et al., 2014; Lalonde & Chandler, 1995b; Razza & Blair, 2009; Suway et al., 2012)—it was 3 to 5-year-olds' performance on the Classic False belief tasks that predict those social outcomes, and therefore children's false belief performance in that developmental stage

necessitates better understanding. Hence, the goal of this chapter is to gain a better understanding of the nature of the cognitive changes affecting false belief reasoning between ages 3 and 5.

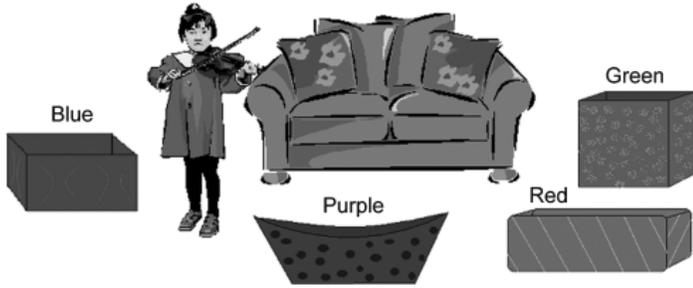
In the current study, I examine a different cognitive process that has so far received little attention—the effect of the ‘curse of knowledge’ on children’s false belief reasoning. The curse of knowledge is the tendency to be biased by one’s own (current) knowledge when reasoning about a more naïve perspective (a.k.a., the ‘hindsight bias’, ‘creeping determinism’, ‘reality bias’, or the ‘knew-it-all-along effect’; Bernstein et al., 2016; Fischhoff, 1975; Mitchell & Taylor, 1999; Wood, 1978). As a classic example of the curse of knowledge bias, adults who learn the outcome of an event (e.g., an election, a battle, a sports game) overestimate the likelihood that others will predict that outcome. In comparison, adults who were not informed of the event outcome tend to be more accurate in their estimates of what others will predict (Fischhoff, 1975; Ghrear et al., 2016; Guilbault et al., 2004).

In the Sally-Anne task, children are made aware of *exactly where Anne moved the object* even though specific outcome information is not a *necessary* condition to infer a false belief. Accordingly, to pass the Sally-Anne task, children are required to overcome the curse of knowledge and infer Sally’s naïve perspective. This is especially difficult for younger children, as they tend to be more vulnerable to the curse of knowledge than older children (Bernstein et al., 2011; Birch & Bloom, 2003; Lagattuta et al., 2010, 2014). Still, even adults’ false belief reasoning can be impaired by possessing specific outcome information (Birch & Bloom, 2007). In their experiment, Birch and Bloom (2007) presented participants with a four-box false belief task, where adults were told that a protagonist, named Vicky, was playing her violin and then decided to go outside (Figure 3.1). Before leaving, she placed her violin inside a blue box. In her absence, participants were told that the locations of the boxes were altered, and the violin was

moved to another box. Some of the participants were told exactly where the violin was moved (e.g., red box), and others were simply told that it was moved to a different box, with no specific information as to where. Participants who knew it had been moved to the red box were significantly worse at predicting Vicky's false belief than participants who did not know where the object had been moved. That is, adults who can unquestionably reason about false beliefs can be biased, or 'cursed' so-to-speak, by their own knowledge when reasoning about false beliefs (for replications and extensions of this work see Farrar & Ostojic, 2018; Ryskin & Brown-Schmidt, 2014).

A

This is Vicki. She finishes playing her violin and puts it in the blue container. Then she goes outside to play.

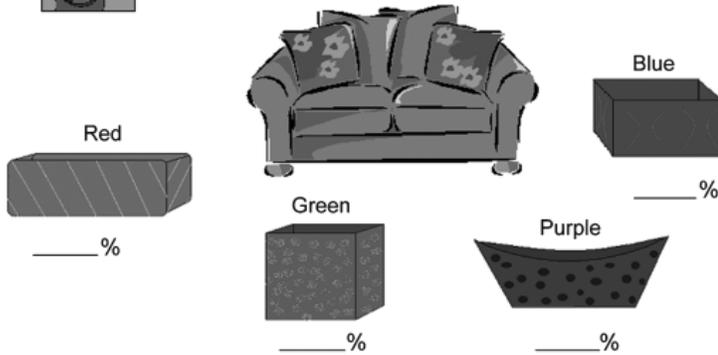


B

While Vicki is outside playing, her sister, Denise, moves the violin to the red container.



Then, Denise rearranges the containers in the room until the room looks like the picture below.



When Vicki returns, she wants to play her violin. What are the chances Vicki will first look for her violin in each of the above containers? Write your answers in percentages in the spaces provided under each container.

Figure 3.1. Stimuli from Birch and Bloom’s (2007) study, where adults are given a plausible reason to believe that Vicki would look for her violin in the current box. Note, that the red box (current box) in section B is in the same spot as the blue box (original box) in section A. From “The Curse of Knowledge in Reasoning About False Beliefs” by Birch & Bloom (2007), *Psychological Science*, 18(5), 384.

Importantly, previous research has hinted at the effect of the curse of knowledge on young children’s false belief reasoning. For example, in an attempt to reduce inhibitory control demands, Setoh, Scott and Baillargeon (2016) presented children with a variant of the Sally-Anne task, where children were not told the current location of the target object, and therefore

children did not have to inhibit a prepotent response of pointing to the actual location of the target object (see Grosso et al., 2019; Setoh et al., 2016). The researchers also included two practice questions to help children practice generating responses. In this task, the researchers told children about Emma who found an apple in a bowl, and she moved it to a box. Then, the researchers presented children with two practice questions, each question involved showing children two pictures of two different items (e.g., an apple and a banana), and asking them to point at one of the items (e.g., ‘where is Emma’s apple?’). Then, the researchers told children that in Emma’s absence, Ethan came, and he took away the apple, and then they asked children where Emma will look for her apple.

Using this task, Setoh, Scott and Baillargeon (2016) found that 2.5-year-olds can pass the task and indicate that Emma will look for her apple in the bowl. However, when children were told the current location of the apple, they pointed to the current location significantly above chance. The researchers interpreted this finding as indicative of an inhibitory control account on children’s false belief performance, where young children have not fully developed the cognitive ability to inhibit the dominant response of pointing to the current location of the target object, and therefore were unable to pass the Sally-Anne task. While inhibitory control plays an important role in the curse of knowledge (e.g., Bernstein et al., 2007; Coolin et al., 2015), as well as false belief performance (e.g., Chasiotis et al., 2006; Joseph & Tager-Flusberg, 2004), the inhibitory control account of the curse of knowledge does not fully explain the occurrence of the bias in our social reasoning— a point I will return to in the final chapter.

Koos and colleagues (1997) also created a variant of the Sally-Anne task, where they reduced the saliency of the current location of the target object. That is, children were told that when the protagonist left the scene, the secondary character ate the target object (the target object

here is a chocolate bar). In this task, 3-year-olds were more accurate at inferring the protagonist's false belief, compared to the Sally-Anne task; that is, 3-year-olds were more likely to indicate that the protagonist will look for her object in its original location. It is possible that removing the target object (i.e., it was eaten) reduced the curse of knowledge (e.g., the current location of the now-eaten chocolate bar is less salient or more ambiguous) and that improved their false belief reasoning. However, the reasons behind children's improvement are unclear. This study remains unpublished, so the details of the method are unavailable. It is possible that children were just as cursed by their knowledge, and they assumed that Sally would know that the chocolate was eaten, but they did not, or could not, point to the character's mouth, or stomach, as a place where Sally would look.

Researchers also compared the Sally-Anne task with another 'reality-unknown' false belief task developed by Call and Tomasello (1999). In this reality-unknown task, the participant learns that a protagonist put an object in one of two identical boxes, with no information as to which one. Then, in the absence of the protagonist, a secondary character swaps the locations of the boxes. The protagonist comes back for the object and says here it is, pointing to one of the boxes. The participant is then asked to retrieve the object. To pass the task, the participant must infer that the protagonist has a false belief, since she was not present when the secondary character swapped the locations of the identical boxes. And the participant must ignore the protagonist and retrieve the object in the other box. Importantly, to pass this task, the participant is not required to overcome the curse of knowledge, given that they do not learn the specific location of the object. Nonetheless, children's performance on the Sally-Anne task is like their performance on the reality-unknown task; 4-year-olds tend to fail both tasks, while 5-year-olds tend to pass both tasks (Call & Tomasello, 1999). The researchers suggest that this might be

because the reality-unknown task is difficult in its own way; to pass the task, children must track the object without seeing it, and they must ignore the protagonist's indication of where the object is (Call & Tomasello, 1999).

While the above studies offer novel and informative ways of examining false belief reasoning among children, they do not systematically examine the effect of specific outcome knowledge on children's false belief reasoning. Experiment 1 and 2 aim to fill this gap by directly examining the effect of the curse of knowledge on false belief reasoning, where children are presented with two identical tasks that vary solely based on whether outcome information is given or not. I use the logic put forth by Birch (2005) and used by Birch and Bloom (2007), and I design two variants of the Sally-Anne task (see Birch, 2005; Birch & Bloom, 2007) where a protagonist hides an object in one of four boxes and leaves the scene. As in the standard task, another character moves the object in the protagonist's absence. The critical manipulation is whether the participants are told specific outcome information or not. Two test trials (Outcome Known, or Cursed, Trials) are consistent with the Sally-Anne task, children are told exactly where the object was moved (e.g., "to the blue box"). The other two test trials (Outcome Unknown, or Curse-Lifted, Trials), children are told that the object was moved, but they are not told to which box ("to one of the other boxes"). After each test trial, children are asked where the protagonist will look for the object. In other words, the manipulation across test trial types allows for investigating whether eliminating, or at least reducing, the effect of the curse of knowledge (i.e., in the Outcome Unknown Trials) would affect children's false belief reasoning.

Given previous research suggesting that the curse of knowledge interferes with the ability to reason about false beliefs (e.g., see Ghrear et al., 2016), I predict that children would perform more accurately on the false belief task when they are not required to overcome the curse of

knowledge. Also, considering that young children are more susceptible to the bias compared to older children and adults (e.g., Birch & Bloom, 2003), I hypothesize that the age-related changes in children's performance on the Sally-Anne task reflect older preschoolers' greater ability to overcome the curse of knowledge, rather than a conceptual change in their understanding of the mind. Therefore, I predict that the younger children would be significantly better at reasoning about false beliefs when the curse of knowledge is removed.

3.1 Experiment 1

3.1.1 Method

3.1.1.1 Participants

I recruited 277 children in Vancouver, BC, Canada, between September 2015 to June 2017. I recruited this sample through multiple strategies, including contacting families through the developmental psychology's recruitment database, approaching parents in a children's museum, and contacting parents through school recruitment where teachers sent students home with consent forms. Predominately, these children belonged to middle or upper-middle class families. I aimed for a sample of children who were functional in English, and who spoke English at least 50% of the time. I only tested children who were eager to participate. Children were tested individually in our laboratory, at a museum, or at local schools.

Thirty-nine of these participants' data were excluded from analyses for reasons that interfered with the validity of their responses. Of the 39 excluded cases, the majority (87%) failed to complete the experimental sessions due to distraction or a failure to respond, 5% involved participants with language comprehension difficulties, 5% involved participants who were interrupted during the experiment, and another 3% involved a participant with a diagnosis of Autistic Spectrum Disorder. For the remaining two-hundred and thirty-eight cases, any trial

where children could not remember the original location of the object was excluded (as will be discussed below). Accordingly, the sample used for analyses included two-hundred and thirty participants (Range: 36 to 83 months; Mean age = 54 months, 55% male); ninety-one 3-year-olds (Mean = 41.5 months, SD = 3.5 months, 56% male), seventy 4-year-olds (Mean = 53.2 months, SD = 3.5 months, 55% male), and sixty-nine 5 to 6-year-olds (Mean = 71.1 months, SD = 6.5 months, 52% male). According to G*Power post-hoc analyses, I had a 98% chance of finding the effect of the curse of knowledge on false belief scores, if it existed. Of the 230 children, 37% percent were of Caucasian descent, 17% were of East Asian descent, 12% were of mixed descent (e.g., half East Asian half Caucasian), one child was of Arab descent, and another child was of African American descent. Parents of the remaining 33% of the children did not disclose ethnicity information.

3.1.1.2 Materials

One laminated paper with an image of four boxes (one yellow, one purple, one red, and one blue; Figure 3.2) was presented during the entire duration of the experiment. A total of eight stick figures were also presented. Each stick figure was a paper cut out of a simple drawing of a person (created using Paint) that was taped on a small stick. Two stick figures (a male and female) were presented for each false belief trial.

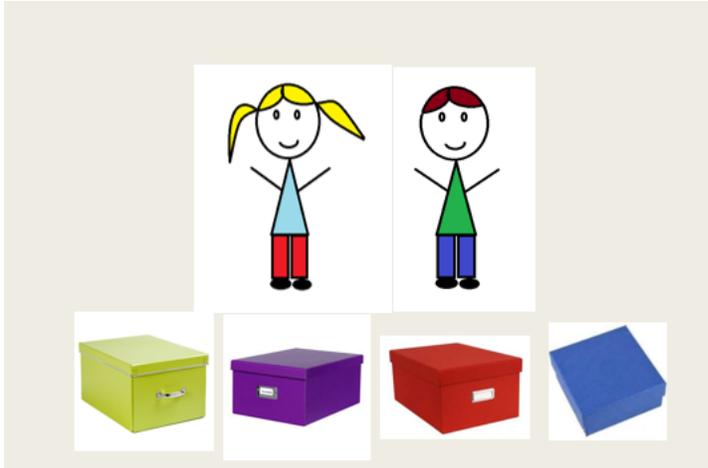


Figure 3.2. Stick figures and boxes for Experiment 1.

3.1.1.3 Procedure

Children were tested individually in a quiet area. Each child was presented with four false belief test trials; for each of the test trials, the experimenter presented a scenario about two characters and acted out the scenario with two stick figures (see Figure 1.2, for a visual illustration of a scenario).

The scenario involved a protagonist who hid an object in one of four boxes. Then, in the absence of the protagonist, another character placed the object in a different box. In two of the tasks, children were told exactly where the second character placed the object. For example, the following scenario was presented:

“Sally was playing with her ball, then she got hungry, so Sally put her ball right in here [purple box], and went home. When Sally was gone, Ryan hid Sally’s ball in a different spot! He may have hid it here, or here, or here [researcher pointed to each of the other boxes]. But we know that Ryan hid the ball here [blue box]. Then, Sally came back.”

In the other two tasks, children were told that the second character moved the object to another box, but they were not told exactly which box. For example, the following scenario was presented:

*“Bill was eating a chocolate bar, but he got full before he could finish it so he put the rest of his chocolate bar in here [red box], so he could have it later. Then, Bill went home. When Bill was gone Jane came, and hid his chocolate bar somewhere else! She may have hid it here, or here, or here [researcher pointed to each of the other boxes]. But, **we don’t know** where she hid it. Then, Bill came back.”*

After each scenario was presented, children were asked where the protagonist will look for the object (e.g., *“Where will Bill look for the chocolate bar?”*), to examine their ability to infer the protagonist’s false belief. Then, children were administered two memory checks to ensure they had paid attention to each story: One question assessed whether children could remember where the second character moved the object (e.g., *“Do you know where the chocolate bar is really?”*) and another question assessed whether children recalled that the protagonist was absent when the object was moved by the second character (e.g., *“Did Bill see Jane moving the chocolate bar?”*). The majority of participants answered the memory questions correctly. Importantly, children were also asked where the protagonist (e.g., Bill) left the object in the beginning (e.g., *“Do you remember where Bill put the Chocolate bar in the beginning?”*). This question is typically used as an exclusion criterion for Classic False Belief tasks because if the child forgot where the protagonist hid the object in the beginning, then it is impossible to assess their understanding of the protagonist’s false belief of the object’s location. Accordingly, I only included children’s data in the analyses for test trials where they could remember the original

location of the object. In other words, the sample only included children who remembered the original locations of the objects for at least one trial.

In this experiment, I used two versions to alternate which trial type was first presented (i.e., Outcome Known Trial or Outcome Unknown Trial; see Table 3.1). That is, the same scenarios and the same order of scenarios were used for both versions, however for Version 1 the first scenario was presented as Outcome Known Trial, and for Version 2 the first scenario was presented as Outcome Unknown Trial.

Table 3.1.

The False Belief Scenarios Presented by Trial Type and Version

Trial	Version 1	Version 2
1	<p>Outcome Known Trial: Sally was playing with her ball, then she got hungry, so Sally put her ball right in here (<i>purple box</i>), and went home. When Sally was gone, Ryan hid Sally's ball in a different spot! He may have hid it here, or here, or here (researcher <i>pointed to the other boxes</i>). But we know that Ryan hid the ball here (<i>blue box</i>). Then, Sally came back.</p>	<p>Outcome Unknown Trial: Sally was playing with her ball, then she got hungry, so Sally put her ball right in here (<i>purple box</i>), and went home. When Sally was gone, Ryan hid Sally's ball in a different spot! He may have hid it here, or here, or here (researcher <i>pointed to the other boxes</i>). But, we don't know where he hid it. Then, Sally came back.</p>
2	<p>Outcome Unknown Trial: Bill was eating a chocolate bar, but he got full before he could finish it so he put the rest of his chocolate bar in here (<i>red box</i>), so he could have it later. Then, Bill went home. When Bill was gone Jane came, and hid his chocolate bar somewhere else! She may have hid it here, or here, or here (researcher <i>pointed to the other boxes</i>). But, we don't know where she hid it. Then, Bill came back.</p>	<p>Outcome Known Trial: Bill was eating a chocolate bar, but he got full before he could finish it so he put the rest of his chocolate bar in here (<i>red box</i>), so he could have it later. Then, Bill went home. When Bill was gone Jane came, and hid his chocolate bar somewhere else! She may have hid it here or here or here (researcher <i>pointed to the other boxes</i>). But we know that Jane hid the chocolate bar here (<i>blue box</i>). Then, Bill came back.</p>
3	<p>Outcome Known Trial: Jenny was reading her book, and then she decided to go to the park. So, Jenny put her book right in here (<i>yellow box</i>). When Jenny left, Joe hid the book in a different spot! Joe may have hid it in here, here, or here (researcher <i>pointed to the other boxes</i>). But we know that Joe hid the book in here (<i>red box</i>). Then, Jenny came back to get her book!</p>	<p>Outcome Unknown Trial: Jenny was reading her book, and then she decided to go to the park. So, Jenny put her book right in here (<i>yellow box</i>). When Jenny left, Joe hid the book in a different spot! Joe may have hid it in here, or here, or here (researcher <i>pointed to the other boxes</i>). But, we don't know where Joe hid the book. Then, Jenny came back to get her book!</p>
4	<p>Outcome Unknown Trial: Rob was playing with his toy car, but he got thirsty and wanted to get some water. Rob put his toy car in here (<i>blue box</i>). Then, Rob went home to get some water. Kristy came, and hid the toy car somewhere else! Kristy may have hid the car here, or here, or here (researcher <i>pointed to the other boxes</i>). But, we don't know where Kristy hid the car. Then, Rob came back.</p>	<p>Outcome Known Trial: Rob was playing with his toy car, but he got thirsty and wanted to get some water. Rob put his toy car in here (<i>blue box</i>). Then, Rob went home to get some water. Kristy came, and hid the toy car somewhere else! Kristy may have hid the car here, or here or here (researcher <i>pointed to the other boxes</i>). But we know that Kristy put the car in here (<i>purple box</i>). Then, Rob came back.</p>

3.1.2 Results

3.1.2.1 Preliminary Analyses

Consistent with previous work (Wellman et al., 2001), data used for our main analyses (below) only included performance on test trials where children could recall the original object location for the given trail (e.g., “Do you remember where Bill put the Chocolate bar in the beginning?”). Responses to these questions were coded as incorrect (0) and correct (1), resulting in a sample of 230 three to six-year-old participants ($M_{\text{pass}} = .72$, $SD_{\text{pass}} = .45$). Preliminary analyses were conducted on the responses to these memory items to ensure that exclusions based on these items were unrelated to our experimental manipulation. I conducted a mixed effect logistic regression with a random intercept for participant in the R environment (Table 3.2), where memory performance was predicted from trial type (Outcome Known = 1, Outcome Unknown = 0), gender (1 = girls, 2 = boys), age group (1 = 3-year-olds, 2 = 4-year-olds, 3 = 5 and 6-year-olds). As seen in Table 3.2, age and gender, significantly predicted memory performance, such that girls were more likely to pass the memory trials compared to boys, $b = -0.38$, $SE = 0.15$, $Wald = -2.22$, $p = 0.026$, and older children were more likely to pass the memory trials compared to younger children, $b = 0.59$, $SE = 0.30$, $Wald = 5.45$, $p < .001$. However, trial type did not significantly predict memory performance; that is, the experimental manipulation did not affect children’s ability to pass the memory question.

Table 3.2.

A mixed effects logistic regression examining the effect of trial type, gender and age group on memory performance.

Predictors	Beta	Wald	P
Intercept	0.60 (.35)	1.74	0.08
Trial Type (0 = Outcome Unknown, 1 = Outcome Known)	-.13 (.15)	-0.85	0.40
Gender (1 = girls, 2 = boys)	-.38 (0.17)	-2.22	0.026*
Age Group (1 = 3 years, 2 = 4 years, 3 = 5 and 6 years)	0.59 (.30)	5.45	< .001***

Note. Values in brackets are standard error.

To examine the validity of the current task, I confirmed whether this study replicated the widely reported age effect on false belief performance (see Wellman et al., 2001), suggesting that older children are more likely to pass the Sally-Anne task compared to younger children. Specifically, I examined the effect of age on children’s performance on the Outcome Known trial, which was designed to emulate the Sally-Anne task. I analyzed children’s responses to the false belief questions for each trial (e.g., “Where will Bill look for the chocolate bar?”). I coded their responses as correct (= 1) if they accurately inferred the protagonist’s false belief (i.e., indicated that the protagonist will look for the object in its original location), and as incorrect (= 0) if they did not infer the protagonist’s false belief (i.e., indicated that the protagonist will look for the object in a different location). I conducted a mixed logistic regression with a random intercept for participant, where I predicted children’s performance on the Outcome Known trials from age group (1 = 3 years, 2 = 4 years, 3 = 5 to 6 years). As expected, I found that age group significantly predicted children’s false belief performance, such that older children were more likely to pass the task, compared to younger children, $b = 0.39$, $SE = 0.17$, $Wald = 2.29$, $p = .022$.

3.1.2.2 Main Analyses

Next, I conducted a mixed effect logistic regression with a random intercept for participant, where I predicted false belief performance from trial type, gender, age group, and version (1 = version 1, 2 = version 2; see Table 2), and the interaction between age group and trial type, given my prediction that younger children would benefit the most from the reduced curse of knowledge. As expected, there was a main effect of trial type (Cohen's $d = 0.28$), such that children's average performance significantly *improved* when specific outcome location was unknown, versus known, $b = -1.99$, $SE = 0.66$, $Wald = 3$, $p = .003$. Importantly, when children failed the Outcome Known Trials, they tended to choose the current location at an average of 80%, which is significantly above chance, $t(138) = 20.44$, $p < .001$, indicating that they were biased by their knowledge of exactly where the object was moved.

Also, as can be seen in Table 3.3, girls were significantly more likely to pass both types of false belief test trials, $b = -0.91$, $SE = 0.45$, $Wald = -2.04$, $p = .042$. This is consistent with previous findings suggesting that girls tend to show better false belief performance than boys (e.g., Charman et al., 2002; Devine & Hughes, 2018). Children's overall false belief performance was not significantly affected by age group or version. Nonetheless, there was a significant interaction between age group and trial type on false belief performance, $b = 0.71$, $SE = 0.30$, $Wald = 2.39$, $p = .02$. As can be seen in Figure 3.3, 3 and 4-year-olds were more likely to benefit from minimizing the curse of knowledge, compared to 5 and 6-year-olds. Impressively, on Outcome Unknown Trials both 3-year-olds ($M_{pass} = 0.54$, $SD_{pass} = 0.50$) and four-year-olds ($M_{pass} = 0.56$, $SD_{pass} = 0.50$) performed as well as the 5- and 6-year-olds ($M_{pass} = 0.56$, $SD_{pass} = 0.50$), revealing that when the curse of knowledge is removed, three-year-olds are just as adept as older preschoolers at inferring others' false beliefs (see Figure 3.3).

Table 3.3.

A mixed effects logistic regression examining the effects of trial type, gender, age group, version and age group by trial type on false belief performance.

Predictors	Beta	Wald	P
Intercept	2.48 (1.27)	1.96	0.051
Trial Type (0 = Outcome Unknown, 1 = Outcome Known)	-1.99 (.66)	-3	0.003**
Gender (1 = girls, 2 = boys)	-0.91(.45)	-2.04	0.042*
Age Group (1 = 3 years, 2 = 4 years, 3 = 5 and 6 years)	0.01 (0.32)	0.02	0.99
Version (1 = version 1, 2 = version 2)	-0.51 (0.47)	-1.10	0.27
Age Group × Trial Type	0.71 (0.30)	2.39	0.02*

Note. Values in brackets are standard error.

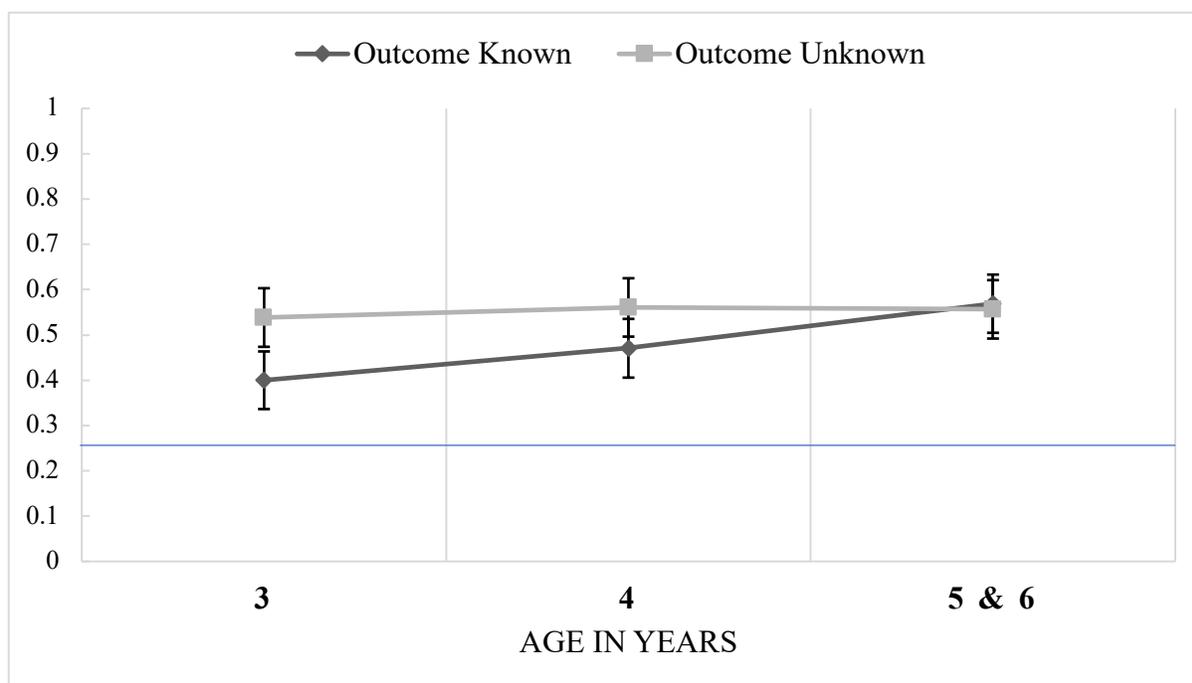


Figure 3.3. The effect of trial type on average false belief performance across age groups. As demonstrated above, 3-year-olds' performance was significantly more accurate in the Outcome Unknown Trials (i.e., when specific outcome information was not presented; 54% pass) versus Outcome Known Trials (i.e., when specific outcome information was presented; 40% pass). Four-year-olds' performance was marginally more accurate in the Outcome Unknown Trials (56% pass) compared to the Outcome Known Trials (47% pass). Five and 6-year-olds, however, showed no significant difference between Outcome Unknown Trials (56% pass) and Outcome Known Trials (57%). Notably, young children performed equally as well as older children in the Outcome Unknown Trials. In the Outcome Known Trials, on the other hand, age group significantly predicted false belief performance, such that older children performed better than younger children. The blue line denotes chance performance (i.e., 25% given there are 4 boxes). At each age, and for each trial type (Outcome Known vs Outcome Unknown), children's performance was significantly above chance, ($p < .02$). Note: Error bars around the means are 95% confidence intervals.

Interestingly, 5- and 6-year-olds who typically perform fairly well on 2-container versions of the Sally-Anne task (i.e., they pass at around a 65% average and a 75% average, respectively; Wellman et al., 2001) were passing at a lower rate on the current 4-container versions (56% average, $SD = 50\%$; Figure 3.3). Of course, unlike a 2-container task the current task cannot be passed by chance 50% of the time, but only 25% of the time. In other words, in

previous research children had a greater probability of succeeding by chance without ever reasoning about a false belief. That is not to say that older children do not understand false beliefs; they clearly do (i.e., they perform significantly above chance), but performance on a 2-container task is less likely to capture children's difficulties than a task where the probability of passing by chance is lower. Importantly, while 5-and 6-year-olds tend to do much *better* at reasoning about false beliefs than younger children they are by no means perfect. Poor false belief performance in older children on modified Sally-Anne tasks is consistent with other work (Fabricius et al., 2010; Fabricius & Khalil, 2003). For example, Fabricius and Khalil (2003) found that adding an additional box to the Sally-Anne task significantly worsens 4 to 6-year-old children's false belief performance. Specifically, in one of their experiments, the researchers found that 4.5 to 5.5-year-old children were 71% accurate when responding to the classic 2-container Sally-Anne task (i.e., 21% better than chance), however they were only 55% accurate when responding to a modified 3-container false belief task (i.e., 22% better than chance). Comparatively, our older children performed on par, or better, in our 4-container task (i.e., 31% better than chance) than the older children in Fabricius and Khalil's (2003) 3-container task. The point here is simply to remind the reader that even older children are not perfect at false belief reasoning. Importantly, however, removing the curse of knowledge from false belief tasks allows 3-year-olds to perform on par with 5-and-6-year-olds.

3.1.3 Discussion

The present results demonstrate that 3-year-old children are significantly more likely to accurately infer a false belief when they do not have specific outcome information, compared to when they have specific outcome information. When young children knew the true location of the object, they tended to indicate that the protagonist will look for the object in *that* location;

that is, they tended to fail the task because they were biased or ‘cursed’ by their own knowledge. Age analyses revealed that the curse of knowledge only affected young children’s false belief reasoning abilities, and not those of older children. Critically, these results illustrate that when the curse of knowledge is eliminated from these measures by removing specific outcome information, *3-year-olds are equally as good as older preschoolers at reasoning about others’ false beliefs.*

A follow up question is: What exactly is it about the Outcome Known condition that interferes with performance? Is it simply that they possess knowledge of the outcome? Or is it that young children are less accurate at reasoning about false beliefs in the Outcome Known trials, because these trials involve more information to process (more cognitive load), compared to the Outcome Unknown trials. That is, in the Outcome Known trials, I asked children to reason about the protagonist’s false belief after presenting a new hiding location, whereas in the Outcome Unknown trials, I asked children to reason about the false belief with no additional information. Accordingly, perhaps the additional cognitive load accounts for the difference in children’s performance on the Outcome Known versus Outcome Unknown trials.

Another possibility that might explain children’s poorer performance in the Outcome Known versus the Outcome Unknown trial is that, in the Outcome Known trial, I made an additional point to the current location of the object, making it the last box I pointed to. In comparison, in the Outcome Unknown trial, I only point to each location once. It is possible that when I ended with a point to the current location of the object in the Outcome Known trial, I drew children’s attention towards that location and away from the correct option (i.e., original location).

Therefore, it might not be solely the outcome *knowledge* that biased children’s responses towards

the current location, but the additional attention that was drawn to that location by the pointing typically used in Classic False Belief tasks.

To address these possibilities, and to replicate and extend our earlier findings, I conduct Experiment 2. Specifically, in Experiment 2, I aim to: 1. Replicate our main finding in Experiment 1; that is, confirm that young children are more accurate at reasoning about false beliefs in an Outcome Unknown trial compared to an Outcome Known trial. 2. Examine the possibility that young children performed more accurately on an Outcome Unknown trial versus an Outcome Known trial, because an Outcome Known trial involved more information to process. 3. Examine whether children performed worse on the Outcome Known trial in Experiment 1 because of the additional final point to the ‘cursed’ location.

To replicate my main finding in Experiment 1, I create a new Outcome Known trial and a new Outcome Unknown trial (see Experiment 2, Procedure). Again, in both trials, a protagonist hides an object in one of four boxes and leaves the scene. In the protagonist’s absence, another character moves the object to another box. In the Outcome Known trial, children are told exactly where the object was moved (the green box), and in the Outcome Unknown trial, children are told that the object was moved into one of the other boxes, but we do not know which one.

To examine the possibilities that either the additional information presented in the Outcome Known trial or the final point to the current location impeded children’s false belief reasoning, I include a Control trial. In this trial, children are not told where the secondary character moved the object, however they are told irrelevant information about one of the boxes: Children are told that as the secondary character leaves the scene, she trips on one of the boxes (green box). Accordingly, in this Control trial, I provide children with additional information, and I make a final point to a box (i.e., the box where she trips), thereby making it more comparable to the

‘cursed’ classic tasks but with the key exception of the outcome remaining unknown. If children perform more accurately on the Control trial versus the Outcome Known trial, then it is the outcome knowledge that affects children’s false belief reasoning, and not the presentation of additional information or having their attention drawn to a particular location.

Furthermore, I modify the design used in Experiment 1 to improve children’s overall performance. Specifically, I make modifications to improve children’s ability to remember the original location of the target object and minimize the number of memory exclusions to maximize power. As you may recall, only trials where children remember the original location are included in the analyses. To improve children’s memory performance, children are asked the memory question directly after they observed the scenario (i.e., where did the protagonist hide the target object in the beginning?). If they fail the memory question, they watch the scenario again and are asked the memory question again. Children are given a maximum of three chances to answer the memory question correctly, and if they could not pass, their data was excluded from analyses. Also, in an attempt to clarify the false belief question, I add the word ‘first’ and ask: where will the protagonist look for her target object *first*? In previous work, this was found to improve children’s false belief reasoning (e.g., Siegal & Beattie, 1991). Finally, since the current experiment includes a third trial type, children were presented with one of each trial type, instead of two of each trial types like Experiment 1. I make this modification because pilot testing revealed that young children had difficulty sustaining attention for 6 false belief stories that were so similar.

For Experiment 2, I make the following predictions: 1. I predict that older children would perform more accurately on the Outcome Known trial compared to younger children, as has been found in Experiment 1 and previous research on the Sally-Anne task (see Wellman et al., 2001).

2. Like Experiment 1, I predict that children would perform more accurately on the Outcome Unknown trial compared to the Outcome Known trial; that is, they would perform more accurately when the bias is minimized. 3. I predict that, like Experiment 1, younger children would be more likely to show a difference in performance between Outcome Unknown versus Outcome Known trials, such that they would perform more accurately on the Outcome Unknown trial. 4. I predict that children would perform more accurately on the Control trial, where they are not cursed by their knowledge, compared to the Outcome Known trial. That is, children would perform more accurately in the Control trial, where they are not given specific outcome information of where the target object is moved, compared to the Outcome Known trial; even though both the Control trial and the Outcome Known trial have additional information, compared to the Outcome Unknown trial, and an additional point. I am agnostic as to whether children would perform more accurately on the Outcome Unknown trial versus the Control trial. Children may perform more accurately on the Outcome Unknown trial, given that the Control trial has more information and an additional point, however I am not sure that that difference would be enough to affect children's false belief performance.

3.2. Experiment 2

3.2.1 Method

3.2.1.1 Participants

For this experiment, I aimed for sample size of 180 participants to secure an 80% chance of detecting a small effect size of the curse of knowledge on false belief scores, if it existed. However, I had to stop in-person data collection due to lab closures because of COVID-19. Given that I had already conducted in-person studies for 162 useable participants (i.e., participants whose data were not excluded due to factors that interfered with validity, see below),

I decided not to run the remaining participants on an online version of the study, as their performance on an online-version may not be comparable to the in-person study. Accordingly, I had a 76% chance of detecting a small effect of the curse of knowledge on false belief scores, if it existed.

I recruited 196 participants in Vancouver, BC, Canada, between September 2019 to March 2020. I used the same recruitment strategies as Experiment 1, and I aimed for similar sample characteristics. I excluded 34 participants' data for reasons that interfered with the validity of their responses. Specifically, 58% of the exclusions were because the child was distracted or did not want to continue the study, 18% were because children did not understand instructions due to a language barrier, 9% were because children did not pass our memory exclusion question after three chances (see Experiment 2, Procedure), 6% were because of parent interference, 3% were because children had atypical development, 3% were because children had a clear colour preference (i.e., one participant picked her favourite color at every trial), and finally 3% was excluded due to experimenter error.

The 162 useable participants were 3 to 5 years old (range: 38 to 71-months-old; mean = 54 months, SD = 9.2 months), and 49% of them were male; forty-eight 3-year-olds (Mean = 43.4 months, SD = 4.5 months, 40% male), sixty-eight 4-year-olds (Mean = 53.8 months, SD = 3.3 months, 46% male), and forty-six 5-year-olds (Mean = 65.5 months, SD = 3.1 months, 65% male). As for ethnicity, 35% were of Caucasian descent, 24% were of East Asian and Southeast Asian descent, 5% were of South Asian descent, 2% were of Middle Eastern descent, 1% were of Latin American descent, 1% were of aboriginal descent, 1% were of west Asian descent, and 21% were of mixed descent (e.g., East Asian and Caucasian descent). As for the remaining 10% of children, their parents did not disclose ethnic background.

3.2.1.2 Materials

For each study session, I used a laptop to present a pre-recorded PowerPoint presentation that guided children through the three stories. In the PowerPoint presentation, I used similar stick-figure puppets, as I used in Experiment 1. A total of six stick-figures were presented, each pair included a boy and a girl stick-figure. I also used an image of four boxes (see Figure 3.4).

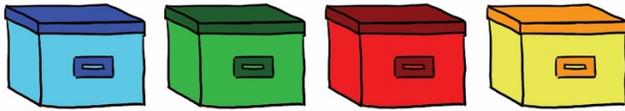


Figure 3.4. The boxes used for Experiment 2.

3.2.1.3 Procedure

For each study session, the child was accompanied by an experimenter who guided the child through the PowerPoint presentation and recorded their responses. The PowerPoint presentation contained an Outcome Known trial, an Outcome Unknown trial, and a Control trial. Each trial was in the form of a story, where a protagonist hid a toy in one location and then it was moved in her absence (see Table 3.4, for each trial). Like Experiment 1, I only revealed where the object was moved in the Outcome Known trial. In the Outcome Unknown and Control trial, children were simply told that we did not know where the object was moved. However, in the Control trial, we told children that the secondary character almost tripped on the green box.

For consistency, I used the same original location for all three trials. That is, the protagonist hid the object in the yellow box across the three stories. Also, in the Outcome Known and Control trial, the researcher pointed at the green box, either because that was where the

object was moved (Outcome Known trial), or because the secondary character almost tripped there (Control trial). I also highlighted to children that because the protagonist left the scene, she could not hear or see what was happening (Table 3.4).

After each trial, children were asked several questions. First, children were asked if they remembered where the protagonist put the object in the beginning. This question was asked first, to ensure that children could remember the original location of the object. If children did not answer this memory question correctly, the experimenter played the story clip again, and they were asked the same question. I excluded children from data analyses who watched the story 3 times and were still unable to answer the memory question correctly (3 children were excluded for this reason). Then, children were asked the false belief question (e.g., where will Sarah look for her doll first?). Lastly, children were asked whether they knew where the object was moved. I created six versions of this experiment to allow for all possible orders of the trials.

Table 3.4.

The stories presented for each trial type.

Outcome Known Trial	Outcome Unknown trial	Control trial
<p>Sarah was playing with her doll, then she decided to go swimming. She put her doll right in here [Yellow Box], and she went to the pool. So, <i>she couldn't see or hear what was happening</i>. Tom came and he hid Sarah's doll somewhere else! He may have hid it here [Red Box], here [Green Box], or here [Blue Box]. We know that he hid the doll right in here [Green Box]. Then, Sarah came back for her doll.</p>	<p>Brandon was playing with his toy horse, then he got hungry. He put his horse in here [Yellow Box], and he went to the kitchen. So, <i>he couldn't see or hear what was happening</i>. Chelsea came and she hid Brandon's horse somewhere else! She may have hid it here [Red Box], here [Green Box], or here [Blue Box]. But, we don't know where she hid the toy horse. Then, Brandon came back for his horse.</p>	<p>Nick was playing with his rubber duck, then he decided to play outside. He put his duck right here [Yellow Box], and he went outside. So, <i>he couldn't see or hear what was happening</i>. Emily came and hid his duck somewhere else! She may have hid it here [Red Box], here [Green Box], or here [Blue Box]. But, we don't know where she hid it. On the way out, Emily almost tripped on this [Green Box]. Then, Nick came back for his duck.</p>

Note. Each child was assigned to one of six possible orders of the above trials.

3.2.2 Results

To begin, I examined the validity of the current task by confirming whether it replicated the widely reported age effect on children's false belief performance using the Sally-Anne task (Wellman et al., 2001). I analyzed children's responses on the false belief questions for each trial (e.g., "Where will Sarah look for her doll first?"). I coded their responses as correct (= 1) if they accurately inferred the protagonist's false belief (i.e., indicated that the protagonist will look for the object in its original location), and as incorrect (= 0), if they did not infer the protagonist's false belief (i.e., indicated that the protagonist will look for the object in a different location). Again, I predicted children's performance on the Outcome Known trial, which was designed to emulate the Sally-Anne task, from age group (1 = 3 years, 2 = 4 years, 3 = 5 years) by conducting a logistic regression with a random intercept for participant. I did not find a main effect of age on false belief performance, $b = 0$, $SE = 0.02$, $Wald = 0$, $p = 1.0$. This was very

surprising given the abundance of previous research that demonstrated that older children perform more accurately on the Classic False Belief tasks (e.g., Perner et al., 1987; Wimmer & Perner, 1983). Here, 3-year-old children performed more accurately than 4-year-old children ($M = 0.61$, $SD = .49$, versus $M = 0.42$, $SD = .50$, respectively), and 3-year-old children performed like 5-year-old children ($M = 0.61$, $SD = .49$, versus $M = 0.61$, $SD = .49$, respectively). These findings made me question the validity of my results and whether I was able to measure what I intended to measure. As such, the results from Experiment 2 should be interpreted with caution.

Next, I conducted a mixed effect logistic regression with a random intercept for participant, where I predicted false belief performance from gender (1 = girls, 2 = boys), version (1 to 6), and age group. As can be seen in Table 3.5, there was no effect of gender, version, or age group on false belief performance. An absence of a gender effect was also surprising, given that Experiment 1 and previous research have reported that girls tend to perform more accurately on false belief tasks compared to boys (e.g., Charman et al., 2002; Frank et al., 2015).

Table 3.5.

A mixed effects logistic regression examining the effect of gender age group, and version on false belief performance.

Predictors	Beta	Wald	P
Intercept	-0.52 (1.23)	-0.42	0.67
Age Group (1 = 3 years, 2 = 4 years, 3 = 5 years)	-0.33 (.37)	-0.88	0.38
Gender (1 = girls, 2 = boys)	0.57 (0.57)	1.00	0.32
Version (1 to 6)	0.14 (.17)	0.81	0.42

Note. Values in brackets are standard error.

For my main analyses, I examined children’s false belief performance across trial type with Outcome Known trial as the reference condition. I created two dummy-coded variables to

examine the effect of the Outcome Unknown trial on false belief performance (Outcome Unknown = 1, Outcome Known = 0, Control = 0), and to examine the effect of the Control trial on false belief performance (Control = 1, Outcome Unknown = 0, Outcome Known = 0). This regression equation also included the effect of age group on false belief reasoning. I also entered the interaction of Outcome Unknown trial and age as a predictor in the equation, as well as the interaction of Control trial and age on false belief reasoning. I found that the predictors did not significantly affect false belief performance (Table 3.6).

Table 3.6.

A mixed effects logistic regression examining the effect of age and trial type on false belief performance.

Predictors	Beta	Wald	P
Intercept	0.31 (.87)	0.36	0.72
Age Group (1 = 3 years, 2 = 4 years, 3 = 5 years)	-0.008 (.41)	-0.02	0.98
Outcome Unknown	0.70 (0.83)	0.85	0.40
Control	0.28 (0.82)	0.34	0.73
Age x Outcome Unknown	-0.42 (.39)	-1.08	0.28
Age x Control	-0.35 (.39)	-0.90	0.37

Note. Values in brackets are standard error.

The outcome of the logistic regression did not support my predictions. Children did not perform more accurately on the Outcome Unknown versus the Outcome Known trial. That is, they did not show an effect of the curse of knowledge on their ability to accurately infer a false belief. Also, there was no significant interaction of age and Outcome Unknown trial on false belief performance; that is, younger children did not perform more accurately on the Outcome

Unknown versus Outcome Known trials. And lastly, children did not perform more accurately on the Control trial compared to the Outcome Known trial.

Given that in Experiment 1 I found an effect of outcome knowledge on false belief performance among young children, I decided to further examine 3 and 4-year-olds' performance on the Outcome Known versus Outcome Unknown trial. Again, there was no significant effect of Outcome Unknown trial on false belief reasoning, see Table 3.7.

Table 3.7.

Logistic regression examining the effect of outcome information among younger children.

Predictors	Beta	Wald	P
Intercept	-0.03 (.32)	-0.10	0.92
Unknown	0.13 (0.33)	0.39	0.70

Note. Values in brackets are standard error.

Next, I examined whether the order of the presentation of trials affected children's false belief performance. I conducted a logistic regression predicting false belief performance from trial order (first = 0, second = 1, third = 2), I found a significant effect of order on false belief performance, $b = -0.42$, $SE = 0.001$, $Wald = -675.1$, $p < .001$, such that children performed more accurately on the first trial ($M = 0.57$, $SD = 0.50$), compared to the second ($M = 0.48$, $SD = 0.50$) and third trials ($M = 0.48$, $SD = 0.50$), this suggests that children may have been more attentive on the first trial. Accordingly, I conducted exploratory analyses to examine whether children showed an effect of trial type (Known, Unknown, Control) on the first trial that they were presented. In a between-subjects design, I conducted a logistic regression comparing children's performance on the Outcome Known trial (reference group) compared to the Outcome Unknown trial and the Control trial. I also entered the interaction of Outcome Unknown trial and age group

as a predictor, as well as the interaction of Control trial and age on false belief reasoning. None of the variables significantly predicted children’s performance (see Table 3.8).

Table 3.8.

Logistic regression examining the effect of age and trial type on the first trial.

Predictors	Beta	Wald	P
Intercept	1.56 (1.33)	1.18	0.24
Age Group (1 = 3 years, 2 = 4 years, 3 = 5 years)	-0.29 (.33)	-0.90	0.37
Outcome Unknown	-0.84 (1.84)	-0.45	0.65
Control	-1.71 (1.94)	-0.88	0.38
Age x Outcome Unknown	0.21 (.46)	0.46	0.65
Age x Control	0.37 (.48)	0.77	0.44

Note. Values in brackets are standard error.

As a next step, I examined what led children to fail the false belief tasks in instances where they failed. As you may recall, in Experiment 1, 80% of children who failed the Outcome Known trial pointed to the current location of the target object, rather than the original location. Here, I wanted to examine if children who failed the Outcome Known trial, failed *because* they tended to point to the current location (i.e., green box). I also examined whether children failed the Control trial because they tended to point to the green box, since there was an additional point to the green box. Accordingly, I conducted a logistic regression examining whether children who failed pointed to the green box in the Outcome Known and Control trials, compared to the Outcome Unknown trial. In this analysis, I used the Outcome Unknown trial as the reference condition, given that the green box was not highlighted to children in the Outcome Unknown trial. I coded children’s responses as 1 anytime they pointed to the green box, and as 0

anytime they pointed to any other incorrect location (i.e., red and blue boxes). I created two dummy-coded variables to examine the effect of the Outcome Known trial on selecting the green box (Known = 1, Unknown = 0, Control = 0), and to examine the effect of the Control trial on selecting the green box (Control = 1, Unknown = 0, Known = 0). As can be seen in Table 3.9, children were significantly more likely to point to the green box in Outcome Known trial versus the Outcome Unknown trial, $b = 0.89$, $SE = 0.41$, $Wald = 2.21$, $p = .027$, suggesting that they were cursed by their knowledge when inferring the protagonist's false belief. However, children were not significantly more likely to point to the green box in the Control versus Outcome Unknown trial, suggesting that children were not biased towards the green box simply because the researcher pointed to it in the Control trial.

Table 3.9.

Logistic regression examining children's tendency to point to the green box.

Predictors	Beta	Wald	P
Intercept	-1.04 (.32)	-3.20	0.001**
Outcome Known	0.89 (.41)	2.21	0.027*
Control	-0.15 (.41)	-0.37	0.71

Note. Values in brackets are standard error.

As can be seen in Figure 3.5, when children failed the Outcome Known trial, they were more likely to point to the green box, compared to the other boxes. In the Outcome Unknown and Control trials, however, most of the children who failed pointed to the red box. Indeed, I found that when children failed the false belief question across trial types, they pointed to the red box significantly above chance, $t(44) = 3.65$, $p = .001$, suggesting that the red box was especially

salient. Surprisingly, in the Control trial the red box was more salient than the green box, even though the green box was talked about in the story.

Given the heightened salience of the red box, I suspected that a bias towards the red box interfered with the task's ability to examine the benefits of eliminating the curse of knowledge on false belief reasoning. Although the curse of knowledge was eliminated in the Outcome Unknown trial, their attention was unintentionally drawn to the red box instead.

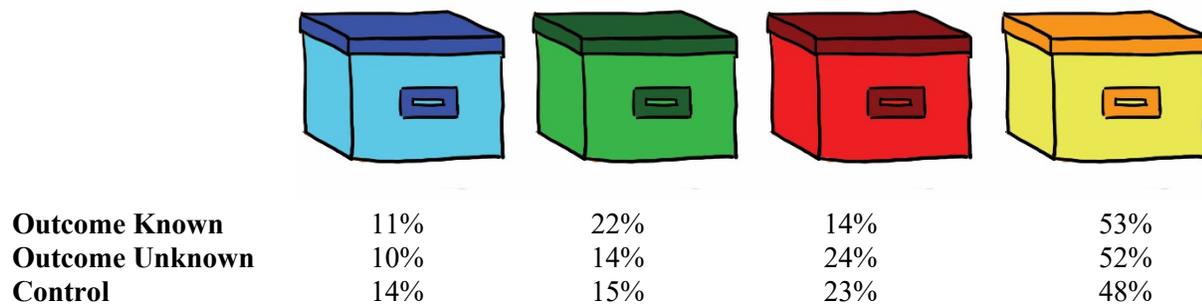


Figure 3.5. The percent of times children selected each box for the false belief question across trial types. Note that children tended to pass the false belief task by pointing to the yellow box (original location) significantly above chance ($p < 0.03$).

3.2.3. Discussion

The current results do not support any of my predictions. Most importantly, however, my findings from Experiment 2 are not consistent with a massive body of literature, suggesting that there is an age difference in children's false belief performance (see Wellman et al., 2001). This led me to question the validity of my study, and whether this study design provided a fair test of my primary hypothesis. Accordingly, I was left to conclude that the integrity of my findings was questionable, and that my hypotheses require further investigation using a different study design.

In the following paragraphs, I discuss a) the possibilities that could explain my current results, b) the insights that may be gleaned from these data, and c) suggestions for future research.

One difference between Experiments 1 and 2 that one might suspect could account for the differences in the results is the way the memory question was administered (i.e., where did the protagonist hide the target object in the beginning?). In Experiment 2, I gave children 3 chances to answer the memory question correctly before I excluded their data. This may have led children to learn that the original location of the target object (yellow box) is ‘always’ the correct answer. Accordingly, those children may have selected the yellow box for the follow up false belief question (i.e., where will the protagonist look for her target object first?) simply because they learned that the yellow box is always the right answer, and not because of a genuine ability to infer false beliefs. If younger children failed the memory question more often, they could have inadvertently had more opportunity to learn to select the yellow box and that could have led them to pass the false belief question as frequently as older children. However, this argument does not apply to my current findings as only seven 3-year-olds needed repetition of the memory question. Similarly, eight 4-year-olds needed repetition of the memory question, and yet 3-year-olds scored higher than 4-year-olds on the false belief questions.

It is also important to note that the differences in findings between Experiments 1 and 2 may be a result of the difference between the ways I excluded children’s data based on memory performance In Experiments 1 and 2. In Experiment 1, I excluded children who did not pass any of the memory questions across the four trials; that is, for each trial the memory question was presented once, and if children did not pass any of the memory questions, they were not included in the main analyses. Accordingly, in Experiment 1, 8 children were excluded from the main analyses due to memory exclusions. In Experiment 2, however, children were presented with the

memory question, and if they failed the question, the false belief scenario was replayed, and children were asked the memory question again. If the child failed the memory question again, this process was repeated one more time; such that children had a maximum of 3 chances to pass the memory question, before their data was excluded from analyses. As a result, in Experiment 2, 3 children's data were excluded from the main analyses due to memory exclusions. It is possible that the differences in our findings in Experiments 1 and 2 stems from the differences in exclusion criteria; that is, it could be that the stricter exclusion criteria in Experiment 1 selected for more intelligent children, compared to the exclusion criteria in Experiment 2, and the effect of the curse of knowledge on false belief reasoning is only relevant for more intelligent children. Nonetheless, I found the same pattern of results in each experiment, even when I did not exclude children based on memory failures. Accordingly, the patterns of results in Experiments 1 and 2 do not appear to be a result of the differences in memory exclusions.

Instead, the differences in results across Experiments 1 and 2 may stem from the different stimuli, specifically the different colours of the boxes (see Figures 3.3 and 3.6). In my analyses of Experiment 2, I found that children who failed the false belief question tended to select the red box significantly above chance. It is possible that the bright shade of red in the current task was especially salient, which may have interfered with children's false belief performance for several reasons. One possibility is that children may have simply selected the red box because it caught their attention. Another possibility is that because children did not know where the object was moved, they inferred the protagonist would assume that it was moved into the most salient box. Yet, another possibility is that children may have made an inference about the researcher's intentions; that is, they may have inferred that the reason the box is salient is because the researcher was drawing their attention to the correct answer. In other words, children may have

inferred that the researcher wanted them to choose that box. The first, and simplest, possibility is the most likely explanation, but I cannot rule out the other possibilities.

It is also possible that it was not the *colour* of the red box that led to the salience of that box, but rather it was the fact that the red box was located between the original and current locations (yellow and green boxes). In this study, the researcher made additional points to the yellow and green boxes. This may have inadvertently made the red box more salient, since it was the closest to the yellow box, and it was located between the yellow and green boxes. When asked the false belief question, children may have pointed to the red box because it was made salient during the story. And it is also possible that children inferred that the protagonist would look for the target object in the closest location to the yellow box, where she originally hid it.

Even though I did not find evidence for my main hypotheses in the current experiment, there was still much to be learned from the experiment. Importantly, there was evidence that children were biased by their own knowledge when inferring the protagonist's belief. In cases where children failed the false belief questions, they were more likely to point to the 'cursed' location (i.e., the green box) in the Outcome Known trial, compared to the Outcome Unknown trial. In other words, these children inferred that the protagonist would share their own knowledge of the current location of the target object. However, children were not more likely to select the green box in the Control trial compared to the Outcome Unknown trial, suggesting that children were not biased by the green box simply because there was an additional point to that location. One reason I included the Control trial in the current experiment was to examine the possibility that children selected the cursed location (in Experiment 1 and in many standard false belief tasks), because there was an additional point to that location. This possibility, however, was not sufficient in explaining the current findings, given that there were additional points to

the green box in the Control trial and the Outcome Known trial, however children were only biased by the green box in the Outcome Known trial where there was both an additional point *and outcome knowledge*. That is, when I drew children's attention to the green box, with no outcome knowledge of the object's location, they did not falsely assume that the protagonist would look in that box. Instead, I found that children's bias towards the green box in the Outcome Known trial was triggered by the curse of knowledge, where children inferred that the protagonist would share their knowledge.

Finally, my findings highlight the importance of pre-testing study material before data collection. In a future study, I would run a pilot experiment to ensure that children are not drawn to one box over the others. Similarly, I now realize the importance of using the same study material as a previous experiment if I aim to replicate the main finding of that experiment. Of course, in an ideal world, robust results will generalize across a variety of contexts and designs, however, sometimes seemingly trivial changes can have not-so-trivial consequences. That is, sometimes the color of the stimuli, the appearance of the researcher, or the setting of the experiment, can unintentionally affect how participants respond to the study.

3.3 General Discussion

In Experiment 1, younger children were more likely to pass the false belief tasks when they were *not* presented with specific outcome information, compared to when they were presented with specific outcome information. That is, younger children tended to infer a false belief more accurately when the curse of knowledge was minimized. In Experiment 2, however, children of all ages performed equally on both outcome known and outcome unknown trials.

Why did Experiment 2 fail to reproduce the main findings of Experiment 1? Experiment 2 was not a *direct* replication of Experiment 1, and as mentioned above there are several reasons

why Experiment 2 did not show the expected findings regarding my specific question of interest (the role of the curse of knowledge) or regarding developmental changes in false belief performance more generally. In particular, the colours of the boxes in Experiment 2 were different from those used in Experiment 1. And in Experiment 2, children tended to show a bias towards the red box, which was more visually salient than the rest of the boxes. This bias towards the red box may have created too much interference to observe the effect of the curse of knowledge on false belief performance. Children *were* cursed by their knowledge; when they were presented with specific outcome information, they inferred that the protagonist would share their knowledge (i.e., they were significantly more likely to choose the green box in Outcome Known trial compared to the Outcome Unknown trial). However, the decrease of the curse of knowledge in the Outcome Unknown trial did not manifest in better false belief performance seemingly due to the greater attention received by the red box. Accordingly, the effect of the curse of knowledge on false belief reasoning deserves further investigation.

The finding suggesting that younger children infer false beliefs more accurately, when they are not cursed by their knowledge (Experiment 1), echoes a growing body of literature suggesting that the classic Sally-Anne task *underestimates* children's ability to reason about the mind. Experiment 1 sheds light on yet another difficulty that young children face when completing the Sally-Anne task; that is, the necessity to overcome the curse of knowledge. The Sally-Anne task is at least in part capturing children's capacity to overcome the curse of knowledge, which raises the question of whether the associations previously established between performance on the Sally-Anne task and various indices of social emotional functioning such as prosocial behavior, peer relationships, social ability, and the ability to communicate effectively,

(e.g., Milligan et al., 2007; Razza & Blair, 2009) are actually reflective of individual differences in the curse of knowledge rather than false belief understanding.

Importantly, false belief reasoning and the curse of knowledge bias are not completely unrelated constructs—they both involve social perspective taking. However, the curse of knowledge is more pervasive in social interactions than one's ability to reason about false beliefs—it is present anytime you need to reason about a more naïve perspective. The detrimental impact of the curse of knowledge has been documented in many applied settings including business, education, politics, communication (both oral and written; and in medical, legal, and government decision-making; e.g., Arkes, 2013; Blank et al., 2003; Giroux et al., 2016; Kirchler et al., 2002). The similarity between the two constructs may have confounded previous research examining the Sally-Anne task, as this research may have been inadvertently measuring variance in the ability to overcome the curse of knowledge rather than the ability to infer a false belief. The similarity between these two constructs also serves to highlight why young children make more mistakes than older children and adults in everyday situations that involve perspective taking: Young children *are* more apt to make perspective-taking errors—not because they lack a concept of false beliefs, but because they are more cursed by their knowledge. The current work puts forth a new measure to examine the development of false belief reasoning without the interference of the curse of knowledge. This measure can be used to examine the correlates of false belief reasoning, including social competence, cognitive skills, and environmental factors. Also, by using the non-cursed measure (i.e., Outcome Unknown trials), along with the cursed measure (i.e., Outcome Known trials), researchers can pinpoint children's false belief reasoning difficulties stem from the curse of knowledge, and how it may relate to other social and cognitive factors.

On a separate note, my findings in Experiment 1 show support for the widely reported effect of age on the curse of knowledge. Only younger children showed the curse of knowledge effect in their false belief reasoning, while older children were not affected by the bias. One may argue that younger children are more likely to overestimate what others would know, compared to older children, because younger children typically find themselves in situations where others know more than they do. That is, younger children are constantly learning from others, and so perhaps it is a fair assumption on their part to infer that others would share their knowledge and know much more than they do? However, this argument does not adequately explain the age-related changes in older adulthood, where older adults, who undoubtedly have experienced that others do not always know more than they do, are also more likely to overestimate others' knowledge compared to older children and adults (e.g., Bernstein et al., 2011). Accordingly, the age-related changes in the tendency to overestimate others' knowledge are not the result of changes in individuals' perceptions of how much others know. Instead, younger children likely show a greater curse of knowledge because they have not yet developed sufficient cognitive resources to overcome the bias (e.g., inhibitory control, source memory).

Chapter 4: The Role of Fluency Misattribution in the Curse of Knowledge

What is the causal mechanism underlying the curse of knowledge? Researchers have put forth two accounts to explain the curse of knowledge in social reasoning. According to the inhibitory control account, the curse of knowledge occurs when an individual is unable to fully suppress, or inhibit, the contents of their knowledge to reason about a naïve perspective (e.g., see Bayen et al., 2007). On the other hand, the fluency misattribution account suggests that the curse of knowledge occurs when individuals mistake the fluency, or ease, with which the information comes to mind as indicative of how obvious the information is to others (Bernstein et al., 2018; Birch et al., 2017; Harley et al., 2004). In other words, this account suggests that the curse of knowledge occurs, because we mistake the subjective ease of processing certain information as an indication of how widely known this information is. So, the curse of knowledge is not a result of a difficulty in suppressing the *contents* of one's mind, but rather it is a tendency to misinterpret the ease with which that content comes to mind (or the feelings of fluency associated with the information). In this chapter, I outline 2 experiments examining the fluency misattribution account of the curse of knowledge. To begin, I provide brief overviews of the inhibitory control and fluency misattribution accounts, then I discuss previous work on the curse of knowledge including my master's thesis which is especially relevant to the current work, and lastly, I provide an overview of the present research.

Inhibitory control involves the ability to control one's attention, thoughts, or reactions, in a goal-oriented way that overrides strong predispositions in favor of more appropriate responses (Diamond, 2013). This ability develops across childhood and into early adulthood (e.g., Cohen-Gilbert & Thomas, 2013; Davidson et al., 2006; López-Caneda et al., 2014), and it tends to deteriorate in older adulthood (e.g., Christ et al., 2010; West & Alain, 2000). To examine

inhibitory control among children, researchers often use Stroop-like tasks, like the Day-Night task (e.g., Gerstadt et al., 1994; Simpson & Riggs, 2005). In this task, children are shown a picture of a moon and a picture of a sun, and they are instructed to say ‘day’ in response to the moon picture, and ‘night’ in response to the sun picture. That is, children must inhibit the more common association of saying ‘day’ to the sun picture (i.e., the dominant response), and instead say ‘night, and they must inhibit the dominant response of saying ‘night’ to the moon picture, and instead say ‘day’. Research suggests that children’s accuracy on this task, and others like it, improves across 3 to 5 years of age (see Carlson & Moses, 2001; Moriguchi & Hiraki, 2013).

Inhibitory control in childhood is predictive of a host of abilities, including moral judgement (Gvozdic et al., 2016), communication (Nilsen & Graham, 2009, 2012), academic performance (Allan et al., 2014), prosocial behaviour (Hao, 2017; Nilsen & Valcke, 2018), social cognition (Carlson & Moses, 2001), and emotional regulation (Rhoades et al., 2009), such that children who are better at inhibiting a dominant response, in favor of a more appropriate response, tend to show more sophisticated performance in all of the aforementioned abilities. Furthermore, inhibitory control has been linked to the ability to overcome the curse of knowledge; that is, individuals who show more accurate performance on inhibitory control tasks tend to be better at overcoming the curse of knowledge (see Bayen et al., 2007; Coolin et al., 2015, 2016; Pohl et al., 2018).

On the other hand, fluency misattribution is a metacognitive phenomenon that results in errors in a variety of perceptual, conceptual and social judgements (Harley et al., 2004; Laham et al., 2009; Mandler et al., 1987; Reber et al., 1998; Weisbuch & Mackie, 2009). This phenomenon stems from an otherwise useful heuristic, namely fluency attribution (Whittlesea, 1993). Information that has been processed before (due to prior exposure) is processed more

fluently (Jacoby & Whitehouse, 1989; Whittlesea, 1993). As such, fluency attribution serves an important memory function by signaling that we have previously encountered the information (Jacoby & Whitehouse, 1989; Whittlesea, 1993). Indeed, when participants process information fluently, they attribute those feelings of fluency to prior exposure (e.g., Jacoby & Dallas, 1981). Fluency *misattribution* occurs when the subjective feeling of fluency is attributed to an incorrect source.

In a demonstration of fluency misattribution, Whittlesea, Jacoby and Girard (1990) presented participants with lists of words. After each list of words, participants were presented with a target word that was visually degraded, making it somewhat difficult to identify. That is, each target word was partially occluded by a dynamic noise mask. Unknown to the participants, the density of this noise mask was manipulated, such that some of the target words were more visually degraded than others, making them more difficult to identify. For each target word, the participants were asked to pronounce the word, then judge whether they saw the target word in the list that was presented. The researchers found that participants pronounced the lightly degraded target words faster than the highly degraded target words, indicating that the lightly degraded words were more fluently processed. Importantly, the researchers found that participants were more likely to mistakenly indicate that they had seen the lightly degraded target words in the list, compared to the highly degraded target words. That is, participants misattributed their fluency in identifying the lightly degraded target words as indicative of previous exposure to the words (see also, Whittlesea, 1993, for the effect of fluency on judgements of meaning, pleasantness, and duration).

Whittlesea (1993) labelled the tendency to misattribute fluency as indicative of previous exposure as ‘the illusion of familiarity’, where individuals falsely claim that they have seen a

stimulus before. According to the discrepancy-misattribution hypothesis, individuals are more likely to experience the illusion of familiarity when a stimulus is unexpectedly fluent (Whittlesea & Williams, 1998, 2000, 2001). To demonstrate this, Whittlesea and Williams (1998; Experiment 3) presented participants with a memorization phase where participants were asked to remember a list of natural words (e.g., rainbow, candle), regular non-words (e.g., hension, framble), and irregular non-words (e.g., jufict, lictpub). Then, in a recognition phase, participants were presented with the words in the memorization phase, as well as new sets of natural words, regular non-words and irregular non-words. They were asked to pronounce each word, and then indicate whether they saw it in the memorization phase. The researchers found that participants were more likely to falsely indicate that they had seen regular non-words, compared to irregular non-words, which makes sense since regular non-words were more fluently processed than irregular non-words. Participants mistook the fluency in processing the regular non-words as indicative of having seen the non-words in the memorization phase. More importantly, however, the researchers found that participants were more likely to falsely indicate that they had seen regular non-words, compared to natural words, which may seem puzzling given that natural words were more fluent than regular non-words. Whittlesea and Williams (1998) suggest that fluency misattribution is more likely to occur when a stimulus is discrepantly fluent; for example, participants are less likely to expect the word *hension* to be fluently pronounced, compared to the word *table* (see also, Whittlesea & Williams, 2000, 2001). The fluency of regular non-words is more salient to participants because of the discrepancy with their expectations, than the fluency of natural words, and therefore the fluency of regular non-words is more likely to lead to fluency misattribution.

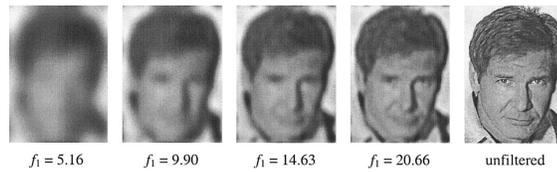
Importantly, familiarity with a stimulus due to previous exposure, can also be misattributed to other sources. That is, when individuals are exposed to a stimulus in the past, the processing of this stimulus becomes more fluent, which in turn can lead to misattributions about the source of familiarity. For example, Jacoby and colleagues (1989) found that participants were more likely to judge last names that they read 24 hours prior, as famous names, compared to newly presented last names. In other words, they mistook the fluency associated with the familiarity of the last names as indicative of how famous, or widely known, these last names were. Researchers have also reported that fluency associated with familiarity influenced judgements in the following ways: individuals mistakenly judge familiar stimuli as having been presented for a longer duration than unfamiliar stimuli (Witherspoon & Allan, 1985), individuals mistakenly judge familiar stimuli as having been visually easier to identify compared to unfamiliar stimuli (Jacoby et al., 1988; Whittlesea et al., 1990), individuals mistakenly judge familiar statements as being more true than unfamiliar statements (Begg et al., 1985; Fazio et al., 2019), and individuals mistakenly judge familiar stimuli as being brighter than unfamiliar stimuli (Mandler et al., 1987).

Harley and colleagues (2004) were the first to argue that fluency misattribution led to the curse of knowledge by using a visual hindsight bias design. In one condition (baseline), participants watched degraded images of celebrity faces gradually clarify (Figure 4.1). For each image, participants indicated when they could identify the celebrity. Subsequently, in the hindsight condition, participants were asked to watch a similar series of degraded images, but they were instructed to estimate when a naïve peer would identify the celebrities. For each trial, in the hindsight condition, participants saw a clear image of the celebrity (visual knowledge), and then the degraded images. Importantly, in this condition, participants saw the degraded images of

celebrities that they themselves identified in the baseline condition (Familiar celebrities), as well as degraded image of celebrities that they had not identified before (New celebrities).

Participants knew the identities of both the Familiar and New celebrities, but they had previous experience identifying the Familiar celebrities in the clarification process, and as such would have processed that information more fluently. Participants' knowledge of the celebrities' identity led to overestimations of how early, in the course of clarification, the naïve peer would identify the celebrities. Importantly, participants were more likely to show the bias for the Familiar celebrities compared to the New celebrities. The latter finding suggests that familiarity with processing the images evoked an even stronger feeling of fluency than that associated with knowing the celebrity, which ultimately strengthened the bias. That is, fluency misattribution contributed to the curse of knowledge over and above the effect of simply possessing knowledge.

Baseline Condition



Hindsight Condition

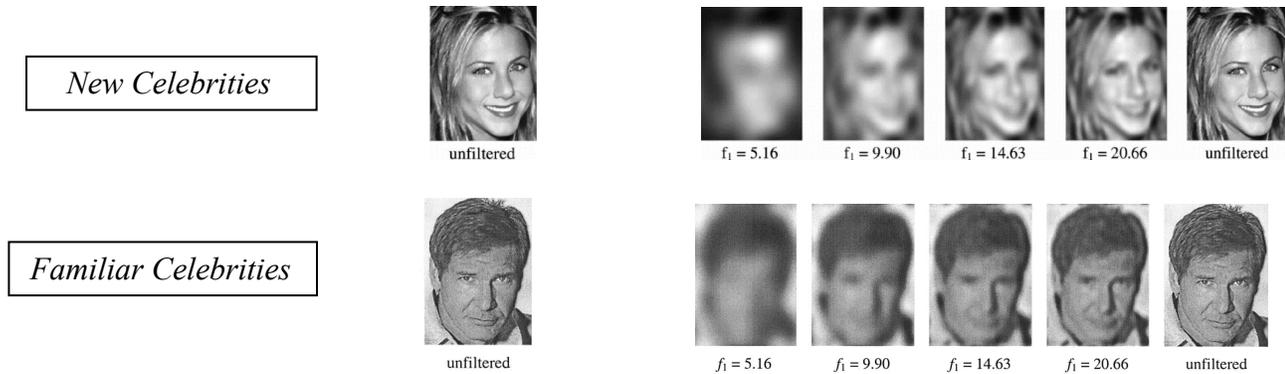


Figure 4.1. Stimuli from the study by Harley et al. (2004). In the baseline condition, participants were instructed to identify celebrities as the images clarified, and in the hindsight condition, participants were instructed to estimate when a naïve peer can identify the images. In the hindsight condition, there were Familiar Celebrities who were identified in the baseline condition, and new celebrities who were not identified before. The top and bottom images are from “The “Saw-It-All-Along” Effect: Demonstrations of Visual Hindsight Bias” by Harley et al., 2004, *Journal of Experimental Psychology: Learning, Memory, & Cognition*, 30, p. 962. Copyright 2004 by the American Psychological Association. The middle images are from “Fluency Misattribution and visual Hindsight bias” by Bernstein & Harley (2007), *MEMORY*, 15, p. 551. Copyright 2007 by Psychology Press.

Consistently, in earlier work in our laboratory, my colleagues and I demonstrated that adults show a curse of knowledge for factual questions that were shown repeatedly (and thus fluently processed), but not known (Birch et al., 2017). That is, fluency misattribution can contribute to the curse of knowledge even in the absence of content knowledge. In one study, for example, participants were presented with three lists of factual questions (e.g., “What was the first organ successfully transplanted from a cadaver to a live person?”) and were given the answers to each question, to memorize (Known questions). The participants were then presented

with a second list of factual questions that were not taught (Familiar questions). Subsequently, participants were exposed to both the Known and the Familiar questions (but not the answers) three additional times. One week later, participants were presented with the same factual questions (Known and Familiar), as well as a new list of factual questions (New questions). After each question, participants were asked to estimate the percentage of their peers who would know the answer to the question. My colleagues and I found that adults made higher peer estimates for factual questions that had been taught (Known), compared to factual questions that were not presented before (New questions); a classic curse of knowledge effect. Our results also showed that adults made higher peer estimates for factual questions that were repeatedly presented a week earlier without the answers (Familiar), compared to factual questions that were not presented before (New); a fluency misattribution effect. These findings suggest that the fluency associated with an item is sufficient in biasing adults' judgements about how common knowledge is among their peers and raise challenges for the inhibitory control account (e.g., after all, there was no knowledge content to be inhibited).

In my master's thesis research, I found preliminary support for the fluency misattribution account by revealing that unfamiliar information does not result in a curse of knowledge among children. That is, when children are taught information that is unfamiliar, and therefore dysfluent, children do not show a curse of knowledge in their social judgements. In my master's thesis, I used a Peer Estimates Task (PE Task), in which 4- to 7-year-old participants were presented with eight factual questions and were asked how many children 'about your age' would know the answers. For half of the questions, the 'Knowledgeable trials', they were told the answers before they made their peer estimates using a 5-option visual scale indicating how widely known information is among their peers (see Figure 4.2A). For the other questions, the

‘Ignorant trials’, they were not told the answers before making peer estimates. I reasoned that if children were cursed by their knowledge, they would estimate that more of their peers would know the answers in the Knowledgeable trials compared to the Ignorant trials.

One of the objectives of my master’s thesis research was to examine Manifestation 3 of the curse of knowledge among children. That is, the curse of knowledge in children’s estimates of how widely known information is among their peers. To manipulate whether children knew the answers to the questions, I taught children the answers to only half of the questions (which half was counterbalanced). To ensure that children did not already know the answers, I selected unfamiliar factual questions (e.g., ‘What animal has the best hearing?’ Answer: ‘The Greater Wax Moth’). I was surprised, at the time, to find that upon learning the unfamiliar information children showed a *reverse* curse of knowledge effect, where they predicted that less of their peers would know the answers in the Knowledgeable trials compared to the Ignorant trials. That is, upon learning the unfamiliar answers children estimated that less of their peers would know the answers, compared to when they did not learn the answers.

Even though I did not predict my master thesis’ findings (though, of course, they are more obvious in hindsight), it fits with research in adults demonstrating the unique circumstances that can lead to a reverse curse of knowledge (Pezzo, 2003). For example, Yopchick and Kim (2012) found that adults show a reverse curse of knowledge when outcome information is surprising and not easily explainable. Specifically, the researchers presented participants with stories that had two possible outcomes, and a ‘causal factor’ (the author’s term) that supported one of the outcomes. For example, participants were presented with a scenario about a battle between the Hutus and the Tutsis and were told that the Hutus have superior discipline in battle. Participants showed the bias when estimating a peer’s ability to predict the outcome, when this

outcome was supported by the causal factor (e.g., the Hutus won the battle was consistent with their superior discipline). However, participants showed a reverse curse of knowledge when they received the outcome that was inconsistent with the causal factor (e.g., the Hutus lost the battle was inconsistent with their superior discipline). Also, the researchers found that the bias did not occur at all when there was no causal factor to support the outcome information. These findings suggest that one must be able to easily integrate new information with what one already knows for the curse of knowledge to occur (i.e., it needs to make sense). A number of researchers working with adult participants have shown that, although the curse of knowledge is prevalent, if the information learned is surprising and does not make sense, the curse of knowledge is reduced or reversed (Mazursky, 1997; Mazursky & Ofir, 1990; Müller & Stahlberg, 2007; Pezzo, 2003; Pohl et al., 2002; Sanna & Schwarz, 2003).

The current research extends my master's thesis research by specifically examining the role of fluency misattribution on the curse of knowledge. My master's thesis showed that children do not show the curse of knowledge effect when they learn unfamiliar information. In this chapter, I examine how the nature of information can either induce the bias or lead to its reversal, and what that tells us about the mechanism driving the curse of knowledge. The inhibitory control account does not easily explain my master's thesis findings. The inhibitory control account suggests that the curse of knowledge occurs because of the difficulty associated with inhibiting content knowledge. In my master's thesis, however, children learned new information and did not show a curse of knowledge in their judgements. According to a strict inhibitory control account, children should have shown the curse of knowledge when taught content information, even if that information was unfamiliar. On the other hand, the findings of my master's thesis seem consistent with the fluency misattribution account of the curse of

knowledge. That is, the unfamiliarity of the information presented may have led to difficulty in processing the information, which cued children to realize the obscurity of the information. The key difference between the two accounts is that the fluency misattribution account emphasizes the effect of fluently *processing* the information, whereas the inhibitory control account emphasizes the effect of *possessing* knowledge.

To test the fluency misattribution account of the curse of knowledge, in the current study, I present children with the same task and factual questions as my master's thesis; however, I give them more familiar, but fake, answers in the Knowledgeable trials. For example, instead of telling children that the Greater Wax Moth has the best hearing, I tell them that the bat has the best hearing. Accordingly, in this study, I aim to examine the effect of familiarity on children's ability to reason about what others know. Familiarity, here, means that children encountered those words before in their daily lives (see Whittlesea and Williams, 2000; for different types of familiarity). Indeed, in my master's thesis, children were more likely to show familiarity with the answer 'bat' versus 'Greater Wax Moth'. I suspect that this familiarity would lead to fluent processing which in turn would be misattributed to the objective apparentness of that information.

I also aim to examine whether the PE task is positively correlated with the Visual Hindsight Bias Measure (VHB Measure; see Bernstein et al., 2007; Harley et al., 2004, discussed in page 8). I found that children's magnitude of the curse of knowledge was positively correlated across the two measures in my master's thesis, which makes sense since the two tasks measure different manifestations of the same bias. However, I aim to examine the reliability of this association, given the very different methodology employed by the two measures. While the PE task examines the effect of conceptual knowledge on children's estimates of their peers'

knowledge (Manifestation 3), the VHB Measure examines the effect of perceptual (i.e., visual) information on children's estimates of whether another individual can see an object (Manifestation 2).

Accordingly, my goals for Experiment 1 are as follows: 1. I aim to examine whether the curse of knowledge occurs when children are presented with familiar information in the Knowledgeable trials of the PE Task. 2. Given the literature suggesting that younger children tend to be more cursed by their knowledge than older children and adults (Bernstein et al., 2007; Birch & Bloom, 2003; Lagattuta et al., 2014), I aim to examine whether older children would be less likely to overestimate how widely-known information is among their peers when they know the information compared to when they do not. 3. I aim to examine whether children's performance on the PE task would be correlated with their performance on the VHB Measure. 4. Given that my master's thesis and Experiment 1 are identical except for the information presented in the Knowledgeable trials (see Table 4.1), I aim to compare the data from the two experiments and directly examine the role of the familiarity of information on the curse of knowledge.

4.1 Experiment 1

4.1.1 Method

4.1.1.1 Participants

I recruited 176 children in Vancouver, BC, Canada, between November 2016 to November 2017. I recruited this sample through multiple strategies, including contacting families through the university's recruitment database, approaching parents in a children's museum, and contacting parents through school recruitment, where teachers sent students home with consent forms. Predominately, I recruited children who belonged to middle or upper-middle

class families. I aimed for a sample of children who were functional in English, and who spoke English at least 50% of their daily lives. I only tested children who were eager to participate. Children were tested individually in our laboratory, at a museum, or at local schools.

For the PE task, I analyzed the data of 104 children (46% male). Of the children who were excluded from analyses, 11 were excluded because they did not follow instructions, or did not complete sufficient trials. Forty-one participants were excluded because they failed the exclusion criteria (discussed Procedure) suggesting that they did not understand the task. Lastly, 20 participants' data were excluded because they knew the true answer to at least one of the questions that they were taught. Recall that for the current experiment, I replaced the true (unfamiliar) answers with fake (familiar) answers, so children who knew the true answers may have questioned the experimenter's validity as an informant, and therefore the experimental manipulation could have been compromised. Nonetheless, the findings were consistent even if I did not exclude these children's data. I decided to exclude their data though, since they did not meet my pre-set exclusion criteria.

According to G*Power analyses, given the current sample size, I had 0.90 power of detecting a medium effect of the magnitude of bias, if it existed. Children who were included in my analyses were 4 to 7 years old (Mean = 5 years, 11 months; Range = 4 years, 0 months to 7 years, 11 months): 25 4-year-olds (Mean = 4, 5; Range = 4,0 to 4,9), 29 5-year-olds (Mean = 5,5; Range = 5,0 to 5,11), 24 6-year-olds (Mean = 6,5; Range = 6,0 to 6,11), and 26 7-year-olds (Mean = 7,7; Range = 7,0 to 7,11). Forty percent of the participants were Caucasian, 29% were East or Southeast Asian, 4% were South Asian, and 19% were of mixed background. As for the remaining 8%, parents did not disclose their ethnic background.

From the 176 children recruited, 105 children participated in the VHB Measure. From this sample, 93 children's data were included in our analyses of the VHB Measure. Of the children who were excluded from analyses, 5 decided not to continue with the study, 4 were very distracted, 3 did not follow instructions or did not understand the task (e.g., a child thought that Ernie knew which toys were hidden because he overheard the researcher). According to G*Power analysis, given our sample size, we had over .90 power of detecting a medium effect size of the hindsight bias, if it existed. The children included in our analyses were 4 to 7-years-old (Mean = 5 years, 10 months; Range = 4 years, 1 month to 7 years, 11 months; 47% male). Thirty-two 4-year-olds (Mean = 4, 5; Range = 4,1 to 4,11), 17 5-year-olds (Mean = 5, 5; Range = 5,1 to 5,11), 21 6-year-olds (Mean = 6, 6; Range = 6,0 to 6,11), and 23 7-year-olds (Mean = 7, 6; Range = 7,1 to 7,11). Forty-seven percent of the participants were Caucasian, 25% were East or Southeast Asian, 5% were South Asian, and 18% were of mixed background.

4.1.1.2 Material

For the PE Task, I used a 5-point visual scale that represents a progressively larger number of peers: 'none', 'a couple', 'some', 'a lot' and 'a whole lot'. For example, the first point is 'none' and the box does not have any child silhouettes, the second point is 'a couple' and the box contains two child silhouettes, the third point is 'some' and the box contains six child silhouettes, and so on (Figure 4.2A).

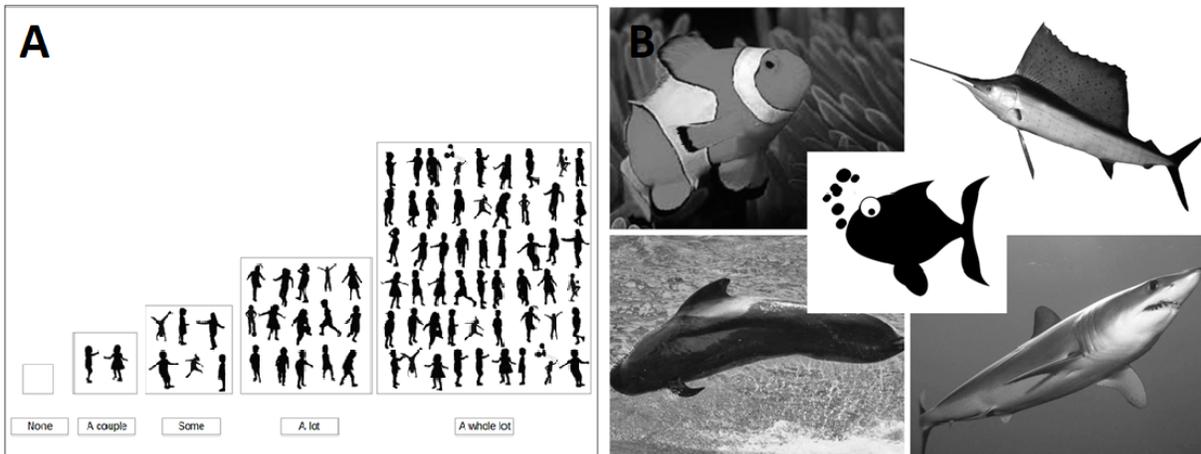


Figure 4.2. Materials for PE Task Experiment 1. Panel A. The PE Task’s 5-option Visual scale. Children used this scale to indicate their estimates of how many peers would know the answers to each question. Panel B. Sample picture with 4 possible options for the questions. After testing, we assessed children’s knowledge about the questions presented in the Test Phase. Children were asked each question and presented with 4 possible answers.

For the VHB Measure, I used materials akin to those used in the VHB measure in Bernstein et al. (2007). The procedure involved 4 objects that measured up to 5 inches long: a bird, a school bus, a tree, and a horse. The materials included a box measuring 13 by 15 inches with one open side and a three-ringed binder placed on top of the box. The binder held 10 clear plastic sheets speckled with black dots. These sheets hung in front of the open side of the box, to occlude the participant’s view of what was inside. Each sheet had a unique pattern of black dots that cover 5% of each sheet, such that each sheet was transparent but blurry. Behind the tenth sheet it was extremely difficult to identify the object inside the box, however as the experimenter flipped through the sheets the view of the object became clearer (Figure 4.3).

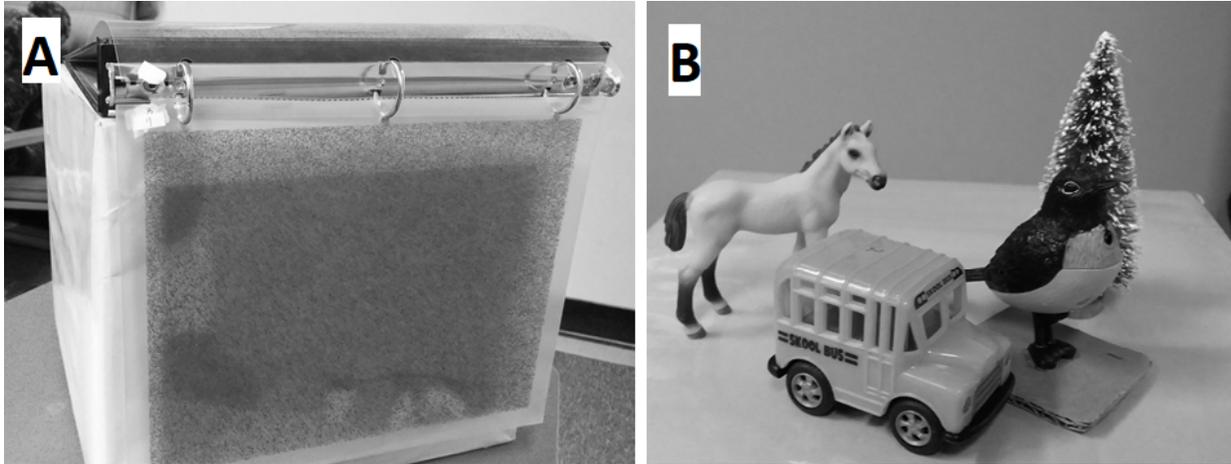


Figure 4.3. Materials for VHB Measure. We used this material to conduct the VHB Measure (Bernstein et al., 2011). We used the binder and the box in Panel A to hide each toy (see Panel B). In the Baseline condition, we hid each toy in the box and covered it up with the 10 plastic sheets making it difficult to see what was inside the box. Then we removed one sheet at a time and asked children what was inside the box. In the Hindsight condition, we repeated the same procedure, however children were asked to estimate when Ernie, a puppet, could see inside the box.

4.1.1.3 Procedure

4.1.1.3.1 Warm-up Phase.

For each study session, the experimenter introduced herself, and asked the child some questions about his or her hobbies, to get the child comfortable answering questions. The experimenter then showed the participant the scale and discussed the different amounts of children associated with each point on the scale (Figure 4.2A). Then, the experimenter explained that she will ask some questions about how many of the child's peers will know different things. The child was asked to indicate their answers by pointing to one of the points on the scale.

4.1.1.3.2 Demonstration Phase.

Subsequently, the experimenter administered 3 demonstration trials where she showed the child how to make a peer estimate. Through these trials, the experimenter showed the participant how to make a peer estimate for an easy question, a difficult question, and a question that is medium in difficulty. To demonstrate an easy question, the experimenter asked, ‘A cow says moo. How many children your age will know that?’ then the experimenter went on to say that a whole lot of children will know the answer to that question, and she pointed to ‘a whole lot’ on the 5-point scale. To demonstrate a difficult question, the experimenter showed children a picture of an odd object and said ‘This is a holophonor. How many children your age will know that?’ Then the experimenter went on to say that no children would know the answer to this question, and she pointed to ‘none’ on the 5-point scale. Lastly, to demonstrate a medium in difficulty question, the experimenter asked, ‘a giraffe is the tallest animal. How many children your age will know that?’ then she went on to say that some children will know the answer to this question, and she pointed to ‘some’.

4.1.1.3.3 Peer Estimates (PE) Test phase.

For the experimental trials, the experimenter asked the child to make peer estimates about how many children would know different factual questions. Each question was presented with a corresponding picture (e.g., a question about fish was presented with a silhouette of a fish). The questions were divided into two sets; Sets A and B. All participants were presented with both sets of questions: half of the participants were taught the answers to Set A, and not Set B, whereas the other half were taught the answers to Set B, and not Set A. Thus, answers for half of the questions were taught to children prior to making their peer estimate (e.g., “An eagle can fly the highest. How many children will know which kind of bird can fly the highest?”), and the

other half of the questions were not taught (e.g., “How many children will know which kind of bear is the largest?”). Half of the participants were presented with Set A first, whereas the other half were presented with Set B first.

The child was then asked two post-test questions to further confirm that they understood the 5-point scale: ‘How many children like to be happy?’ and ‘how many children like to be sad?’. If the child indicated that *less* than ‘some’ of their peers like to be happy, or that *more* than ‘some’ of their peers like to be sad, then we inferred that the child did not understand the task and we excluded their data. Lastly, the experimenter asked the child the factual questions with 4 possible response options (Figure 4.2B), to confirm that children did not know the answers to the questions they were not taught.

Note, Experiment 1 used the same procedure and material as my master’s thesis. Except, I replaced the accurate, but unfamiliar, answers to the questions with fake, but familiar, answers. Specifically, in Experiment 1, I used the most commonly selected guesses from my master’s thesis, when children were asked the factual questions and presented with four possible answers. For example, I told participants that the ‘ladybug’ instead of the ‘fairyfly’ is the smallest insect (see Table 4.1). During informed consent, I told parents that children would be given fake answers to some factual questions, but at the end they would be told the correct answers.

4.1.1.3.4 Visual Hindsight Bias (VHB) measure.

Following the PE Task, most children agreed to participate in the VHB measure. In a Baseline condition, the experimenter hid a toy inside a box, behind the ten speckled sheets, such that it was impossible for the child to see the hidden toy. The experimenter then removed one sheet at a time, so that the toy became progressively clearer, and the child was asked what he or she thought the toy was, before each sheet was removed. When the child identified the toy, the

experimenter noted how many of the ten sheets had been removed. The experimenter completed this procedure 4 times with 4 different toys. In the Hindsight condition that followed directly (i.e., with no delay), the experimenter introduced Ernie, the puppet, and explained that Ernie is just as smart as the participant, but he had never played this game before. The experimenter told the child that they will be playing the same game with Ernie. The experimenter then hid each toy, and once again, removed each sheet. During this round, the child was asked to indicate when Ernie could identify the toy behind the sheets. When the child indicated that Ernie identified the toy, the experimenter asked the child ‘what will Ernie say is inside the box?’. If the child indicated the appropriate toy, the experimenter noted the number of sheets removed. Then, the experimenter asked the child *how* Ernie knew what was inside the box. This question confirmed that the child believed that Ernie could identify the toy because he could currently see it, as opposed to the child thinking that Ernie heard the researcher say the name of the toy in Baseline condition (only one child’s data was excluded for that reason).

Table 4.1

Mean peer estimates for each question across Knowledge and Ignorant trials in my master's thesis and Experiment 1.

		Master's thesis				Experiment 1			
	Factual Question	Answers	Magnitude of Bias	Mean peer estimate in Knowledgeable Trials	Mean peer estimate in Ignorant trials	Familiar Answers	Magnitude of Bias	Mean peer estimate in Knowledge Trials	Mean peer estimate in Ignorant trials
Set A	How many children will know which kind of insect, or bug, is the smallest?	Fairyfly	-0.67*** ($p < .001$)	1.39	2.06	Ladybug (57.4%)	0.59*** ($p < .001$)	2.39	1.80
	How many children will know which kind of bear is the largest?	Polar bear	0.12 ($p > 0.60$)	2.41	2.29	Polar bear (44.7%)	0.46** ($p = .004$)	2.76	2.3
	How many children will know which kind of dog cannot swim?	The basset hound	-0.04 ($p > 0.85$)	1.98	2.02	Poodle (27.7%)	0.73*** ($p < .001$)	2.37	1.64
	How many children will know which animal is the best jumper?	Flea	-0.52** ($p = .005$)	2.27	2.79	Rabbit (40.4%)	0.36** ($p = .006$)	3.22	2.86
Set A Average			-0.29* ($p = .015$)	2.00 (SD = .79)	2.29 (SD = .85)		0.54*** ($p < .001$)	2.69 (SD = .53)	2.15 (SD = .64)
Set B	How many children will know which kind of bird can fly the highest?	Ruppell's vulture	-0.60** ($p = .002$)	1.81	2.41	Eagle (65.3%)	0.82*** ($p < .001$)	2.78	1.96
	How many children will know which part of the human body is called a nape?	The back of the neck	0.16 ($p > .35$)	1.33	1.16	Top of the head (42.9%)	- 0.03 ($p > .85$)	1.54	1.57
	How many children will know which animal is the fastest in the sea?	Sailfish	-.05 ($p > 0.75$)	2.15	2.20	The Mako shark (46.9%)	0.07 ($p > .70$)	2.20	2.13
	How many children will know which animal has the best hearing?	The greater wax moth	-0.51** ($p = .004$)	1.73	2.24	Bat (49%)	0.50* ($p = .012$)	2.46	1.96
Set B Average			-0.25* ($p = .049$)	1.75 (SD = .90)	2.00 (SD = .72)		0.34** ($p = .009$)	2.25 (SD = .88)	1.91 (SD = .84)

Notes. For the Magnitude of Bias columns, I reported the mean difference in peer estimates for Knowledgeable and Ignorant trials, and I ran one-sample t-tests to examine whether the differences were significantly different than zero (i.e., no bias). I reported the p-values in brackets. In Experiment 1, I taught children fake but familiar answers in the Knowledgeable Trials. These answers were most commonly selected as being correct in my master's thesis. In brackets, under the familiar answers, I reported the percentage of children who selected those answers as being correct in the Ignorant trials of my master's thesis. SD is Standard Deviation.

4.1.2 Results

4.1.2.1 PE Task: Did children show the curse of knowledge?

To examine the effect of knowledge on peer estimates, I created a magnitude of bias score for each child by subtracting each child's peer estimate in the knowledgeable trials by the average peer estimate in the Ignorant (baseline) trials for each factual question. Given that children estimated that some factual questions were more difficult than others (e.g., see Table 4.1, for a summary of the baseline estimates of peer's knowledge by question), I used these difference scores to account for differences in question difficulty. That is, for each factual question (e.g., How many children will know which bird can fly the highest?), I subtracted each child's peer estimate when they were taught the answer to the question (e.g., An eagle can fly the highest) by the average peer estimate when children did not learn the answer to the question (see Blank, Nestler, von Collani & Fischer, 2008, for similar analyses). I then examined whether the average magnitude of bias across factual questions was significantly different from zero. Children showed a significant curse of knowledge effect, such that the magnitude of the bias ($M = .44$) was significantly bigger than zero, $t(103) = 6.24, p < .001, d = 0.61$. Children made bigger peer estimates when they were taught the answers to the factual questions, compared to when they were not taught: the classic curse of knowledge.

4.1.2.2 PE Task: Did the magnitude of bias decrease with age?

I conducted a multiple linear regression investigating the effect of age (in months) on the average magnitude of bias, as well as the effect of set order (i.e., whether children were presented with Set A or B first), Set taught (i.e., whether children were taught the answers to Set A or Set B) and sex. I found that these variables did not explain a significant amount of the variance in the magnitude of the bias, $F(4, 98) = 1.18, p = .32$, with an adjusted R^2 of 0.01. There

was a trend in Age such that younger children showed a bigger magnitude of bias, however the effect of Age was not significant ($\beta = -.16, p = .12$; see Figure 4.4).

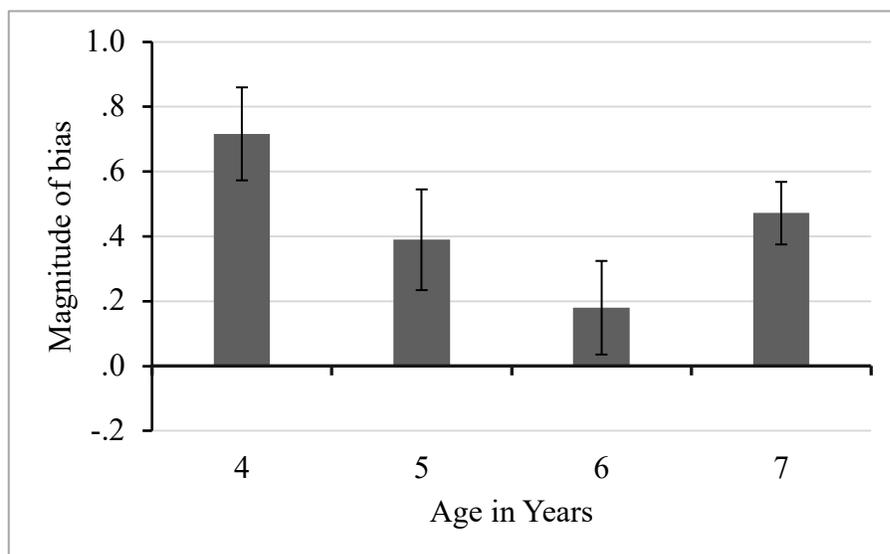


Figure 4.4. The magnitude of bias by age in Experiment 1 in PE task. Overall, children showed a magnitude of bias that is significantly bigger than zero, demonstrating the typical curse of knowledge effect. Children made higher peer estimates when they learned the familiar answers to the factual questions, compared to when they did not learn the answers. Error bars reflect standard error.

4.1.2.3 VHB Measure: Did children show the bias? If so, did it decrease with age?

For my main analyses here, I conducted multilevel regression using the lme4 package (Bates et al., 2007) in the R environment (R core development team, 2016), and I probed interactions and calculated regions of significance with the online calculator (Preacher et al., 2006). Maximum likelihood was used for estimation. To examine whether children showed the curse of knowledge in the VHB measure, I entered the number of sheets removed to uncover the identity of the toy as a continuous dependent variable nested within participants (i.e., 0 to 10). Condition, centered age (in months), and their interaction were entered as predictors. I also

found a significant interaction between age and condition, $b = .02$, $SE = .009$, $t = 2.34$, $p = .02$. Probing the interaction suggested that the number of sheets removed in the hindsight condition was significantly lower than in the Baseline condition across all ages, and the difference was larger for younger than older participants. Younger children showed a bigger curse of knowledge effect than older children (Figure 4.5); at age one standard deviation below the mean, $b = -1.91$, $SE = .19$, $t = -10.26$, $p < .001$, at mean age, $b = -1.60$, $SE = .13$, $t = -12.19$, $p < .001$, at age one standard deviation above the mean, $b = -1.29$, $SE = .19$, $t = -6.98$, $p < .001$.

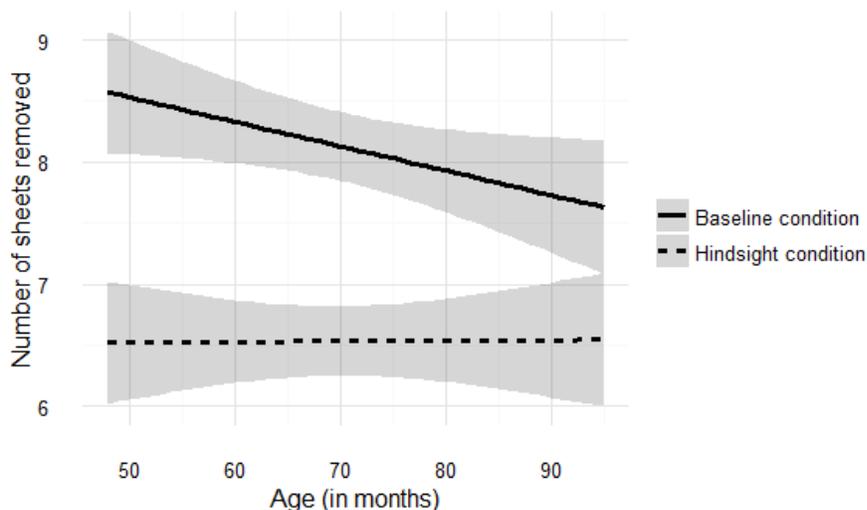


Figure 4.5. Performance on the VHB Measure across age in Experiment 1. This figure shows the mean number of sheets removed to identify the hidden objects in the Baseline and Hindsight conditions across age. Note, the bands indicate 95% confidence intervals.

4.1.2.4 Did performance on the PE Task and the VHB Measure correlate?

I did not find a significant bivariate correlation between the magnitude of bias in the PE task and VHB Measure, $r(62) = .16$, $p = .21$. Indeed, the correlation was not significant, when we controlled for age, $r(59) = 0.12$, $p = .37$. That is, children's performance on the PE task appeared to be unrelated to their performance on the VHB Measure.

4.1.2.5 Does the magnitude of the bias differ across my master's thesis and Experiment 1?

To directly compare the effects of being taught familiar versus unfamiliar answers to the questions, I conducted a multiple linear regression investigating the effect of experiment (1, 2), as well as age on the magnitude of bias. I found a significant regression equation, $F(2, 201) = 30.21, p < .001$, with an adjusted R^2 of 0.22. Both experiment ($\beta = .39, p < .001$) and age ($\beta = -.27, p < .001$) were significant predictors of the curse of knowledge. When children were taught familiar answers to the factual questions, they made bigger peer estimates for the Knowledgeable trials compared to the Ignorant trials. And overall, younger children showed a bigger curse of knowledge compared to older children.

4.1.3 Discussion

In Experiment 1, when children were taught familiar answers to the same factual questions as my master's thesis, children made higher peer estimates for the questions that were taught versus the questions that were not taught, as predicted. That is, upon hearing answers that contained familiar items, children estimated that the questions that they were taught would be more widely known compared to the questions that they were not taught. There was some support for our second prediction; there was a trend for the curse of knowledge to decrease with age, although it was not significant. When I combined my data from my master's thesis and Experiment 1, age was a significant predictor of the magnitude of the bias. Accordingly, the age-related decline previously observed in the other two manifestations of the curse of knowledge is seemingly also present in children's judgments of how prevalent knowledge is among one's peers, but it may not be as robust. Also, this time, when I taught children familiar answers in the PE task, I did not find a significant correlation between the magnitude of the bias in the PE task

and the VHB measure. I discuss the implications of not finding a correlation between the two tasks in the General Discussion.

Importantly, I found an interaction between my master's thesis and Experiment 1 on peer estimates, such that children showed a curse of knowledge in Experiment 1 but not in my master's thesis; children showed a reversal of the bias in my master's thesis. That is, children who were presented with unfamiliar answers to the questions made lower peer estimates compared to baseline (Ignorant) estimates; whereas children who were presented with familiar answers to those same questions made bigger peer estimates compared to the baseline estimates.

One limitation of the cross-experimental comparisons is that children were not randomly assigned to the two experiments. My master's thesis data collection was completed before I started data collection for Experiment 1. To address this limitation, I conduct Experiment 2, where children are randomly assigned to one of three conditions: an Unfamiliar condition, a Familiar condition, or an Ignorant condition. In all three conditions, I present children with the same six factual questions. In the Ignorant condition, I do not teach children answers to the factual questions, before asking them to make peer estimates (e.g., 'How many children will know which kind of insect can fly the highest?'). In the Familiar condition, I teach children familiar answers to the factual questions (e.g., 'the *butterfly* can fly the highest') before asking them to make peer estimates. In the Unfamiliar condition, I teach children unfamiliar answers to the factual questions (e.g., 'the *stonefly* can fly the highest') before asking them to make peer estimates.

In Experiment 2, I use a different set of questions from those used in my master's thesis and Experiment 1, to confirm that the curse of knowledge is not specific to the questions used in those experiments. This time, I directly examine the effect of familiarity on the curse of

knowledge. To select factual information for this experiment, I noted eleven pairs of familiar and unfamiliar animals, and I conducted a pretest to confirm that one of the animals was more familiar to children than the other. In the pretest, I asked twelve 5 to 8-year-old children (67% male), who did not participate in the later experiment, which of two similar animals they heard of before. For example, I asked children which type of eagle they heard of before, a bald eagle or a harpy eagle. I selected six pairs of animals for Experiment 2 where at least 11 (out of 12) children chose the ‘familiar’ animal. I then created six factual questions that fit with either the familiar or unfamiliar animal. For example, in Experiment 2, I present children with the question ‘which type of insect, or bug, can fly the highest?’, because both the *butterfly* and the *stonefly* can fly the highest relative to other flying insects. See Table 4.2 for an overview of the six factual questions and their corresponding familiar and unfamiliar animals.

Again, in Experiment 2, I aim to examine the role of fluency misattribution on the curse of knowledge in children’s peer estimates. In this experiment, the fluency misattribution account would predict that children would show the curse of knowledge when taught familiar information, and they would *not* show the curse of knowledge when taught unfamiliar information, and they may even show a reversal of the bias. That is, when information is fluently processed (due to familiarity), children would overestimate how widely known the information is among their peers, however when information is dysfluent, children would realize the obscurity of the information and not become biased in their peer estimates. In contrast, the inhibitory control account, as it stands, would predict that children would show a curse of knowledge for both familiar and unfamiliar information.

I hypothesize that the fluency misattribution account would provide a better fit for my findings, compared to the inhibitory control account. I predict that children would only show the

curse of knowledge when they are taught familiar information, and children would not show the curse of knowledge in the unfamiliar condition, and they may even show a full reversal of the bias, as in my master's thesis.

4.2 Experiment 2

4.2.1 Method

4.2.1.1 Participants

I recruited 149 children in Vancouver, BC, Canada, between May to December 2019. I used the same recruitment strategies as Experiment 1, and I aimed for similar sample characteristics. Predominately, children belonged to middle or upper-middle class families. One hundred and thirty-eight children (46% male) were included in our analyses. Of the children who were excluded from analyses; 5 children were distracted, 4 children were atypically developing (e.g., diagnosed ASD), and 2 did not understand the task (e.g., provided their own answers to factual questions when they were asked for peer estimates). Children were tested individually at a university laboratory, children's museum, and local schools. The participants were 6 to 7 years old (Mean = 6 years, 11 months; Range = 6 years, 0 months to 7 years, 11 months). The sample consisted of 71 6-year-olds (Mean = 6, 7; Range = 6,0 to 6,11), and 66 7-year-olds (Mean = 7, 5; Range = 7, 0 to 7, 11). Thirty-seven percent were Caucasian, 29% were East and Southeast Asian, 5% were South Asian, 4% Middle Eastern, 1% aboriginal, 1% Latin American, and 13% were of mixed background (e.g., half east Asian, half Caucasian). As for the remaining 10% of children, parents did not indicate ethnicity. According to G*Power post-hoc analyses, I had 0.81 power of detecting an effect of the curse of knowledge, if it existed.

4.2.1.2 *Material and Procedure*

Each child watched a series of PowerPoint slides with an experimenter who guided them through the study. Like Experiment 1, the session began with a demonstration of how to use the 5-point scale (see Experiment 1, Procedure). Then, the PowerPoint slides presented the experimental trials. Each child was presented with factual questions and asked to make a peer estimate on the 5-point scale (see Table 4.2, for questions and answers). In the Ignorant condition, children were not presented with answers to the factual questions. In the Familiar condition, children were presented with familiar answers. And in the Unfamiliar condition, children were presented with unfamiliar answers. At the end of each condition, children were asked the answers to the factual questions and provided with three possible answers. Again, I counterbalanced the order of the factual questions by splitting the six questions into Set A and B, and I presented half of the children with Set A first, and the other half with Set B first.

4.2.2 *Results*

4.2.2.1 *Did children show the curse of knowledge in the Familiar and Unfamiliar conditions?*

As in Experiment 1, I computed the magnitude of bias scores for the Familiar and the Unfamiliar conditions. As predicted, I found a curse of knowledge effect in the Familiar condition, where the magnitude of the bias was significantly higher than zero, $t(45) = 2.60$, $p = .012$, $d = .38$: Children gave higher peer estimates when they were taught familiar answers to the questions; they were cursed by their knowledge (Figure 4.6). A multiple linear regression investigating the effect of set order and sex on the magnitude of bias in the Familiar condition, found that these variables did not explain a significant amount of variance ($p > .80$).

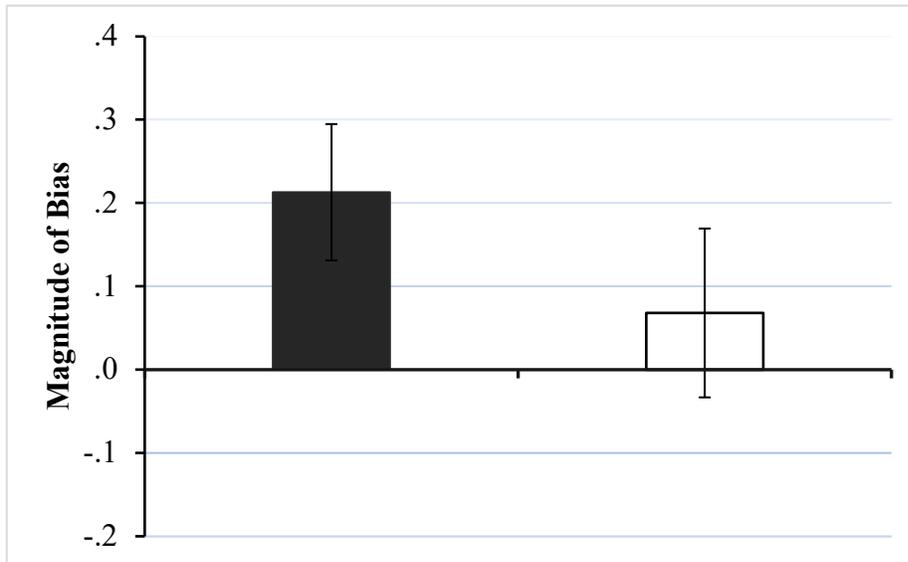


Figure 4.6. The magnitude of bias across Familiar and Unfamiliar conditions in PE task, Experiment 2. Children showed a magnitude of bias that is significantly bigger than zero in the Familiar condition, but not the Unfamiliar condition. Error bars reflect standard error.

Importantly, we also found that the bias was not significantly different than zero in the Unfamiliar condition ($p > .50$): Children did not give higher peer estimates when they were taught unfamiliar answers to the questions; they were not cursed by their knowledge. Interestingly, they also did not give lower peer estimates when children were taught the unfamiliar answers, as they did in my master's thesis.

Table 4.2

Mean magnitude of bias for Familiar and Unfamiliar Conditions in Experiment 2.

Factual Questions	Familiar Condition (n = 46; Mean age = 7 years; 41% male)		Unfamiliar Condition (n = 46; Mean age = 7 years; 41% male)		Ignorant Condition (n = 45; Mean age = 7 years; 53% male)
	Answers	Magnitude of Bias	Answers	Magnitud e of Bias	
How many children will know which kind of bear can run the fastest?	Grizzly M = 1.98	.13 (<i>p</i> = .40)	Pizzly M = 1.91	.07 (<i>p</i> = .69)	M = 1.84
How many children will know which kind of bee is the least likely to sting?	Bumblebee M = 2.28	.06 (<i>p</i> > .70)	Solitary bee M = 1.98	-.24 (<i>p</i> = .18)	M = 2.22
How many children will know which kind of insect is the heaviest?	Beetle M = 1.76	.36* (<i>p</i> = .043)	Weta M = 1.63	.23 (<i>p</i> = .17)	M = 1.40
How many children will know which kind of insect can fly the highest?	Butterfly M = 2.37	.28 ⁺ (<i>p</i> = .097)	Stonefly M = 2.04	-.05 (<i>p</i> > .80)	M = 2.09
How many children will know which kind of bird is the smartest?	Crow M = 2.24	.36* (<i>p</i> = .039)	Jackdaw M = 1.72	-.15 (<i>p</i> = .40)	M = 1.87
How many children will know which kind of fish changes colours?	Goldfish M = 1.43	.08 (<i>p</i> > .75)	Goby fish M = 1.91	.56* (<i>p</i> = .01)	M = 1.36
Average	2.01 (SD = .55)	0.21* (<i>p</i> = .012)	1.87 (SD = .69)	0.07 (<i>p</i> = .51)	1.80 (SD = 0.65)

Notes. In the Magnitude of Bias columns, I reported the mean difference between Ignorant and Familiar Conditions and Ignorant and Unfamiliar Conditions. I reported the alphas in brackets, showing whether the magnitude was significantly different than zero (i.e., no bias). For each factual question, I reported the mean peer estimate across conditions. In the last row, I reported the overall means for the Ignorant, Familiar and Unfamiliar conditions. SD is Standard Deviation. M is mean.

As with my master's thesis and Experiment 1, the magnitude of the curse of knowledge varied by item/question (see Table 4.2), and the item variability may further illustrate how the

nature of the *answer* to the question influenced the bias (not just the question itself), supporting our findings that it was not possessing (and failing to inhibit) knowledge per se that led to the curse of knowledge, but rather how that knowledge was processed (e.g., fluently or not). Figure 4.7 visually illustrates not only the variability in the magnitude of the bias but also the direction. As can be observed, the magnitude of the bias tended to be bigger in the Familiar condition for all questions, except for Question 6 which asked: “How many children will know which kind of fish changes colors?” For the Familiar condition, I told children that the goldfish changes colors, and for the Unfamiliar condition, I told children that a goby fish changes colors. I knew from the pretest (described in Experiment 2, Procedure) that goldfish was more familiar (and should be more fluent) than goby fish. However, the goby fish changes color to camouflage *immediately* in the presence of a predator, whereas a goldfish’s color changes *gradually across its lifetime*. I suspected that this latter form of changing color did not occur to the children and therefore this answer seemed implausible to them, disrupting the fluency of processing this information. Indeed, previous research suggests that implausible, or surprising information, can disrupt fluency and diminish, negate, or even reverse the curse of knowledge (Mazursky, 1997; Pezzo, 2003; Yopchick & Kim, 2012).

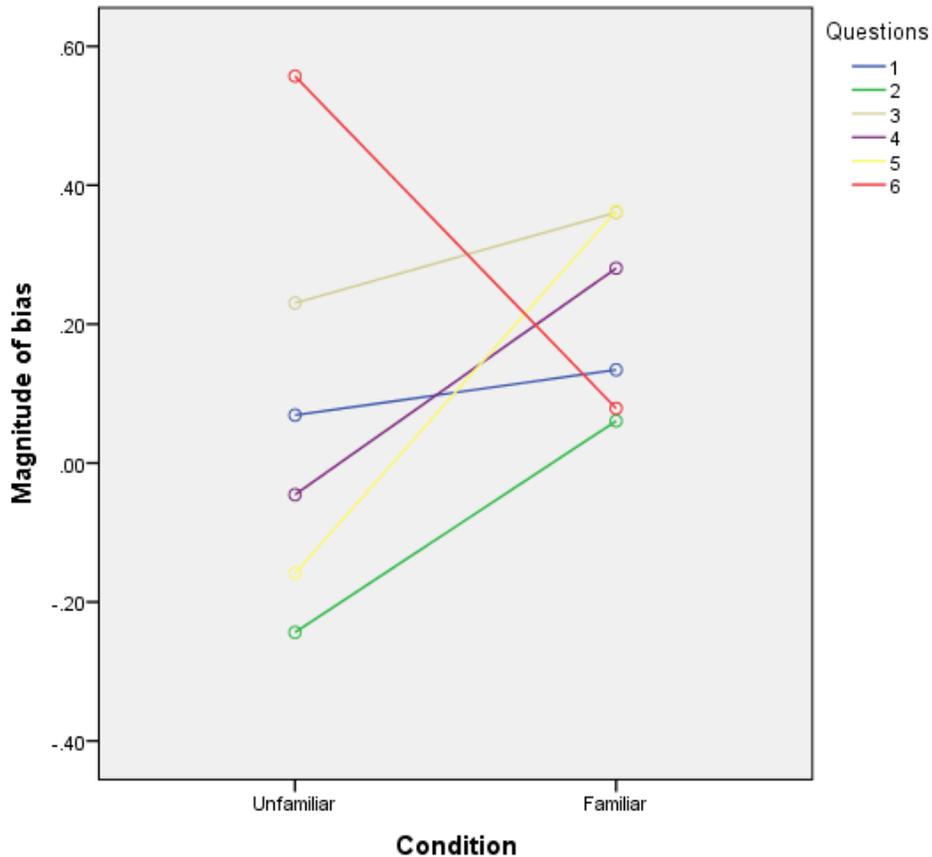


Figure 4.7. The effect of condition on the magnitude of the bias for each factual question in Experiment 2.

Next, I examined the effect of familiarity on the magnitude of the curse of knowledge using an independent t-test comparing the effect of condition on the magnitude of the bias. I found that there was no significant overall effect of familiarity on the magnitude of the bias, $t(90) = -1.11, p = .269, ns$. However, excluding the item 6 outlier discussed above revealed a significant effect of familiarity, $t(90) = -2.02, p = .046$ on the magnitude of bias across Familiar (Mean bias = .24) and Unfamiliar (Mean bias = -.03) conditions, further supporting my main prediction.

4.2.3 Discussion

Children only showed a curse of knowledge effect when they were taught familiar information. That is, they made bigger peer estimates when they were taught familiar information, compared to when they were not taught the answers to the factual questions. In contrast, children did not show a significant difference in their peer estimates when they were taught unfamiliar information, compared to when they were not taught the answers. Accordingly, fluency misattribution plays a necessary role in the occurrence of the curse of knowledge in children's inferences about what their peers would know.

4.3 General Discussion

In this chapter, I examined whether the curse of knowledge affects children's estimates of how widely known information is among their peers. Across two experiments and my master's thesis, I presented children with factual questions, and I taught them the answers to half of those questions. In my master's thesis, I presented children with unfamiliar answers to half of the questions, and I found that children made *lower* peer estimates when they were taught the answers, compared to when they were not taught the answers. In comparison, in Experiment 1, I presented children with familiar (but fake) answers to half of the same factual questions, and they made *higher* peer estimates when they were taught the answers, compared to when they were not taught the answers (i.e., they showed the curse of knowledge effect). In Experiment 2, I directly compared children's peer estimates when taught familiar answers, and children's peer estimates when taught unfamiliar answers, with baseline estimates, where children were not taught any answers to the factual questions. Again, I found that children showed the curse of knowledge only when taught familiar answers. They did not show the curse of knowledge when taught unfamiliar information.

These experiments provide support for the fluency misattribution account of the curse of knowledge and provide the first evidence for this in children's ability to reason about what others know. The fluency misattribution account suggests that the curse of knowledge occurs when we, as humans, misattribute the fluency (ease, or familiarity) associated with information. Instead of correctly attributing the ease to our familiarity with the information, we misattribute that ease as it being indicative of how apparent that information is to others (or how widely known it is among others). In other words, when considering other individuals' perspectives, we tend to use the fluency of the information as a cue for how obvious or widespread the answer is (e.g., Birch et al. 2017; Harley et al., 2004). When children found the information familiar and easy to process, they tended to misattribute their subjective ease in processing the information as indicative of its *objective* ease. While I confirmed the role of fluency misattribution in children's judgments of how widely known information is (Manifestation 3), I propose that fluency misattribution would affect children's curse of knowledge similarly whether one is considering a previously held perspective (Manifestation 1) or is considering another individual's perspective (Manifestation 2), however, future research is needed to address that question empirically.

Researchers have also put forth 'sense making' as a mechanism that leads to the curse of knowledge. When individuals can make sense of certain information (e.g., it fits with their knowledge structure), they are more likely to overestimate the apparentness of this information (e.g., Pezzo, 2003; see also, Bernstein et al., 2018, suggesting that fluency misattribution is a better account for the bias). On the other hand, if information is surprising, implausible, or does not make sense, researchers tend to find either no bias (e.g., Birch & Bloom, 2007) or even a reversal of the bias (e.g., Yopchick & Kim, 2012). Consider, for example, Birch and Bloom's (2007) study (discussed in Chapter 3, pages: 41 to 42). Birch and Bloom (2007) found that even

adults can be cursed by their knowledge when reasoning about a protagonist's (Vicki) false belief. Specifically, when adults were told where Vicki hid her violin (red box), they made higher probability ratings that Vicky would look in that box compared to when they did not know the current location of the violin. Critically, adults only showed the curse of knowledge effect if they had a plausible reason to believe that Vicky would look in that box (i.e., the red box occupied the old location of the box where Vicky originally placed the violin; Figure 3.1), and not when they did not have a plausible reason to believe that Vicky would look in the box where they knew that the violin had been moved. That is, adults were biased by their knowledge when they were given outcome information that made sense to them.

I believe that the sense making account is consistent with, or subsumed under, a fluency misattribution account. That is, I believe that when individuals can make sense of new information, they experience a feeling of fluency, and that fluency can be misattributed to another source (e.g., how objectively obvious that information is). In contrast, when the information is surprising *and* does not make sense, individuals' feelings of dysfluency become salient and leads them to realize the obscurity of the information, which in turn interferes with the curse of knowledge.

Why did children show a significant reversal of the bias in my master's thesis, but not in Experiment 2? I suspect that the answers in my master's thesis felt more dysfluent than the answers in Experiment 2, because they were more unfamiliar. However, it is also possible that the answers in my master's thesis also seemed more surprising, or implausible, than answers in Experiment 2. For example, children in my master's thesis may have been genuinely surprised to learn that a moth, and not the bat, has the best hearing. Or, that a flea, and not a rabbit, is the best jumper. However, in Experiment 2, children may have had no inclination about the answers for

the factual questions. For example, it is possible that children had no expectations about which bug is the heaviest. Indeed, adults show a complete reversal of the bias when they find information surprising and unexplainable (e.g., Yopchick & Kim, 2012). Although Experiment 2 did not reveal a full reversal of the bias, the primary hypothesis that familiar answers will lead to a curse of knowledge, whereas unfamiliar answers will not, is supported. I suspect that whether participants show no curse of knowledge or a full reversal is due to the degree of dysfluency experienced with different stimuli.

Interestingly, the finding that children do not show the curse of knowledge when reasoning about unfamiliar information suggests that children are less vulnerable to the bias than earlier work might have implied. Indeed, 4- to 7-year-old children were fairly competent at making judgements about the prevalence of a piece of knowledge. When they were exposed to unfamiliar answers, they realized the obscurity of the questions and logically lowered their peer estimates. These findings are especially intriguing because the literature on the curse of knowledge, so far, suggests that young children are routinely biased by their own knowledge. Instead, children demonstrated critical thinking skills when considering what their peers would know and were surprisingly adept at using the unfamiliarity of the information as a cue to guide their peer estimates.

I also investigated the relation between Manifestations 2 and 3 of the curse of knowledge. Specifically, I examined whether children's tendency to be biased by their knowledge is similar when judging the pervasiveness of knowledge and when judging the knowledge of a specific individual. I found some evidence that these manifestations of the bias are related. Particularly, in my master's thesis, I found that children who tended to show a bigger magnitude of bias in their peer estimates, also showed a bigger magnitude of bias when judging Ernie's ability to see

an object. However, I did not show this finding in Experiment 1, suggesting there may not be as much overlap between these manifestations in childhood as initially hypothesized. I acknowledge, however, that the current findings rest on correlational analyses of two very different methodologies; the VHB measure examines children's reasoning about knowledge acquired perceptually (visual recognition), whereas the Peer Estimates task examines their reasoning about conceptual knowledge. As such, I am cautious in making any strong claims about the similarities or differences between the two manifestations.

4.7.1 Summary

The current research suggests that while the curse of knowledge affects children's inferences of how widely known information is among peers, the bias does not occur anytime children learn new information. The bias is contingent on the familiarity of the information they learn. These findings provide the first evidence of fluency misattribution in children's ability to reason about what others know. I hope to encourage research examining the previously neglected role of fluency misattribution in children's social cognition. There is a wealth of research showing the effect of fluency misattribution on social and cognitive judgements in adulthood (see Claypool et al., 2015; Newman et al., 2014; Silva et al., 2017; see Unkelbach & Greifeneder, 2013), however such research with children is essentially non-existent. Given the important role that the bias plays in individuals' ability to communicate and understand others' perspectives, research aimed at better understanding the factors that contribute to the curse of knowledge and the ways to minimize it would be especially fruitful.

Chapter 5: General Discussion

The ability to infer what others know is critical in understanding other perspectives. It allows one to infer the intentions, thoughts, feelings and even the desires of others. To conceptualize this ability, it is beneficial to frame it as an umbrella term for a number of social skills, including the skills to infer another individual's knowledge based on age, gender, profession, prior experience, and so on. It also involves the ability to reason about another individual's knowledge state based on their certainty, accuracy and social identity. Children possess many of these skills from early on (see Poulin-Dubois & Brosseau-Liard, 2016), and they become more sophisticated in using these skills with age (Birch et al., 2020; Brosseau-Liard et al., 2014; Huh et al., 2019). For instance, when children are faced with conflicting information from two informants, they tend to use the characteristics of their informants to choose who to learn from. They prefer to learn from an informant who is an expert, who is more certain, who has access to information, and who is more familiar (e.g., Atance & Caza, 2018; Bergstra et al., 2018; Brosseau-Liard, 2017; Burdett et al., 2016).

The curse of knowledge influences the ability to infer what others know, in that it leads one to overestimate the likelihood that others would share their knowledge. Given the importance of the ability to infer others' knowledge, it is not surprising that this bias affects decision making across various contexts, including legal settings (see Giroux et al., 2016), medical domains (Arkes, 2013; Arkes & Schipani, 1994), economics (Bhattacharya & Jasper, 2018; Biais & Weber, 2009; Kirchler et al., 2002), and teaching (Dawson et al., 1988). The curse of knowledge affects one's social reasoning, even if they are explicitly warned about the bias and are motivated to overcome it (Camerer et al., 1989; Damen et al., 2018; Pohl & Hell, 1996). The curse of knowledge affects social reasoning across the lifespan (Bernstein et al., 2011; Pohl et

al., 2018). Importantly, however, older children and adults tend to be less vulnerable to the bias compared to younger children and older adults (Bernstein et al., 2011; Pohl et al., 2018).

Accordingly, there is tremendous value in examining the course of knowledge developmentally, to better understand the origins of this bias, the mechanisms behind the bias, as well as the relations between the course of knowledge and social and cognitive abilities across the lifespan.

My dissertation sheds light on these critical issues. I contribute to our understanding of the bias in early development in several ways. First, my dissertation provides evidence that the course of knowledge is a universal feature of our minds by showing that this bias affects non-Western children's social reasoning (Chapter 2). I found evidence of the course of knowledge in Turkana children's judgements, which suggests that this bias is *not* contingent on Western culture. Second, I found that 3-year-olds are just as good as 5 and 6-year-old children in their ability to infer false beliefs when they are not required to overcome the course of knowledge (Chapter 3). That is, the oft-replicated finding that younger children do not have the capacity to reason about false beliefs is at least partially explained by the age-related change in the course of knowledge. In Chapter 3, I put forth a new way to examine children's false belief reasoning without the interference of the course of knowledge. Lastly, I provide support for the fluency misattribution account of the course of knowledge by demonstrating that the bias only occurs for information that was familiar or fluent (Chapter 4). I also provide the first evidence of Manifestation 3 of the course of knowledge in childhood (i.e., the effect of the bias in children's estimates of what their peers know) and offer a new measure that is sensitive to individual as well as age-related differences, which may be especially useful for future research.

In this chapter, I begin by discussing fluency misattribution versus inhibitory control as mechanisms behind the course of knowledge. Next, I discuss the universality of the course of

knowledge and how it appears to be a byproduct of our human ability to learn. Lastly, I discuss the curse of knowledge on false belief reasoning among children, and how the most widely used measure of false belief reasoning may be confounded by the curse of knowledge. I also discuss the implications of this finding on the myriad of studies examining false belief reasoning and its correlates.

5.1 Mechanisms involved in the curse of knowledge.

Researchers put forth the inhibitory control account and the fluency misattribution account to explain the causal mechanism behind the curse of knowledge. The inhibitory control account was investigated among children and adults (e.g., Bernstein et al., 2011; Lagattuta et al., 2014), and this account suggests that the bias occurs when one is unable to fully suppress their own knowledge to reason about a naïve perspective (Bayen et al., 2007). The fluency misattribution account was investigated exclusively among adults, and this account suggests that the bias occurs when one misinterprets the ease (or fluency) in processing information as indicative of how apparent, or obvious, this information is (Bernstein et al., 2018; Birch et al., 2017; Harley et al., 2004). While the inhibitory control account places emphasis on *possessing* knowledge and becoming biased by that knowledge, the fluency misattribution account places emphasis on the fluency one feels while *processing* the information. The fluency misattribution account suggests that individuals would only show a curse of knowledge if information is fluently processed, and they would not show a curse of knowledge, or would even show a complete reversal of the bias, if the information is dysfluent. Conversely, the inhibitory control account of the curse of knowledge, as it has been described previously, suggests that individuals should become cursed anytime they possess knowledge.

In Chapter 4, I examined the effect of fluency misattribution on the curse of knowledge among children. I manipulated fluency misattribution through familiarity; the logic was that when information was familiar to children, they would be more likely to process it fluently, and when information was unfamiliar to children, they would be less likely to process it fluently (for similar work, see Begg et al., 1985; Jacoby et al., 1989; Fazio et al., 2019). Children only showed the curse of knowledge in their peer estimates when the information was familiar and did not show the curse of knowledge when the information was unfamiliar. In other words, fluency misattribution played a critical role in the occurrence of the bias, where children only showed the bias when it was possible to mistake the ease of processing the familiar information as indicative of how obvious that information is to their peers.

My findings are more in line with the fluency misattribution explanation of the curse of knowledge, compared to the inhibitory control account. Nonetheless, it is worth considering whether my findings in Chapter 4 could indeed be interpreted under an inhibitory control account or some modified version of it. For instance, familiar information is more salient than unfamiliar information (Awh et al., 2012), and therefore it is likely more difficult for children to inhibit familiar information, compared to unfamiliar information, when reasoning about other perspectives. It is important to note, however, that it could be the fluency of the information that makes it more salient or difficult to inhibit. Indeed, researchers found that the more fluent, or ‘automatic’, a cognitive process is, the more difficult it is to inhibit that process in favor of another process (Macleod & Dunbar, 1988).

It is often difficult to disentangle the effects of inhibitory control and fluency misattribution. For instance, Pohl and colleagues (2018) examined the effect of inhibitory control on the curse of knowledge by manipulating the availability of information (see also, Coolin et al.,

2016), nonetheless this manipulation may also affect the fluency of processing that information, which may lead to fluency misattribution. In their study, participants were given factual questions that required numerical responses (e.g., ‘how many months are elephants pregnant?’; Pohl et al., 2018). After providing their responses, participants were asked to complete distractor tasks, and then they were asked to recall their original responses to the questions. In one condition, they were not given the correct answers before recalling their original responses to the questions. In a low-inhibition condition, the researcher told the participants the correct answers (e.g., elephants are pregnant for *21 months*) before asking for their original responses. In the high-inhibition condition, the researcher told the participants the correct answers, before asking for their original responses, and the correct answer was displayed on a computer screen, as well as the sheet of paper where participants provided their original responses. The researchers found that participants exhibited a larger magnitude of the curse of knowledge in the high-inhibition condition, compared to the low-inhibition condition; that is, participants showed a bigger curse of knowledge effect when information was provided through 3 sources (i.e., the computer screen, the researcher, and the sheet of paper), compared to just one (i.e., the researcher).

When considering this study, it can be argued that researchers were inadvertently examining the effect of fluency misattribution on the curse of knowledge. That is, while increasing the availability of the correct answer can make it more difficult for participants to inhibit that information (i.e., high-inhibition condition), increasing the availability of the correct answer can *also* induce more fluency in processing the new information. Indeed, researchers have shown that repeated exposure to a stimuli leads to an increase in fluency misattribution (Birch et al., 2017; Janiszewski & Meyvis, 2001; Whittlesea et al., 1990). Accordingly, the finding that participants show a bigger curse of knowledge effect in the high-inhibition

condition, compared to the low-inhibition condition, may be because the high-inhibition condition leads to more fluency than the low-inhibition condition, which may in turn lead to more fluency misattribution.

In previous work, my colleagues and I aimed to disentangle the effects of fluency misattribution and inhibitory control on the bias by creating conditions where participants were asked to estimate how many of their peers would know the answers to a) factual questions that the participants knew the answers to, b) factual questions that the participants did not know the answers to c) factual questions that the participants were familiar with, but did not know the answers to (Birch et al., 2017). For example, in one experiment participants were taught the answers to factual questions and one week later they were asked for their peer estimates of those items as well as new items (i.e., Unknown Items). Their estimates of their peers' knowledge were compared across three types of items: Known Items, participants knew the answers; Unknown items, participants did not the answer, and Familiar items, participants were taught the answers to the factual questions one week prior, but they forgot the answers to those questions. We found that participants were cursed by their knowledge in the Known *and* Familiar items, suggesting that it is possible for individuals to be cursed by their knowledge without possessing knowledge. Indeed, the participants could not remember the answers for the factual questions, but the fluency associated with the factual questions was enough to produce the curse of knowledge. This finding raises challenges for an inhibitory control account because it is difficult to imagine how it could be hard to inhibit content that one cannot even recall.

In another experiment, my colleagues and I examined whether the curse of knowledge would occur even if participants never learned the answers to the factual questions at all (Birch et al., 2017). In Experiment 3, participants were presented with factual questions one week before

they were asked to make peer estimates, however the participants never learned the answers to those questions. We found that even when the participants never had any knowledge about the answers for the factual questions, they still showed a curse of knowledge based solely on the fluency associated with those questions. Accordingly, the inhibitory control account of the curse of knowledge was insufficient for explaining the occurrence of the bias, since the curse of knowledge affected individuals' peer estimates even when they did not possess any knowledge to inhibit.

Furthermore, the inhibitory control account does not adequately explain the reverse curse of knowledge effect, where upon learning certain dysfluent or surprising information, individuals deem it as being less obvious to others (Ghrear et al., 2021; Pezzo, 2003; Yopchick & Kim, 2012). That is, a 'more familiarity-more inhibition' variant of the inhibitory control account might explain why less familiar information will be easier to inhibit, but it is unclear how it would explain full reversals of the bias that occur from very unfamiliar or dysfluent information. The reversal of the bias seems to be more consistent with a fluency misattribution account, where individuals experience dysfluency in processing the information, and this dysfluency cues them to realize how obscure that information really is.

Accordingly, I argue against the inhibitory control account of the curse of knowledge. Given the evidence discussed above, I do not believe that inhibitory control is the causal mechanism underlying the curse of knowledge, as it does not fully explain the occurrence of the bias. I argue that fluency misattribution is the causal mechanism responsible for the occurrence of the curse of knowledge, and inhibitory control, as a cognitive skill, plays a moderating role on the effect of the bias. Inhibitory control can be deployed to attenuate the magnitude of the bias on our social reasoning (e.g., Coolin et al., 2015, 2016). That is, once fluency misattribution leads to

the curse of knowledge, inhibitory control can lessen the effect of the curse of knowledge.

Inhibitory control may either moderate the curse of knowledge by allowing one to suppress their own knowledge when reasoning about a naïve perspective, or it could allow one to suppress the feelings of fluency associated with the information.

For example, inhibitory control may lessen the effect of the curse of knowledge when an individual learns familiar information (e.g., the bat has the best hearing). The individual may mistake the fluency of processing this information as indicative of how obvious it is and become biased by their knowledge. In this case, if the individual has good inhibitory control, they stand a better chance of not becoming swayed by their knowledge, or the fluency associated with that knowledge. However, if the individual has weak inhibitory control, then they would likely show a greater curse of knowledge in their social reasoning. In contrast, if an individual learns unfamiliar information (e.g., the greater wax moth has the best hearing), then they would not experience fluency in processing the information, and they would not show the curse of knowledge, or they may even show the reverse of the bias. Accordingly, inhibitory control would not be required. Viewing inhibitory control as a moderating, rather than a causal, factor of the curse of knowledge, helps make sense of the evidence suggesting that the curse of knowledge is not contingent on the possession of knowledge, on the one hand, and it also explains the correlation between the curse of knowledge and inhibitory control (Coolin et al., 2015, 2016). Indeed, the moderating role of inhibitory control may also partially explain the age-related changes in the magnitude of the bias. The specific ways in which inhibitory control can be deployed to minimize the curse of knowledge (e.g., inhibiting knowledge versus inhibiting feelings of fluency) would be interesting avenues for future research.

Source memory also plays an important role in fluency misattribution, such that individuals who forget the source of their knowledge and fail to recognize the source of the feelings of fluency associated with that knowledge are likely to misattribute the fluency to an accurate source (Jacoby, 1989; Oppenheimer, 2008). Consistent with this, the curse of knowledge is more likely to occur when one has forgotten the source of their knowledge. For instance, in other work, my colleagues and I found that adults were more likely to overestimate how many others would share their knowledge when they were asked to make a peer estimate one week after learning new information, compared to when they were asked to make a peer estimate directly after learning the new information (Birch et al., 2017). That is, participants were more likely to show a curse of knowledge after they had some time to forget exactly how they learned that information. Consistent with this, research suggests that older children tend to be more accurate at recalling how they came to know certain information, compared to younger children (Earharts & Roberts, 2014; Gopnik & Graf, 1988; O'Neill & Gopnik, 1991), and this age-related change in source memory may partially account for the smaller effect of the curse of knowledge among older children compared to younger children.

The development of cognitive abilities, including inhibitory control and source memory, are important to consider when investigating the curse of knowledge across the lifespan (e.g., Birch & Bloom, 2003; Coolin et al., 2016; Lagattuta et al., 2010; Sommerville et al., 2013). We, as humans, develop the cognitive capacity to reduce the effect of the curse of knowledge in our social reasoning. The fluency misattribution account, on its own, may not adequately account for the age-related changes in the bias. One might even imagine that it would predict that older children and adults would show a bigger magnitude of bias compared to younger children, given that older children and adults have more experience in the world. Indeed, one may argue that

older children and adults are more likely to be familiar with different information, compared to younger children, and therefore they should process new information more fluently and show a bigger magnitude of the bias. To explain the finding that younger children show a bigger magnitude of bias compared to older children and adults, we must consider the role of other cognitive abilities (e.g., source memory and inhibitory control) in facilitating the ability to overcome the curse of knowledge. It is also important to consider children's maturing ability to make inferences about what others know. With age, children become more sophisticated in estimating what another individual would know based on their profession, experiences, accuracy and confidence (e.g., Brosseau-Liard et al., 2014; Brosseau-Liard, 2017; Brosseau-Liard & Poulin-Dubois, 2014). This maturing ability to accurately estimate what others would know may also help children adjust for the effect of the curse of knowledge when considering other perspectives.

One may wonder how the curse of knowledge fits with the 'self-simulation' view on social perspective taking (Gallese & Goldman, 1998). This view suggests that to understand other individuals' perspectives, we, as humans, tend to imagine how we would think, react, or feel, in a similar situation, and we would draw inferences about the mental states of others accordingly (Ball et al., 2013; Cannon et al., 2014). It is reasonable to suspect that in attempting to infer an individual's perspective, one may simulate their own knowledge and inaccurately infer that the individual also shares this knowledge. Tamir and Mitchell (2013) suggest that we tend to self-simulate when an individual is 'like us'. Specifically, if the individual shares similar group membership, attitudes or beliefs, we tend to self-simulate to reason about their perspective, however we do not self-simulate when an individual is *not* like us (Ames, 2004; Tamir & Mitchell, 2013). In other words, we tend to draw from our own experiences and mental states,

only when we identify with another individual (see Epley, 2008). Once we deem the individual as being like us, we use our mental state as a starting point (or ‘an anchor’) to infer their perspective, and then we adjust from that anchor to accommodate for individual differences (i.e., ‘anchor and adjustment’; Epley, 2008; Epley et al., 2004; Tamir & Mitchell, 2013; Tversky & Kahneman, 1974). For example, if I want to buy my friend a sweater for Christmas, I may begin by considering which sweaters I like, and then I would adjust my inference of what she would like based on her characteristics. I may realize, for instance, that while my favorite color is pink, her favorite color is mustard yellow, and accordingly I would adjust my choice of which sweater to buy.

This view on social perspective taking is different from the curse of knowledge in important ways. For one thing, self-simulation does not adequately account for the asymmetry in perspective taking that has been observed in curse of knowledge research (Birch & Bloom, 2003; Birch et al. 2017), in that it does not explain why we do not infer that others would share our *ignorance*. Birch and Bloom (2003) found that when children knew the contents of the boxes, they overestimated Percy’s knowledge (see Chapter 1, page 8 to 9), in contrast, when children did not know the contents of the boxes, they did *not* overestimate Percy’s ignorance which is inconsistent with a self-simulation view. Adults show the same asymmetry: individuals who know specific information overestimate what others know, however individuals who do not know the information are more accurate at inferring how many of their peers will know that information (Birch et al., 2017).

Furthermore, the self-simulation view on social perspective taking suggests that we only use our own mental states to make inferences about another individual’s perspective when that individual is like us (e.g., Epley, 2008; Stern & West, 2016; Tamir & Mitchell, 2013). However,

in a large-scale study, Farrar and Ostojic (2018; $n = 744$) found that adults are similarly biased by their knowledge whether they were considering the perspective of an in-group member, out-group member, or a dog. Indeed, the curse of knowledge does not seem to depend on whether we identify with another individual or not. This is not to dismiss the self-simulation view, as researchers have confirmed its utility in social perspective taking (e.g., Ball et al., 2013), but it appears that the curse of knowledge is a separate process. The curse of knowledge affects our social reasoning even if we are reasoning about the perspectives of individuals who are very different than ourselves.

It is still possible, nonetheless, that a form of anchoring and adjustment occurs when individuals are cursed by their knowledge. That is, it might be the case that when individuals are cursed, they tend to automatically anchor on their own knowledge of a given topic, and then they may be able to adjust away from that anchor and accommodate for how much another individual would know. For instance, Anne may unconsciously infer that her husband would share her knowledge about accounting, but as she considers how she came to learn about accounting (e.g., she has been an accountant for 8 years), and she considers her husband's knowledge of accounting (e.g., her husband did not study accounting), she may be able to adjust her inference about her husband's knowledge.

5.2 The curse of knowledge is universal.

Researchers consider the curse of knowledge to be a byproduct of the way we, as humans, learn new information (Henriksen & Kaplan, 2003; Hoffrage, Hertwig & Gigerenzer, 2000). That is, researchers suggest that the curse of knowledge stems from our innate ability to prioritize newly learned information over outdated information. To examine the universality of the curse of knowledge, I investigated the bias among Turkana children of Northwest Kenya,

who have a very different cultural and social upbringing compared to Western children. I found evidence of the bias among Turkana children, where Turkana children overestimated the likelihood that their peers would share their knowledge of specific facts.

However, only Turkana boys, and not Turkana girls, showed the curse of knowledge effect in their peer estimates. Of course, the type of task that is used to measure the curse of knowledge has a large bearing on how much of the bias participants exhibit in their social reasoning. After all, even though adults exhibit a curse of knowledge in their false belief reasoning using a modified task (e.g., Birch & Bloom, 2007), they have little difficulty with the classic Sally-Anne task. Therefore, it is likely that our specific test of the curse of knowledge was not sensitive enough to capture the curse of knowledge among Turkana girls. That is, I believe that the curse of knowledge affects Turkana girls' social reasoning, and the bias is universal, however I suspect the current study was not able to tap into the bias in their social reasoning. It is likely that if my colleagues and I increased the time interval between teaching children the answers to the factual questions and asking them to make their peer estimates, Turkana girls would have shown a curse of knowledge effect. Indeed, participants are more likely to become cursed by their knowledge when they are asked to make their peer estimates *one week* after learning new information, compared to when they are asked to make their peer estimates directly after learning new information (Birch et al., 2017). This would be an interesting avenue for future research investigating the curse of knowledge in non-Western children.

Nonetheless, our study seems to suggest that Turkana girls are less vulnerable to the curse of knowledge, compared to Turkana boys. This gender difference may stem from differences in Turkana boys' and girls' day to day lives. Turkana girls tend to be responsible for

more social tasks (e.g., looking after their siblings), while Turkana boys tend to be responsible for more solitary tasks (e.g., herding the animals). Turkana girls' tendency to spend more time in their communities, compared to Turkana boys, may afford them more opportunities to develop their social skills, including their capacity to accurately infer other individuals' knowledge. For instance, they may have more opportunities to witness how individuals learn different information (e.g., they may learn that the Turkana elders know about the conflicts within the community, because they tend to resolve those conflicts), and they may have a better understanding of expertise (e.g., they may learn that the Turkana boys have a better understanding of goats, because they spend a lot of time herding animals). Turkana girls tend to interact with many individuals, including their younger siblings, their elders, and their peers, and they may have had more opportunities to realize that not everyone shares their knowledge and to gain an appreciation for what others are likely to know.

It is also possible that through their social interactions with others, Turkana girls have more opportunities to practice and develop their source memory and other cognitive resources that allow them to be less susceptible to the bias. For example, Turkana girls may be better at overcoming the curse of knowledge, compared to Turkana boys, because they have more practice developing their executive function skills and source memory through their social interactions. Indeed, research suggests that individuals who have more social relations tend to have better cognitive abilities, including better performance on executive function tasks, memory tasks, and language ability (e.g., Gow & Mortensen, 2016; Mousavi-Nasab et al., 2012; Regina et al., 2011; Van Gelder et al., 2006). Interestingly, there is also evidence suggesting that females (in general) tend to have better inhibitory control and source memory, compared to males. Researchers reported gender differences in inhibitory control tasks that favor females (e.g.,

Carlson & Moses, 2001; Carlson & Wang, 2007), as well as gender differences in episodic memory (i.e., the ability to recall a past experience; see Tulving, 2002) that favor females (e.g., Bridge, 2006; Grysman & Hudson, 2013; Herlitz et al., 1997). Critically, episodic memory tasks also measure one's source memory, or the ability to recall how one came to know something (Johnson, 2005). It may be that Turkana children exhibit a stronger gender difference in inhibitory control and source memory, compared to Western children, and this might be because of the gender differences in Turkana children's daily social experiences, compared to Western children.

Furthermore, I did not find the commonly reported age-related decline in the curse of knowledge among Turkana children. That is, while it is widely reported that younger Western children show a bigger curse of knowledge effect compared to older Western children (e.g., Bernstein et al., 2011; Lagattuta et al., 2014), I did not find that age effect among Turkana children. One possibility for this difference is that Western children are more likely to regularly attend formal education, compared to Turkana children, where Western children are taught various topics that may help develop their critical thinking skills (e.g., math, science, history). To further examine this possibility, it may be interesting to investigate whether the age-related changes in the curse of knowledge exist in other populations that have low formal education rates. If it is true that the age-related change is influenced by the learning that occurs in formal education, then I would expect that children who do not have access to formal education will not demonstrate the same degree of age-related changes in the curse of knowledge.

Nonetheless, the finding showing that the curse of knowledge is evident among Turkana children suggests that the bias stems from a universal feature of the human mind. That is, while the magnitude of the bias may vary due to various social factors, it nonetheless exists at least to

some degree across individuals despite different cultural upbringings. Note, my findings do not speak to whether the bias stems from an evolutionary adaptive learning system, per se. They are however consistent with the argument that the curse of knowledge may be a by-product of the way humans learn new information. Indeed, researchers suggest that the curse of knowledge is a by-product of our minds' ability to prioritize recently learned information over old and outdated information (e.g., Haselton et al., 2009; Henriksen & Kaplan, 2003; Hoffrage et al., 2000). Our minds typically integrate new information so efficiently, and fluently, that it becomes difficult to step away from that information. Of course, not *all* learning is fluent leading to variations in the degree to which this fluency contributes to the curse of knowledge.

We, as humans, live in environments that are constantly changing, and it is critical for our survival to keep up with new information about our environment (e.g., it may help us avoid a predator, track food, find shelter, etc.). Put this way, the curse of knowledge seems like a small price to pay for our ability to learn new information and keep track of our environment. Moreover, we begin acquiring the cognitive resources (e.g., source memory, inhibitory control) that can help minimize the curse of knowledge early in development. Indeed, given its current existence, the curse of knowledge was not sufficiently disruptive to our survival to be weeded out by evolution. Consider as well that even though the curse of knowledge leads to a robust effect on our social judgments, it does not completely negate our ability to gauge what others know—it only sways our estimates in a particular direction. For example, in one of our experiments with adults, we found that participants estimated that 8.6% more of their peers would know the answers to questions they were taught compared to questions they were not taught (Experiment 1, Birch et al., 2017). That is, they did not assume that *everyone* would know what they knew, but rather they showed a small and robust bias in their peer estimates. The

magnitude of the bias also varies depending on the situation or the nature of the research design (e.g. how much time has passed since acquiring the information; see Christensen-Szalanski & Willham, 1991). Moreover, the consequences of the bias will depend on the situation. For example, you may confuse your friend if you overestimate how much they know about the current news, but the consequences could be greater if a judge overestimates how much information was available to a physician when ruling on a case of medical negligence.

5.3 The implications of the curse of knowledge bias for children's false belief reasoning

I examined the effect of the curse of knowledge on false belief reasoning. I found that younger children were more likely to accurately infer a false belief when they were not required to overcome the curse of knowledge (Chapter 3, Experiment 1). While other research has hinted at the effect of outcome knowledge on false belief reasoning among children (e.g., Call & Tomasello; Koos et al., 1997; Setoh et al., 2016), my research in Chapter 3 was the first to directly examine the effect of the curse of knowledge on children's false belief reasoning with identical tasks that only varied based on whether outcome information was presented or not. As such, my work contributes a new measure that would allow researchers to specifically examine false belief reasoning rather than the ability to overcome the curse of knowledge. Importantly, this measure can capture younger children's ability to reason about false beliefs, as well as older children. In addition, using both versions of the false belief task (Outcome Known vs Outcome Unknown) will allow researchers to specifically address whether, and which, children struggle with overcoming the curse of knowledge.

In previous work, researchers linked children's false belief reasoning with inhibitory control. Across many studies, researchers reported that children's performance on inhibitory control tasks, where they were required to suppress a prepotent response, was correlated with

their performance on the classic Sally-Anne task (e.g., Carlson et al., 2004; Chasiotis et al., 2006; Mutter et al. 2006); such that children who were better at suppressing dominant but irrelevant responses were also more accurate at reasoning about false beliefs. Consistent with this, researchers suggest that to reason about the mental states of others, children must be able to suppress their prepotent tendency to reference reality; that is, to suppress the dominant response to indicate that Sally will look for the object where it was moved (Carlson et al., 1998; Carlson & Moses, 2001). While inhibitory control plays an important role in social competence (e.g., Bailey & Henry, 2008; Ciairano et al., 2007; Rhoades et al., 2009; Thorell et al., 2004), false belief reasoning does not *always* require that one inhibit their own knowledge to reason about another perspective. As discussed in Chapter 3, one can reason about false beliefs without knowing the specific outcome of an event (e.g., I may know that Sally has a false belief about the location of her ball, without knowing where Ryan moved the ball). Indeed, the reported association between performance on the Sally-Anne task and inhibitory control tasks may be accounted for by children's ability to overcome the curse of knowledge (Bailey & Henry, 2008; Bernstein et al., 2007; Fizke et al., 2014). In other words, the association between the Sally-Anne task and inhibitory control measure may be inadvertently capturing the relation between overcoming the curse of knowledge and inhibitory control. To examine this view, future research can investigate whether children's performance on inhibitory control tasks is correlated with their performance on the false belief tasks in Chapter 3. If children's performance on the inhibitory control task is only correlated with their performance on the cursed trials (i.e., Outcome Known trials) and not the non-cursed trials (i.e., Outcome Unknown trials), then there is reason to believe that the previously reported association between performance on the inhibitory control tasks and the Sally-Anne task is explained by the curse of knowledge.

Furthermore, the age-related improvement on children's performance on the Sally-Anne task seems to stem from a developing ability to overcome the curse of knowledge, rather than a developmental change in children's conception of the mind. That is, the developmental period between 3 and 5 years of age may reflect the development in cognitive skills (e.g., inhibitory control, source memory) that facilitate one's ability to overcome the curse of knowledge. Importantly, researchers have already demonstrated that children's ability to pass the Sally-Anne task may have more to do with the cognitive demands associated with the task rather than the ability to reason about false beliefs (e.g., Devine & Hughes, 2014; He et al., 2012; Setoh et al., 2016). My findings in Chapter 3 suggest that young children experience a difficulty with perspective-taking, but this difficulty does not stem from an inability to infer false beliefs, rather this difficulty stems from a greater susceptibility to the curse of knowledge.

Indeed, the Sally-Anne task may not be measuring what it was intended to measure. This finding echoes a myriad of studies suggesting that the Sally-Anne task *underestimates* young children's ability to reason about false belief, leading some to suggest the task should be abandoned (e.g., Bloom & German, 2000). Abandoning the task completely would be a mistake because performance on the task has proven to be a reliable predictor of an abundance of critical social outcomes (e.g., Capage & Watson, 2001; De Rosnay et al., 2014; Peterson et al., 2016). Instead of abandoning the task, it would be beneficial to revise the task to minimize the curse of knowledge and compare performance in false belief reasoning that does versus does not involve overcoming the curse of knowledge. For instance, if the purpose is to understand the relation between false belief reasoning and social competence, then one would examine children's false belief reasoning in a non-cursed false belief task (e.g., Outcome Unknown trial). However, if the purpose is to understand how the ability to overcome the curse of knowledge relates to social

competence, then one would examine children's reasoning in a cursed task (e.g., Outcome Known trial, or the Peer Estimates task).

Similarly, the previously reported predictors of children's performance on the Sally-Anne task and other similarly cursed false belief tasks, such as family demographics (e.g., Farhadian et al., 2010), and parents' tendency to use mental state words (e.g., Symons et al., 2005), may be predictors of children's broader perspective-taking ability (i.e. the ability to overcome the curse of knowledge), rather than predictors of the ability to reason about false beliefs, per se. Applying the same logic, the various studies that reported an association between false belief reasoning and social competence (e.g., bullying, communication, prosocial behavior, peer relationships, social-emotional well-being; e.g., Razza & Blair, 2009; Walker, 2010), may actually be measuring the relation between the ability to overcome the bias and social competence. Even research examining the neural underpinnings of false belief reasoning (e.g., Meinhardt et al., 2011; Samson et al., 2004) may be demonstrating the underpinnings of the ability to overcome the curse of knowledge. Importantly, my results stress the need to revisit and reinterpret substantive bodies of literature that have relied exclusively on the standard ('cursed') Sally-Anne task.

5.4 Other Implications and Future Directions

One may wonder how fluency misattribution would affect the occurrence of the curse of knowledge among Turkana children. Would Turkana children become biased by their knowledge if they were taught dysfluent or unfamiliar information? Although this question would require empirical investigation, I hypothesize that Turkana children, like the North American children in Chapter 4, would not show the curse of knowledge if they were taught unfamiliar information. In Chapter 2, Turkana children were taught answers to facts about familiar items (e.g., how many

Turkana towns are there?), the fluency of processing this information may have been mistaken for how widely known the information is among their peers. However, I would not expect Turkana children to show the curse of knowledge if they were taught unfamiliar information (e.g., how many cities are there in British Columbia?). In other words, given that the bias appears to be universal and fluency misattribution appears to be the causal mechanism behind the bias, then the fluency misattribution account of the bias should hold cross-culturally.

Consistent with this, I found that some of the questions presented to Turkana children did not produce a curse of knowledge effect. Specifically, when Turkana children were presented with some of the answers to Set B questions, they did not show the curse of knowledge effect in their social judgements. Even though I did not expect this result, I suspect that those questions did not lead to the curse of knowledge effect because of the unfamiliarity of the answers (e.g., how do we make oil/petrol for cars?). I theorized that children did not process the new information as fluently as those questions in Set A (e.g., how long does a snake live?) and therefore did not become biased by their knowledge (and even decreased their estimates of how many of their peers would know the answer compared to those who were not taught the answer to those questions).

One may also wonder how the fluency misattribution account fits with my findings in Chapter 3; that is, how would fluency misattribution affect the curse of knowledge in children's false belief reasoning? Again, this question requires future investigation, however I suspect that children showed the curse of knowledge in their false belief reasoning because the outcome information was fluently processed and made sense to children. Indeed, there was nothing about the outcome information that created dysfluency. In the false belief scenario, children learned that Ryan moved the ball from the purple box to the red box, which seems like a plausible

outcome. Children likely integrated this new information fluently, as they do with most learning, and they mistook this fluency as indicative of how obvious this outcome was to Sally. In contrast, I suspect that if children were told that a witch cast a spell and teleported the ball from the purple box into the cupboard, children would be less likely to think that Sally would share this knowledge. That is, this outcome would seem less plausible and it would disrupt the fluency with which it would be integrated into children's knowledge, which in turn would reduce or eliminate the curse of knowledge. Some preliminary findings in our laboratory support the fluency misattribution account in children's false belief reasoning. In an unpublished study, children were shown three boxes and they watched a protagonist hide a small ball in one of the three boxes and leave the scene (Birch & Akmal, 2010). In the protagonist's absence, children either witnessed the researcher move the ball into another box (plausible outcome), or they witnessed the researcher put the ball in her own coffee mug (implausible outcome). Children were less biased by their knowledge when they witnessed the implausible outcome versus the plausible outcome. That is, they were unlikely to infer that the protagonist would share their knowledge of the implausible outcome and look in the researcher's coffee mug for the ball.

Furthermore, my dissertation examined Manifestation 3 of the curse of knowledge among children, which has not been previously examined in the literature. Chapter 2 and 4 offer the first evidence of the curse of knowledge in children's estimates of how widely known certain information was among peers. In Chapter 4, I examined the association between Manifestations 2 and 3 of the curse of knowledge; that is, I examined whether there was an association between the magnitude of the bias in the ability to infer another individual's perspective, and the magnitude of the bias in the ability to estimate how widely known that information was. To do this, I investigated children's performance on the Peer Estimates task and the Visual Hindsight

Bias measure (VHB measure; see page 8). Although I found a significant correlation between the two tasks in my master's thesis, I did not find a significant correlation in Experiment 1, Chapter 4. This might suggest that the relation between the two tasks is not reliable, which may make sense considering the difference in methodology used in the Peer Estimates task versus the VHB measure. The VHB measure examines children's reasoning about knowledge acquired perceptually (visual recognition), whereas the Peer Estimates task examines their reasoning about conceptual knowledge. Accordingly, I cannot draw any definitive conclusions about the relation between the two manifestations in childhood. Future work is needed to elucidate this relation. One way to address this question is by presenting children with the same information for both types of estimates; that is, asking them how many of their peers would know the answer, and also asking them whether a specific individual would know the answer. If the two estimates are correlated, after controlling for age, then there would be reason to believe that the two manifestations of the bias are correlated in childhood.

Indeed, research examining the relation between the two manifestations of the bias, especially across early development, might be very interesting. There may be reason to believe that the ability to reason about the perspective of an individual and the perspectives of a group enlist different reasoning abilities, and these abilities may follow different developmental trajectories. Often when one is making inferences about a specific individual's mental state, they have some knowledge about characteristics of the person that can guide inferences about what he or she knows (e.g., their age, profession, interests), as well as contextual information that can guide inferences (e.g., whether the individual was present when a given event unfolded). In comparison, making inferences about how widespread information is among a group of people involves more abstract reasoning. Reasoning about the likelihood that Jane knows X is quite

different from reasoning about the proportion of one's peer group who will likely know X, given that a group, by its nature, is composed of multiple individuals whose characteristics and contextual experiences can vary in many ways.

It is possible that the way we adjust our inferences about an individual's perspective is quite different from the way we adjust our inferences when reasoning about how widely known information is among peers. For example, if I was talking about the effect of bilingualism on executive functioning with Jane, who is a graduate student in chemistry, it would be obvious to me that she does not share my knowledge about psychology, and therefore I may be cued to adjust my inferences about what she knows about the topic. However, if Jane was a graduate student in psychology, then I may infer that she shares some of my knowledge in psychology, and therefore I may not be as inclined to adjust my inferences about what she knows.

Accordingly, individual characteristics can offer clues about what an individual knows, which can guide one's tendency to adjust their inferences about others' knowledge. In contrast, reasoning about the knowledge of one's peer group may not provide such clear cues about the likelihood that others would share their knowledge.

An additional contribution of my dissertation is that I provide a new measure, the Peer Estimates task, to examine the curse of knowledge in children's estimates of how widely known certain information is among peers. The Peer Estimates task measures individual, and age-related, differences in children's judgements of how widely known information is. This assessment can be used with a variety of questions for further research investigating the relations between the magnitude of the curse of knowledge and various individual differences such as motives, intelligence, and personality. For instance, research among adults confirmed an association between individuals' self-presentation needs (i.e., the motivation to appear favorably

to others; e.g., Campbell & Tesser, 1983) and the magnitude of the curse of knowledge, such that individuals who had a higher tendency to present themselves favorably were more likely to show the curse of knowledge across Manifestations 1 and 2 (see Musch & Wagner, 2007). Researchers also found that the curse of knowledge is negatively correlated with intelligence among adults (Stanovich & West, 1998), such that individuals who were more intelligent were more likely to consider opposing views to their understanding of a topic. Also, research suggests that anxiety leads to a greater curse of knowledge bias in adults' inferences about the spatial perspective of another individual (Todd et al., 2015). Additionally, research suggests that adults who are high in narcissism tend to show a bigger bias for outcomes that they themselves were able to predict (Howes et al., 2020). Future research can examine whether these same relations exist in childhood using the Peer Estimates task, to gain a better understanding of how emotions, intelligence, motives, and other individual differences affect the curse of knowledge developmentally.

In sum, my dissertation shed light on the effect of the curse of knowledge on social perspective taking in childhood. My findings suggest that the curse of knowledge is a universal phenomenon that impacts non-Western children's social reasoning. My research also demonstrates the effect of the curse of knowledge on children's false belief reasoning. In addition, it suggests that the curse of knowledge is contingent on fluency misattribution, such that children only show the curse of knowledge when information is processed fluently. My dissertation paves the way for many research avenues, and it provides new measures to better understand the effect of the curse of knowledge on our social cognition. I hope that through a better understanding of the curse of knowledge and its effects on our social abilities, future

research can discover ways to reduce the bias, and in turn improve individuals' ability to understand one another.

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