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ANIMACY'S EFFECT ON ATTENTION AND MEMORY IN HUMANS AND MACAQUES (*MACACA MULATTA*)

by

JENNIFER M. JOHNSON

Under the Direction of David A. Washburn PhD

ABSTRACT

Understanding what stimuli are naturally salient is important not only for future research designs, but also for understanding underlying cognitive mechanisms that influence memory and attention across species. Previous research has shown that humans show preferential processing of animate over inanimate stimuli. Specifically, humans remember animate stimuli better than inanimate stimuli, and animate stimuli capture attention more quickly than inanimate stimuli (Nairne, Thompson, & Paneirada, 2007; New, Cosmides, & Tooby, 2007). One hypothesis is that this occurs due to evolutionary adapted mechanisms: our ancestors needed to monitor living things more closely because those things are most important to survival. However, few researchers have looked into the animacy effect using picture stimuli, and there has been no research on the animacy effect with nonhuman animals. The current study examined whether this adaptation theory holds true for humans and macaques using novel picture-based tasks. Neither antisaccade or symbolic match-to-sample tasks showed a significant difference in attention to or memory for animate versus inanimate stimuli with human participants, suggesting that the animacy effect does not hold true for picture stimuli. Additionally, macaques only exhibited a significant difference between stimulus types in the memory task, casting doubt on the theory that attention to animacy may be an evolutionary adapted mechanism, at least for static stimuli.

INDEX WORDS: Animacy, Evolution, Attention, Memory, Macaque monkeys

# ANIMACY'S EFFECT ON ATTENTION AND MEMORY IN HUMANS AND MACAQUES (*MACACA MULATTA)*

by

JENNIFER M. JOHNSON

A Thesis Submitted in Partial Fulfillment of the Requirements for the Degree of

Master of Arts

in the College of Arts and Sciences

Georgia State University

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2019

ANIMACY'S EFFECT ON ATTENTION AND MEMORY IN HUMANS AND MACAQUES (*MACACA MULATTA*)

by

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May 2019

# DEDICATION

To my parents, William, and Rigby. Thank you for your unconditional love and encouragement.

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I would like to acknowledge my committee, IDEA lab colleagues, and LRC family. My thesis would not exist without your guidance, valuable input, and support.

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# 1 INTRODUCTION

The goal for the evolutionary psychologist is to reconstruct adaptive problems faced by our ancestors and to uncover the psychological mechanisms that evolved to solve these problems (Cosmides & Tooby, 1992; Laland & Brown, 2011). Evolutionary psychologists argue that our behaviors, biases, and thoughts can provide insight in to our ancestral past (Cosmides & Tooby, 1992; Klein, Cosmides, Tooby, & Chance 2002). They reason that cognitive processes, such as attention and memory, were shaped by natural selection in the same way biological processes were (Klein et al., 2002). Whereas most memory research, for example, looks at what memory mechanisms can and cannot do and under what conditions those systems are engaged, adaptive cognition research has taken a step back to the functionalist approach of the early 1900s to examine how and what evolutionary value is provided by different aspects of cognition. Scientists within this tradition, including evolutionary psychologists and many cognitive neuroscientists, ask what kind of problems various cognitive processes were designed or developed to solve.

Tooby and Cosmides (1989) outlined several steps that evolutionary psychologists needed to take to develop informed theories. The first four steps involve developing models of adaptive problems that our ancestors had to solve and then hypothesizing how these problems could have molded the human mind, all before collecting data. Unfortunately, even Tooby and Cosmides (2005) acknowledged that many evolutionary researchers skip these steps and instead fit a hypothesis to existing data. Specifically, these ‘Just so’ stories arise when researchers form *post hoc* narratives about observed cognitive and behavioral phenomena. Teleological hypotheses plague evolutionary psychology due to their lack of testability and sensational quality (Laland & Brown, 2011). These retrospective hypotheses are not falsifiable because it is

impossible to observe the direct evolutionary path of memory (Laland & Brown, 2011; Nairne, Pandeirada, & Thompson, 2008).

Many evolutionary psychologists attempt to trace our cognitive evolution to our Stone Age ancestors from the Pleistocene era on the African plains, referred to as the environment of evolutionary adaptedness (EEA; Tooby & Cosmides, 1989), but little is actually known about the problems faced by our ancestors at this time. Additionally, it has been argued that environments varied widely across populations during the Pleistocene era making it incorrect to claim that all 'hunter-gatherers' faced similar problems (e.g., Laland & Brown, 2011). Whereas this view of the EEA is not agreed upon by all evolutionary psychologists, and some researchers have adapted their view to include the variability of the era (Tooby & Cosmides, 1990), the stereotypical idea of our ancestors as generic hunter-gatherers on the African plains continues to be used as a foundation for hypotheses and undisciplined conjectures (Laland & Brown, 2011).

Nairne, Thompson, and Paneirada (2007) developed a testable, evolutionarily-driven hypothesis without relying on post hoc speculations. They noted that many strategies that enhance retention have been identified and thoroughly studied, such as the method of loci or elaboration, but few had looked at the functional value of these strategies (e.g. Blunt, 2015; Willoughby et al., 1997). Nairne and colleagues hypothesized that humans should be sensitive to the fitness-relevance of the to-be-remembered information and consequently should remember information better when processed in relation to survival relevance. From this *a priori* hypothesis, Nairne and collaborators used an incidental learning task where participants rated words for their usefulness to survival if they were stranded in a foreign grassland, usefulness to moving to a foreign land, or overall pleasantness. The results supported the hypothesis by showing that participants' retention was better for those who processed the words based on

survival in both free recall and recognition tasks. Nairne et al. (2008) replicated the findings of Nairne et al. (2007), and included other processing conditions: Imagery, self-reference, generation, and intentional learning. Participants in the survival processing condition remembered significantly more words than participants in all other conditions. Subsequent research supported the hypothesis for the survival processing effect across various contexts and methodologies (e.g., Aslan & Bauml, 2008; Nairne & Pandeirada, 2010, 2011; Nairne et al., 2008; Nouchi, 2012).

However, many researchers have found various proximate mechanisms that mediate the survival processing effect (e.g., Howe and Derbish, 2010; Kroneisen & Erdfelder, 2011; Nouchi, 2013; Savine, Scullin, & Roediger, 2011; Tse & Altarriba, 2010). Although these proximate mechanisms do not falsify the survival processing hypothesis, evolutionary psychologists seem to be going back to 'Just so' stories to explain away the differences in results. For example, Howe and Derbish (2010) argued that the survival processing effect is not due to some specific adaptive mechanism that enhances memory in survival-related scenarios, but instead, that domain-general processes, such as elaboration strategies, lead to enhanced retention of novel and unique situations like those used in the typical survival-effect task. Across multiple experiments, Howe and Derbish demonstrated that elaboration strategies accounted for the differences in memory of survival-related scenarios versus other neutral scenarios. Using a memory load paradigm, Nouchi (2013) also showed that the survival processing effect can be accounted for by elaboration processes. However, Nairne and Pandeirada (2016) argued that the fact that domain-general processes like elaboration or richness-of-encoding are involved in enhancing survival-processing is irrelevant to whether the survival processing advantage is an adapted process, because processing based on survival relevance must have been so important that it needed to co-

opt these basic but general processes. Similarly, they argued that any evidence of boundary conditions for the survival-processing effect do not weaken their argument. These arguments result in roundabout discussions about which came first: domain general processes that can result in enhanced survival processing, or enhanced survival processing that draws from domain general processes.

Despite the survival-processing effect starting out as a testable, *a priori* hypothesis that avoided 'Just so' criticisms, attempts to downplay all domain-general processes that have been demonstrated to underlie the effect and several boundary conditions have resulted in a general loss of interest. Additionally, Nairne (2014) suggested that "survival" may be too general of a concept to have steered selection, and instead hypothesized that we evolved unique mechanisms to deal with specific adaptive problems. Because of this, Nairne and other researchers developed more specified hypotheses that focused on distinct adaptive dimensions (Nairne, 2014; Nairne et al., 2013; New, Cosmides, & Tooby, 2007; VanArsdall, Nairne, Pandeirada, & Blunt, 2013).

### 1.1 Animacy Effect

The importance of the animate-inanimate distinction has been widely studied across disciplines. Animate entities (hereafter *animates*), as defined by Gelman and Spelke (1981), include biological structures that can initiate action, grow and reproduce, know, learn and think, and are biological structures. Although Gelman and Spelke (1981) state that animates are primarily people, in general, animate versus inanimate is the difference between living and nonliving things, or even more simply, the distinction between animals and nonanimals. In developmental research, the distinction between living and nonliving has been shown to arise early in infancy (Gelman & Spelke, 1981; Opfer & Gelman, 2010). Research has shown that animacy is an important factor in learning early in life (see summary in Opfer & Gelman, 2011).

Specifically, attention to animacy enhances children's abilities to learn new nouns, and children have early developing expectations about animates' role in agency in various events, affecting their sentence comprehension and understanding of syntax (Branigan, Pickering, & Tanaka, 2008; Childers & Echols, 2004). Additionally, the neuroscience literature shows that different brain regions are associated with processing animate entities compared to inanimate entities (e.g., Caramazza & Shelton, 1998; Grossman et al., 2002; Morito, Tanabe, Kochiyama, & Sadato, 2009; Wiggett, Pritchard, & Downing, 2009). For example, Wiggett et al. (2009) found ventrolateral visual brain regions were activated when processing a variety of different animate stimuli whereas ventromedial visual brain regions were activated when processing a variety of inanimate stimuli across various kinds of tasks.

New et al. (2007) hypothesized that animate entities would have been more important than inanimate entities to detect and monitor because they could be predators, prey, or mates, or could provide social information. Specifically, New et al. predicted that changes would be detected faster and with greater accuracy in a change-detection task to animates than inanimates because of the ancestral need for the former to be actively monitored. In a series of studies using a change-detection paradigm, New et al. reported that animates were detected more quickly and more accurately, and they reported that this was not due to differences in low-level features of those stimuli. Further, animate stimuli were found not to be judged as more interesting than inanimate stimuli, ruling this out as a factor in allocated attention. It is interesting that the research also indicated that the animacy advantage does not stem from experience with motion, by using static pictures of vehicles as a control category, due to the significance of vehicles in the modern world.. Changes to vehicles were detected faster than changes to naturally static inanimate stimuli, but changes to animates were detected significantly faster than both. New et

al. concluded that human attention was adapted to prioritize animates because they were an ancestrally important category. This bias has been labelled the “animate-monitoring hypothesis” (New et al., 2007).

After New et al.’s (2007) seminal findings on the effect of animacy on change detection, several studies have supported the hypothesis that animate stimuli are given priority in visual attention tasks. Altman et al. (2016) replicated the findings of New et al. (2007), but also found that unchanging animate stimuli distract from detecting inanimate changes to the scene. Altman and colleagues hypothesized that participants prioritized attention to animate items in the scene, even when the animate stimulus was not the target, interfering with detection of changing inanimate targets. Additionally, change detection response time to inanimate changes was positively correlated with the distance between the inanimate target and the unchanged animate, further supporting the idea that attention is allocated to animates and thus they serve as a distractor when they are not the target. Jackson and Calvillo (2013) found that animate stimuli were processed more efficiently than inanimate stimuli, and animacy status of stimuli reduced the detrimental effects of high perceptual load in a visual search task. Additionally, the animate monitoring hypothesis has been supported in attentional blink tasks (Guerrero & Calvillo, 2016), and inattentional blindness tasks (Calvillo and Hawkins, 2016; Calvillo & Jackson, 2014). Importantly, the studies that include threat as a variable found the effect regardless of the threat-level of the stimuli (Guerrero & Calvillo, 2016; New et al., 2007). Threat-levels are known to have an important attention advantage over non-threatening objects (Fox, Griggs, & Mouchlianitis, 2007), so it is important that attention biases for animate entities have been differentiated from advantages associated with threatening objects (Guerrero & Calvillo, 2016; New et al., 2007).

VanArsdall et al. (2013) drew from the animate-monitoring hypothesis to create a series of predictions about how animacy would affect memory. They hypothesized that animate entities would be better remembered than inanimates. VanArsdall et al. designed a paired-association task that paired pronounceable non-words (e.g., “FRAV”) with animate or inanimate properties (e.g., “made of wood”) to avoid item selection flaws typically associated with word choice. This allowed participants to process and to remember the same stimuli, but with the stimuli processed as animates by one group of participants and as inanimates by the other group. VanArsdall et al. employed a recognition task (Experiment 1) and a recall task (Experiment 2) and found that animates were remembered better than inanimates on both tasks. However, the animate characteristics in these experiments only corresponded with uniquely human qualities (e.g., “believes in God”). Therefore, the animate non-word pairs consisted of a more cohesive group of attributes than did the inanimate pairs.

Given the nature of their stimuli, VanArsdall et al. (2013) could not conclude whether it was animacy versus ‘human-characteristic’ that resulted in improved memory. Aslan and John (2016) tested this question by replicating the study of VanArsdall et al. with children ranging in ages 4 to 11 years. Unlike VanArsdall et al., Aslan and John included stimulus characteristics that were uniquely human, as well as some that represented animals. The size of the animacy effect was equivalent across the three age groups and there was no difference between the advantage of human versus animal attributes in any group. Aslan and John posited that animacy is salient early in life and that experience does not determine the animacy effect, because if it did, human attributes would show an advantage over animal attributes.

Nairne, VanArsdall, Pandeirada, Cogdill, and LeBreton (2013) further explored the mnemonic value of animacy by reanalyzing Rubin and Friendly’s (1986) predictor variables of

cued recall adding animacy to the list of potential predictor variables. They found that overall animacy was one of the highest predictors of cued recall, matching the predictor strength of imagery, which was the highest rated variable from the Rubin and Friendly study. Additionally, Nairne et al. directly tested animacy as a predictor variable by comparing participants' recall of animate versus inanimate words in an intentional encoding task. Words were matched in age of acquisition, category size, category typicality, concreteness, familiarity, imagery, Kucera-Francis written frequency, meaning, number of letters, and relatedness. Again, they found that participants were significantly more likely to recall animate over inanimate words.

Bonin, Gelin, and Bugaiska (2014) replicated Nairne et al.'s (2013) experiment using an incidental instead of intentional encoding task. Participants were asked to categorize the words as either animate or inanimate and were then given a surprise recall task. Not only were animates better recalled than inanimates, but animates were also categorized more quickly than inanimates during the incidental learning phase, suggesting that processing time did not play a role in the animacy advantage. Additionally, Bonin et al. found the same effect using pictures with the word label written underneath. In their third experiment, Bonin et al. looked at the animacy effect using a word-recognition task. Within this experiment, participants could rate words as either *remember* (consciously remember seeing this item) or *know* (when they recognize an item based on familiarity but cannot consciously remember seeing it). Again, the results showed that animate words were categorized faster, more animate words were recognized, and animacy did not have a significant effect on false alarms. Additionally, more *remember* responses (versus *know*) were given to animate words, indicating that participants had an enhanced quality of memory for the animate items. Finally, Bonin et al. tested whether richness, as determined by a sensory experience rating (SER), drove the animacy advantage, and observed no difference in

SER ratings for animate vs. inanimate words. The authors concluded that the animacy effect cannot be ascribed to differences in richness of the sensory or perceptual features of animate versus inanimate entities.

VanArsdall et al. (2015) explored the animacy effect in paired-association learning, in a context of foreign-language learning, by testing whether it is easier to learn animate foreign-language vocabulary than inanimate. In Experiment 1, participants learned 24 Swahili-English word pairs (taken from Nairne et al., 2013), then were tested (after a distractor task) using the Swahili word as the cue. They repeated this procedure 3 times. The results showed a significant main effect of word type and the animacy advantage remained constant across trials. To rule out the interpretation that animate concepts were more accessible overall, Experiment 2 assessed whether using equally constrained categories for animate and inanimate kinds would affect the results: four-footed animals versus household furniture. Experiment 2 replicated the procedure of Experiment 1 and confirmed a significant advantage of animate word-pairs, even with this category control. Additionally, more inanimate responses were incorrect.

Memory for spatial and temporal context is also aided by the animacy effect. Gelin, Bonin, Méot, and Bugaiska (2017) investigated whether participants would remember the spatial location or temporal location of a previously presented word better if the word had been animate. In Study 1, participants were presented with words in one of four locations on a screen and told to categorize the word as animate or inanimate. In a surprise recognition test, participants decided whether they had seen a word before and if they had, they selected where the word had appeared on the screen during the categorization task. Study 2 used the same methods, but instead of manipulating spatial location, each word in the categorization phase appeared with text indicating whether it was presented early, middle, or late in the series of words. During the

surprise recognition test, participants needed to remember the temporal context of recognized words. In both studies, participants categorized animates faster in the categorization phase, they recognized more animates, and they had a better memory for the contextual information of the animate words. False alarm rates were not different between the two groups and an index of bias analysis showed no difference in bias between animate and inanimate, suggesting attention was not preferentially drawn towards animates. Overall, the results illustrated that memory for contextual information was enhanced when it was related to animacy, even when animates were processed more quickly and learning was incidental.

These early experiments testing the robustness and replicability of the animacy effect showed that animacy enhances memory in free recall and cued recall, including paired-association tasks, it appears to aid memory for spatial and temporal contexts, and has been found in both word and picture tasks, and with both adults and children (Aslan & John, 2016; Bonin et al., 2014; Gelin et al., 2017; Nairne et al., 2013; VanArsdall et al., 2013, 2015). These studies have also established that animates are processed more quickly, determined by categorization or reaction time, suggesting that the animacy advantage is not a byproduct of processing time. Other studies have looked directly into possible variables that underlie the animacy effect. For example, VanArsdall et al. (2017) elaborated on VanArsdall et al. (2015) by testing the idea that the animacy effect may be due to more efficient category retrieval cues stemming from animates potentially being a more cohesive category with more diagnostic features than inanimates. VanArsdall et al. (2017) used an embedded list task to mask any category formation by participants and still found that animates were remembered better than inanimates. Similarly, Gelin et al. (2015) used various instructions at encoding to rule out the idea that the animacy advantage is dependent on encoding instructions that prompt the effect. Bonin et al. (2015)

investigated the role of different forms of deep processing, including elaboration and richness-of-encoding, by adding a concurrent memory load task during initial processing of the words, which is thought to block the use of elaboration strategies (Nouchi, 2013). In a different experiment, Bonin et al. instructed participants to imagine themselves interacting with each word to examine the role of interactive imagery processing. More animate words were remembered than inanimate words in both studies. However, the interactive memory task did boost memory for inanimates but not animates, decreasing the overall difference between animates and inanimates. Bonin et al. concluded that processing of animates may trigger the use of interactive imagery. However, because a significant difference between the number of animates and inanimates remembered remained in the interactive imagery condition, Bonin et al. suggested that interactive imagery is not the only proximate mechanism that gives animate stimuli a memory boost compared with inanimates.

The current study required new methods for testing the animacy effect that would reflect previous word-based tasks in the animacy literature while exploring animacy's role in memory and attention. A series of pilot studies was conducted for this purpose. First, a symbolic match-to-sample (*SMTS*) task was piloted to mirror the paired-association word-learning tasks that have been commonly employed by previous animacy research (Aslan & John, 2016; Bonin et al., 2014; Gelin et al., 2017; VanArsdall et al., 2013, 2015), as well as a serial-probe recognition task that embodies the recognition-memory tests with word stimuli. I also explored the effects of potential types of stimuli, specifically whether the line-drawing stimuli used by Bonin, Gelin, and Bugaiska (2014) would be adequate for reproducing the animacy effects found by Bonin et al. (2014) without including a written label with the picture stimuli. I also examined whether food stimuli would be a meaningful variable to include within the inanimate category.

Specifically, including food in the inanimate category could potentially weaken the animacy effect, given that it is also related to survival, but is categorized as inanimate.

## 1.2 Expected Results

The effect of animacy on memory and attention is a relatively new finding that appears in a limited number of studies. At this time, it is not known whether animacy status itself is the salient feature driving the animacy effect or if there are other as-yet untested cues that underlie the salience and recruit other memory and attention strategies. Most of the research has been focused specifically on why and when the animacy effect appears, rather than on how. Additionally, no studies to date have examined whether animacy effects would be apparent in responses by nonhuman animals. Testing the animacy effect in nonhuman animals may help to answer questions about ultimate mechanisms and give some insight into proximate mechanisms: whether animacy itself is the driving force of the memory and attention effect. Nonhuman animals encounter both living and nonliving stimuli, but are not subject to most of the experiential, language-based, or interpretation confounds that might affect human participants. Demonstration of animacy effects in attention and memory performance by nonhuman animals will thus serve both to extend the effect to new populations and also to narrow the list of possible mechanisms.

The task was designed to incorporate new picture-based methods that are suitable for both humans and nonhuman primates (hereafter *primates*) in testing the animacy effect. In the study, I examined whether the animacy effect is seen in primates and whether using pictures as stimuli changes the effect in humans. I predicted that if imagery plays a role in enhancing memory for animates, then using pictures would weaken the difference between animates and inanimates relative to effects reported in the literature for word stimuli (Aslan & John, 2016;

Bonin et al., 2014; Gelin et al., 2017; VanArsdall et al., 2013, 2015), similar to the findings of using interactive imagery in Bonin et al. (2015). Examining contexts in which the animacy effect appears or disappears will help identify whether animacy status has a direct effect on memory and attention or whether other mechanisms mediate animacy's role.

Animacy's effect on memory has primarily been tested using recall and recognition of written words. Only one experiment so far has tested animacy memory using pictures (Bonin et al., 2014); however, Bonin and colleagues also included the written name of the picture stimuli with each stimulus presentation. Thus, the field lacks evidence to show that the memory advantage is not solely a language-based effect and that it will hold true for rich visual stimuli without written labels.

The current study required new methods for testing the animacy effect that would reflect previous word-based tasks in the animacy literature while exploring animacy's role in memory and attention. A series of pilot studies was conducted for this purpose. First, a symbolic match-to-sample (*SMTS*) task was piloted to mirror the paired-association word-learning tasks that have been commonly employed by previous animacy research (Aslan & John, 2016; Bonin et al., 2014; Gelin et al., 2017; VanArsdall et al., 2013, 2015), as well as a serial-probe recognition task that embodies the recognition-memory tests with word stimuli. I also explored the effects of potential types of stimuli, specifically whether the line-drawing stimuli used by Bonin, Gelin, and Bugaiska (2014) would be adequate for reproducing the animacy effects found by Bonin et al. (2014) without including a written label with the picture stimuli. I also examined whether food stimuli would be a meaningful variable to include within the inanimate category. Specifically, including food in the inanimate category could potentially weaken the animacy effect, given that it is also related to survival, but is categorized as inanimate.

### *1.2.1 Method*

In the SMTS tasks, participants were shown an arbitrary symbol (lexigram) and were told to match the lexigram to its corresponding picture. (For specific pilot-study details, refer to Appendix A and B; however, the general method and results will be reported here.) Lexigram-picture pairs were randomly chosen before the experiment, and participants needed to learn the pairs by trial and error. During the learning phase, lexigrams were presented with two possible choices. A block of trials consisted of one trial for each lexigram-picture pair, and trials within each block were randomly ordered. After a certain number of learning blocks, dependent on the task, participants completed a filler task for 20 minutes, followed by a test phase. The test phase included one trial per lexigram-picture pair, and unlike the learning phase, there were four possible picture choices.

In the serial-probe recognition task (see Appendix A for detailed methods), participants saw a series of stimuli presented one at a time. At the end of the series, participants were presented with two stimuli and were asked to choose the stimulus that was in the preceding series. Series ranged from 6 to 12 items and included an equal number of animate and inanimate stimuli.

### *1.2.2 Results*

No significant differences between animate and inanimate stimuli were found in the SPR task. However, significant differences between stimulus types were found in all three SMTS task. The effect of adding food stimuli to the inanimate group produced conflicting results from what was expected, as it was hypothesized that the animacy effect would be weakened by adding a survival-relevant category to the inanimate category. In contrast, adding food increased the

difference between animate and inanimate groups in both experiments that included food as a stimulus category (see Table 1; Experiments A & B).

**Table 1. Pilot Results.**

| **Experiment** | **A** | **B** | **C** | **D** |
|---|---|---|---|---|
| **Task Type** | SMTS | SMTS | SMTS | SPR |
| **Animate Mean** | 0.82 | 0.74 | 0.68 | 0.81 |
| **Animate SD** | 0.21 | 0.24 | 0.25 | 0.08 |
| **Inanimate *M* (without food)** | 0.78 | 0.69 | 0.58 | 0.79 |
| **Inanimate *SD* (without food)** | 0.2 | 0.27 | 0.26 | 0.12 |
| **Inanimate *M* (with food)** | 0.76 | 0.62 | NA | NA |
| **Inanimate *SD* (with food)** | 0.23 | 0.24 | NA | NA |
| **p-value without food** | *p* = .07 | *p* = .21 | *p* = .08 | *p* = .41 |
| **p-value with food** | *p = .02* | *p = .01* | NA | NA |

### *1.2.3 Discussion*

The SMTS task provided some significant differences between animate and inanimate stimuli and seemed like the task that the monkeys would most likely and most quickly learn. Although food was initially included in the stimulus pool, foods were removed as potential stimuli after Experiment B because the addition of food to the analysis yielded confusing results. Although it seems important to investigate why humans treated food stimuli as extremely inanimate, that question was beyond the scope of the present thesis. Thus, food stimuli were excluded from subsequent experiments.

Additionally, although simple black-and-white line drawings are easier to standardize and control, it was concluded that real photographic images with no background would be more appropriate to use with nonhuman primates. Thus, it was decided to use the SMTS methodology

with photographs of animate and inanimate stimuli, excluding all food pictures from the stimulus pool.

### 1.3 Current Study

The current study consisted of two tasks: SMTS and antisaccade. SMTS was used to examine the role of animacy on memory, similar to the use of paired-association and list recognition paradigms in the majority of studies to date in which the effect of animacy on memory has been examined (Aslan & John, 2016; Bonin et al., 2014; Gelin et al., 2017; VanArsdall et al., 2013, 2015). The antisaccade task was used to explore animacy's effect on attention. Efficient antisaccade performance requires participants to control the reflexive tendency to orient toward a stimulus (whether animate or inanimate) that captures attention by flashing in the participant's peripheral vision. Thus far, no experiments have used antisaccade tasks to look exclusively at the role of animacy on attention. Antisaccade tasks SMTS and antisaccade have been successfully applied to work with primates (e.g. Johnston & Everling, 2006; Washburn, Hopkins, & Rumbaugh, 1989), but never with animacy as an independent variable. In accordance with animacy literature, it was predicted that more animate stimuli should be remembered than inanimate stimuli in the SMTS task, and that animate stimuli would capture and hold attention longer than animate stimuli in the antisaccade task, making it harder for the participant to orient away from the animate cue compared with the inanimate cue.

# 2 EXPERIMENT

## 2.1 Participants

*Humans*. A total of 128 undergraduates from Georgia State University participated in a one-hour study as partial fulfillment of a course requirement. From this sample, 47 were tested with the SMTS task and 81 were tested the antisaccade task, described below.

*Monkeys.* Five adult male rhesus macaques housed at Georgia State University's Language Research Center (LRC) participated in the study. All had been trained previously to use an automated test station located in their home cages (Richardson et al., 1990; Rumbaugh et al., 1989). The macaques had 24-hour access to the test station, working when they chose. The current experiment was part of a larger schedule of tasks, and therefore the monkeys only had access to the tasks in this experiment on a set number of days, dependent on individual schedules. Subjects were not food or water deprived for this testing.

## 2.2 Apparatus

*Humans*. Testing took place in the Individual Differences in Executive Attention laboratory at Georgia State University. Participants were individually seated at computer testing stations consisting of a computer, joystick, and headphones. For correct responses, participants heard a "whoop" sound, but for incorrect responses, participants heard a "buzz" sound and received an 8-s timeout.

*Monkeys.* A total of 128 undergraduates from Georgia State University participated in a one-hour study as partial fulfillment of a course requirement. From this sample, 47 were tested with the SMTS task and 81 were tested the antisaccade task, described below.

The macaques were tested using the LRC Computerized Testing Apparatus (described in Richardson et al., 1990). The test station consisted of a computer, joystick, pellet dispenser, and

speaker. For correct responses, the monkey heard a “whoop” sound and received an automated delivery of a 45-mg banana-flavored pellet via a pellet dispenser. For incorrect responses, the monkey heard a “buzz” and received a 12-s timeout.

All tasks were written in Python 2.7 with pygame 1.9.1 library installed. Picture stimuli were selected from the Bank of Standardized Stimuli (BOSS), a database of color photos with no background that have been normalized across more than 15 dimensions (Brodeur, Guérard, & Bouras, 2014). The set did not include multiple photos of the same kind of object or species (e.g., two different violins). Each monkey had an individualized subset of BOSS stimuli formed with data from the pilot classification task (see Appendix B).

## 2.3 General Procedures

*Humans.* Before beginning the task, participants received simple written instructions about the task, including information about feedback and trial initiation procedures. Participants were also informed that more correct responses would shorten the length of the task.

*Monkeys.* The experiment took place during multiple half-day sessions, intermixed with a variety of other cognitive tasks throughout the course of this study. Additionally, all monkeys completed pilot testing that confirmed they were capable of generalizing category membership of inanimate versus animate picture stimuli to novel picture stimuli (See Appendix B).

### *2.3.1 Experiment 1: SMTS Procedure*

In SMTS tasks, the relation between the sample (lexigram) and the target stimulus was randomly determined by the experimenter. Participants learned the arbitrary relationships and then could be tested on their ability to remember the learned relationships. In this task, memory strength was measured by correct and incorrect matches during a test phase that took place after initial training and an unrelated filler task.

For this task, a picture appeared on the screen and once participants touched the picture, using a joystick, the picture disappeared and two arbitrary symbols (hereafter lexigrams) appeared on the screen randomly in two of the four corners. One lexigram was the target that correctly paired with the picture and the other was a foil. The participant used the joystick to choose the lexigram that was paired with the picture. Lexigram-picture pairs had to be learned via trial and error throughout a training phase. Each training block consisted of each lexigram-picture pair presented one time in a random order. The test phase was identical to training, except there were 4 lexigram options instead of 2, appearing in all four corners of the screen. Participants completed a training phase, followed by an unrelated filler task, followed by a test phase (see Figure 1). The test session consisted of one trial per lexigram-picture pair. For the present study, 30 pictures were randomly chosen from the stimulus set (15 animate and 15 inanimate) and randomly paired with one of 60 possible lexigrams. The additional unmatched 30 lexigrams were intermixed as foils to correct choices.

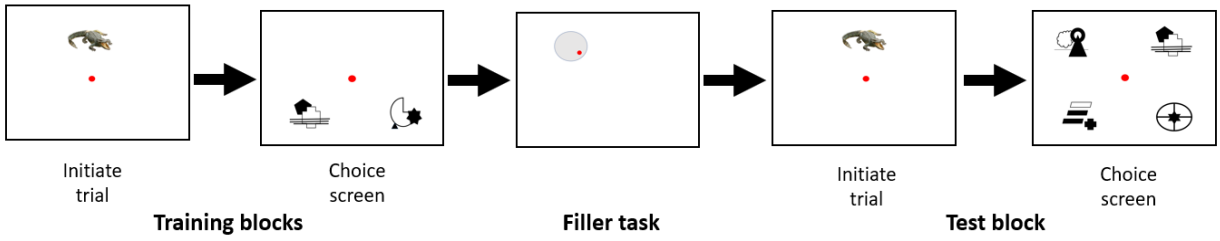


**Figure 1. SMTS Experimental Setup. Each box represents a different screen for the SMTS task. First participants completed a number of training blocks, followed by a filler task, and lastly, they completed the test block.**

*Humans.* Each participant completed one session consisting of four training blocks (*n* = 120). After training, participants spent 15 minutes working on an unrelated filler task that consisted of an absolute-quantity discrimination task that required participants to make speeded judgments on quantity size of an array of dots. After the filler task, participants completed the

test phase trials (*n* = 30). Participants who were not above 60% accuracy in the training phase were excluded from analysis (*n* = 9).

*Monkeys.* Each subject was given five sessions consisting of 25 training blocks, each block with 30 trials (*n* = 750 trials per session). Two monkeys needed an additional training session to reach an accuracy of above 60%. Subjects then received a test session consisting of 5 training blocks, followed by 20 minutes on the CHASE or Pursuit task (described in Rumbaugh et al., 1989) as a filler, followed by the test phase.

### *2.3.2 Experiment 2: Antisaccade Procedure*

In an antisaccade task, participants learn that the antisaccade cue serves as a reliable indicator that a target will appear on the opposite side of the screen to the peripheral stimuli. Participants can utilize the cue and shift their focus in the opposite direction to the target resulting in better performance in cued trials versus non-cued trials. Thus, the antisaccade task is used as a measure of inhibitory control. For the present study, participants made a speeded two-choice discrimination based on the identity of a target that was located either to the left or right of the screen, and the targets were either an “E” or an “F.”

Participants began a trial by moving their cursor to a box in the center of the screen. In order for the trial to begin, the cursor needed to remain in the box for 2 s. This ensured that participants were not holding the cursor in any one direction when the response screen appeared. Additionally, when the start screen appeared, the cursor would be randomly located in one of the four corners of the screen, also to ensure that the participant could not hold the joystick in one direction throughout the task. For the monkey task, the start box changed colors to help a monkey know when they were appropriately inside the square.

During training, participants received two types of trials: Introductory E or F trials, where the E or F would appear on the screen for 600 ms; and introductory antisaccade trials, where the E of F would appear on the screen for 400 ms after either a 400-ms pause, or 400 ms display of an antisaccade cue. The antisaccade introductory cue was a red "X" stimulus that appeared in 50% of the trials prior to, and on the opposite side of the screen from, the subsequent E or F target (see Figure 2). The training trials were used to teach participants to deflect the E and the F targets correctly and prepared participants to make use of antisaccade cues.

After training, participants received a test phase which consisted of an animate or inanimate stimulus as the antisaccade cue on 25% of the trials. In these trials, the E or F target would appear on the screen for 200 ms after either a 200-ms pause, or the display of an antisaccade cue. Speed and accuracy of joystick deflection were recorded for all trials.

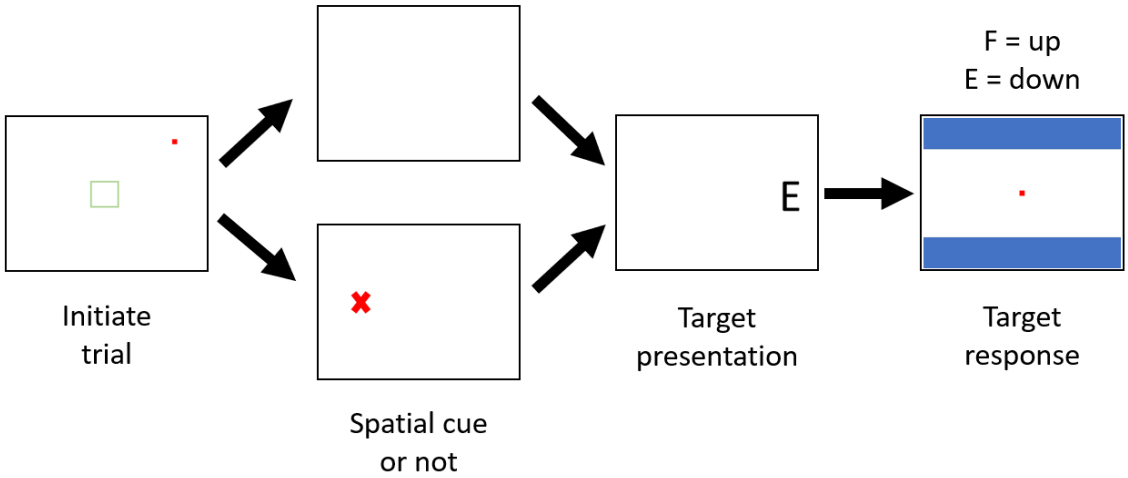


**Figure 2. Antisaccade Experimental Setup for the training trials. Each box represents a different screen for the antisaccade task.**

*Humans.* 81 participants completed one session consisting of the both the training and test phase. Participants were given 10 introductory E vs. F trials, followed by 15 introductory antisaccade trials, and then 150 test trials. Participants were excluded from analysis for not

finishing the task (*n* = 6) or for failing to reach learning criteria, set at 80% correct during introductory antisaccade trials during training, (*n* = 4).

*Monkeys.* Each subject was trained to deflect his cursor appropriately for E and F targets. Four out of five monkeys required forced trials on a proportion of training trials in order to remove bias for deflecting the joystick one way over the other. All monkeys required several sessions where the E and F did not disappear from the screen during their response. After a monkey reached 80% correct in non-forced training trials where the E and F remained on the screen, they received training sessions consisting of 100 introductory E vs F trials, and 600 introductory cue trials (Figure 3). When subjects reached 80% correct on these training sessions, they received a test session consisting of 100 introductory E vs F trials, 200 introductory antisaccade cue trials, and 900 test trials, in this order. One monkey was removed from the task when he did not exceed chance levels after 10 sessions (~8,000 trials) after the forced-trial training. Multiple efforts were made to improve performance by varying timeout length and number of pellets dispensed. Two additional monkeys were removed from the task after fourteen sessions for unrelated health concerns. Two monkeys reached the 80% correct training criteria and were able to complete the antisaccade test phase.

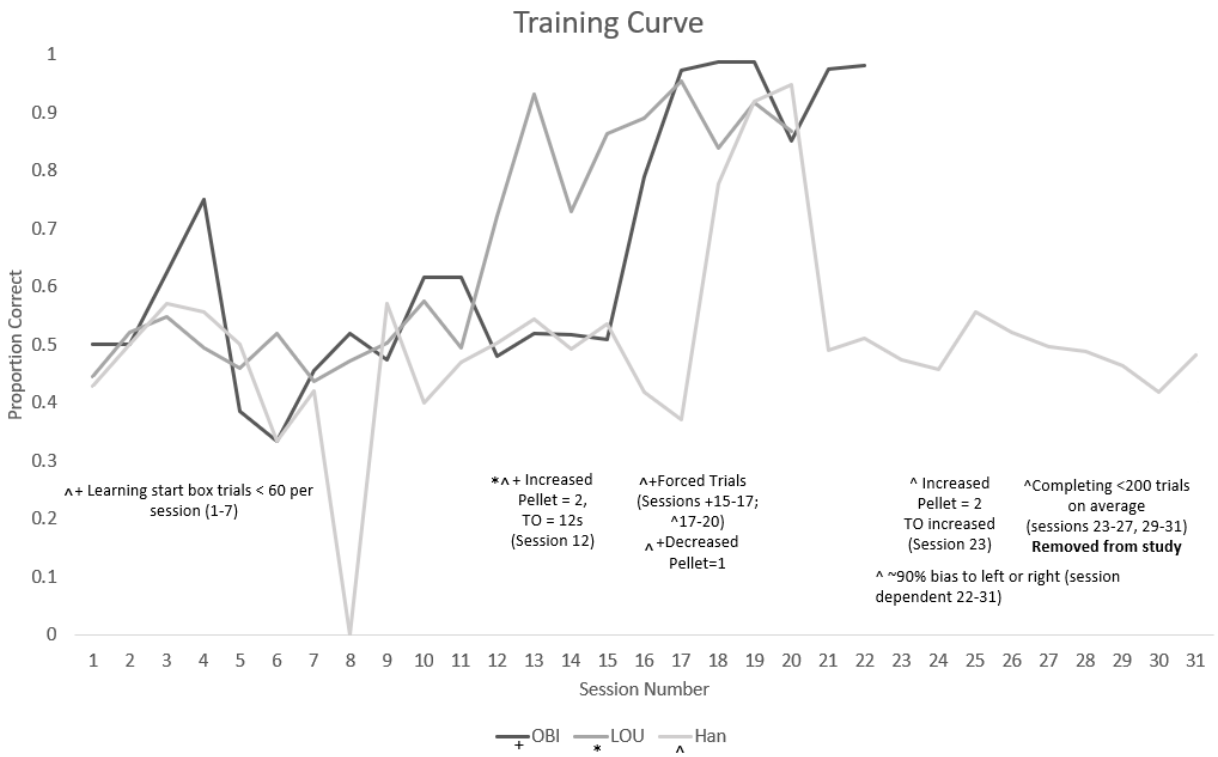


**Figure 3. Training curve for the 3 monkeys that were not removed for health concerns. *Learning start box* sessions were the sessions where the macaques were struggling to understand how to use the start box to begin a trial, and trial count was low in these sessions. TO is timeout length in seconds.**

# 3 RESULTS

## 3.1 SMTS

A one-tailed t-test was used to analyze the difference between the number of correct trials with animates and number of correct trals with inanimates in the test phase because it was expected that animates would be better remembered than inanimates. Humans did not show a significant difference between the two stimulus types: Animates, $M$ = 12.32 (82%), $SD$ = 2.28; Inanimates, $M$ = 12.00 (80%), $SD$ = 2.70; $t(37) = 0.77$ $p = .22$. Of the 38 participants, 14 remembered more animates than inanimates (in absolute terms), 13 remembered more inanimates than animates, and 11 remembered an equal number of each type. Monkeys, however, did remember significantly more animates than inanimates: Animates, $M$ = 5.6, $SD$ = 2.88; Inanimates, $M$ = 4.0, $SD$ = 2.74; $t(4) = 2.36$, $p = .04$ (Figure 4). Additionally, all human participants performed significantly above chance levels during the test phase (i.e., accuracy >= .4 for chance levels of .25, $p < .05$), but only two out of five monkeys were significantly above chance.

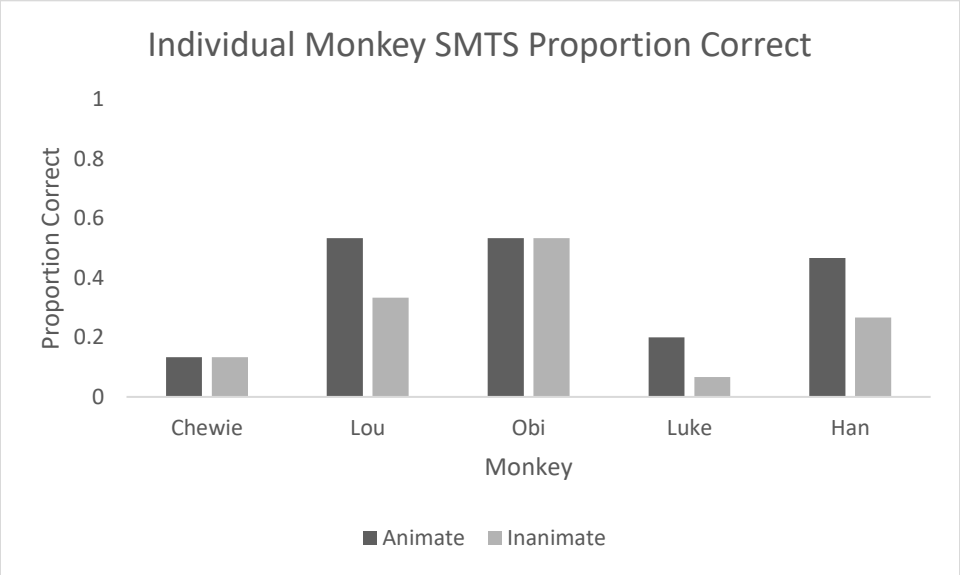


**Figure 4. Proportion correct for animate versus inanimate stimuli for each monkey subject.**

### 3.2 Antisaccade

In order to remove trials in which a participant was not paying attention, all trials with a reaction time greater than 2,000 ms were excluded from analysis. Outliers, defined as reaction times greater than 3 standard deviations from the mean, were also excluded. Response times were also excluded for trials in which an error was made. Because I expected animate cues to produce a significantly longer RT than inanimate cues, a one-tailed paired t-test was used to analyze the difference between the reaction time of correct animate versus correct inanimate antisaccade trials. On average, humans did not show a difference in reaction time when cued by animate cues compared to inanimate cues, $t(70) = 0.98$ $p = .17$ (Figure 5). Both monkeys that completed the antisaccade task exhibited a slight but nonsignificant increase in reaction time when cued by inanimate cues compared to animate cues, $t(1) = -1.48$ $p = .81$. Additionally, there was no significant difference in accuracy between animate and inanimate antisaccade trials for both humans and monkeys, $p > .05$ (Figure 6). Although there was no difference between reaction time of animate versus inanimate cued trials, cued trials resulted in significantly longer reaction times compared with baseline (non-cued) trials for humans, $t(70) = 3.87$ $p < .001$, but not for monkeys, $p > .05$ (Figure 5).

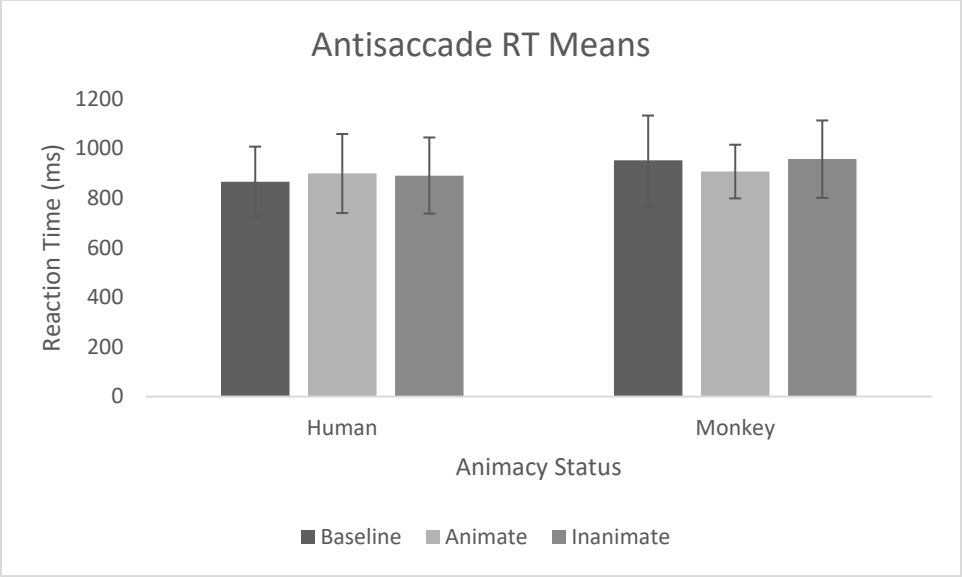


**Figure 5. Mean reaction time for correct responses on trials that were uncued (baseline) or cued with animate or inanimate stimuli in the antisaccade task, with standard deviation bars, by species.**

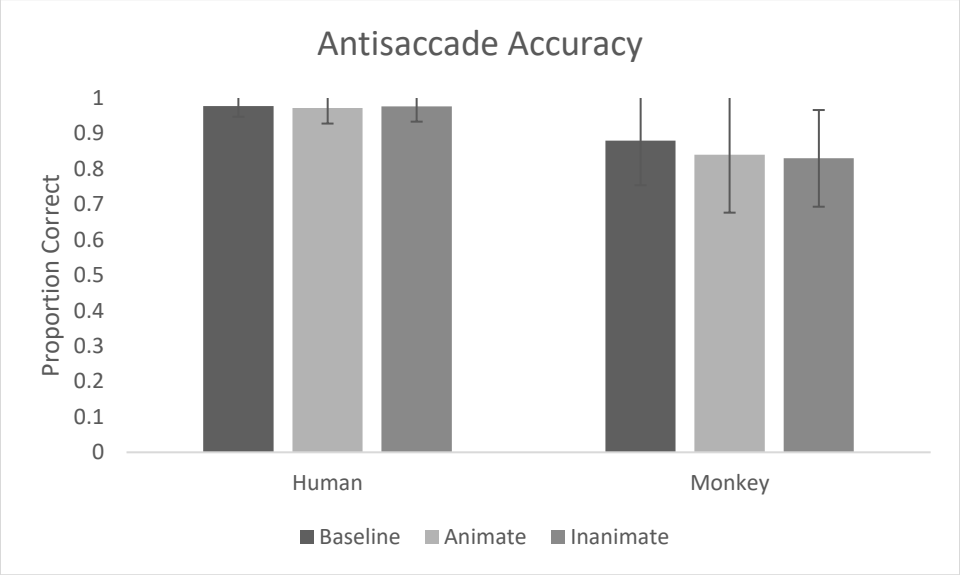


**Figure 6. Mean proportion correct on trials that were uncued (baseline) or cue with animate or inanimate stimuli in the antisaccade task, with standard deviation bars, by species.**

# 4 CONCLUSIONS

In this study, humans and monkeys were given both a SMTS and antisaccade task to investigate the hypothesis that the animacy status of stimuli would affect memory and attention. Specifically, in this experiment I tested whether animate stimuli were better remembered and more closely monitored than inanimate stimuli. According to the animacy effect hypothesis (VanArsdall et al., 2013) and the animate-monitoring hypothesis (New et al., 2007), humans have evolved to prioritize processing of animate entities over inanimate entities because animates would have been the more important to remember and monitor, as they could be predators, prey, mates, or sources of social information. However, the animacy effect and animate monitoring hypothesis are relatively new findings that are still in the early stages of being understood. Whereas researchers have claimed the effect to be robust (Bonin et al., 2015), there have been few publications and variations on methodologies, especially with respect to the effects of animacy on attention. The current experiment was the first, to my knowledge, to use pictures in any animacy-effect investigation and the first to use an antisaccade task with *any* stimulus types to test for the animacy effect.

The results of this study provide no support for an effect of animacy on attention and only superficial support animacy's effect on memory. Importantly, cued tasks took significantly longer to respond to, for human participants, compared with uncued trials, and monkeys showed no difference in RT to cued and uncued trials. This fails to replicate the basic antisaccade phenomena where we expect the cued trials to facilitate performance compared with uncued, to the degree the participant can utilize the cue and shift attention in the opposite direction. For humans, the data suggest both stimulus types captured attention and equally impeded performance, but for monkeys, there is no evidence that the flashing cues captured attention,

relative to baseline. Additionally, neither species exhibited a significant difference in reaction time when animate stimuli were presented versus when inanimate stimuli were presented as cues in this task. That is, animate cues did not capture and hold attention longer than inanimate cues in the antisaccade task. These results contrast with previous animate monitoring research in humans (Altman et al., 2016; New et al., 2007) and suggest that animacy status may not play as central a role in attention as previously thought. Findings by Altman and colleagues (2016) had suggested that animate entities in a change detection task will distract from inanimate targets even when the animate entity is unchanging and not the target of the trial. Therefore, it was predicted for the antisaccade test in the current experiment that animate stimuli would be more likely to capture and hold attention, as evidenced by less efficient shifting of attention away from the flash of the animate cue and towards the target stimulus (E or F). Additionally, it was anticipated that targets that flashed following animate cues would be more likely to be missed resulting in more incorrect responses during animate cued trials. However, the current experiment yielded no evidence to support these predictions. There was no difference in attentional control as a function of cue-stimulus animacy.

Why might this be? Unlike in the Altman et al. experiment (2016), animate and inanimate distractors were not directly pitted against each other in the current study, so that they competed directly for attention in individual trials. This allows for the possibility that rich visual stimuli, whether animate or inanimate, were equally distracting. Effects on attention might only be observed when the animate and inanimate cues are directly and simultaneously competing on a trial-by-trial bases, allowing one (e.g., animate) to pull attention from the other (e.g., inanimate). Follow-up studies could explore this suggestion using a dot-probe task (e.g., Mogg & Bradley, 1998) in which participants identify the location of a dot, presented on the left or right side of the

screen following the bilateral presentation of competing stimulus primes (e.g., an animate and an inanimate image, randomize to left or right positions). Allocation of attention is measured by the time to react to the presentation of the dot probe. Researchers suggest that RT is faster when attention is already allocated to the side where the dot probe appears (Koster et al., 2004). The dot-probe task has been used to study attention bias to emotion cues, for example, but never to stimuli that varied in animacy. We could hypothesize that if animates are attracting more attention than inanimate, that RT would be faster for trials when the dot probe appears on the same side as the animate stimulus, as attention would already be allocated to that side over the inanimate stimulus.

Additionally, the Altman et al. (2016) task presented vivid scenes where the animal, while camouflaged and thus sharing bottom-up features with the background, was still the only animate entity in the scene. However, the inanimate target was fully integrated into the inanimate scene, making the animate-inanimate distinction simultaneously a figure-ground distinction (Altman et al., 2016). This may have allowed animates to “pop out” as the singleton or odd-item (i.e., only animate) in the scene, capturing attention and being easily perceived as the figure-focal point of the scene. Altman and colleagues recognized this potential confound. In contrast, the current study involved animate and inanimate distractors with no background, removing the possibility of the figure-ground confound. All stimuli would pop-out by virtue of flashing. That is, it seems possible that the Altman findings were less about animate stimuli capturing attention more than inanimate stimuli, but rather resulted from the inanimate stimuli contrasting less from the background images. Perhaps an inanimate object embedded in animate stimuli would have similarly popped-out and captured attention. Follow-up investigations to the present study

should explore this possibility by using visual search in which target and foil animacy status are manipulated factorially.

It is interesting that superficial support for the animacy effect in memory was found in the SMTS monkey task. Monkeys did remember significantly more animate than inanimate stimulus pairs. However, caution is required when interpreting this difference. Despite previous training, two out of five of the monkeys were at chance-levels during the test phase, likely due to the increase from two-choice trials in training to four-choice trials in the test phase. Additionally, two out of five of the monkeys remembered exactly the same number of animates and inanimates. Whereas there is no denying that the overall means were significantly different, and thus provide some support for the animacy effect, it would be negligent to ignore the role of individual differences and chance-level results. Future research is needed that includes more monkeys and increases training criteria, to ensure all monkeys are above chance-levels at before the testing phase. This will help to reveal whether monkeys in general are more like the three animals that showed an advantage for memory of animates, or like the two monkeys who remained at chance performance.

Perhaps most damning to the suggestion that meaningful animacy effects were obtained for the monkeys is the fact that no significant difference was found for humans between the number of animate versus inanimate stimuli remembered in the SMTS task. It seems difficult to interpret animacy effects for monkeys in the absence of similar effects on the same task for humans. Again, these results contradict previous findings from research with human participants—including the encouraging trends in my own pilot tests. The literature suggests that humans are adapted to remember animate over inanimate stimuli. It must be noted, however, that all previous memory studies, to my knowledge, have had a language-based component. That is, either

participants were learning lists of words, relations between words (e.g Popp & Serra, 2015; VanArsdall et al., 2016) or, in one instance, participants were remembering pictures with word labels (Bonin et al., 2014). Animate words are better remembered than inanimate words, even when the words are matched on other variables like familiarity, frequency, and concreteness that are known to affect memorability (Bonin et al., 2014). Thus, previous research has left unanswered the question of whether animacy's memory advantage is solely a language-based effect. The lack of significant differences in the current experiment's SMTS data from humans provides the first evidence on this question, supporting this possibility.

There is, however, published research to suggest why picture stimuli might not be effective for demonstrating the animacy effect. Bonin and collaborators (2015) suggested that imagery may play a role in why animate words are better remembered than inanimates. They found that requiring participants to encode animate or inanimate words using interactive imagery did not increase the recall rate of animate words, but did increase the recall rate of inanimate words. Animate words were remembered significantly better, but Bonin and collaborators suggested that processing animate words may naturally prompt the use of interactive imagery more so than inanimate words. If the animacy effect depends on imagery mechanisms, then it might follow that one would get no animacy effect with picture stimuli, which do not require participants to generate mental images (the image is visible). Thus, it is possible that using pictures, as in the current experiment, equated and controlled for the differences in imagery previously found between animate and inanimate words. However, it must be acknowledged that many animacy researchers have attempted to control for imageability and similar variables in their word list and still found a significant difference between animate and inanimate memory (e.g. Nairne et al., 2017; Popp & Serra, 2015), so it remains undetermined the amount of influence imagery has on

the animacy effect. Subsequent research could be used to test this by presenting picture stimuli but specifically encouraging participants to imagine the images interacting with one another.

The use of picture stimuli was seen as a strength in the present study as they allowed nonhuman subjects to be tested using the same method as humans, and they allowed potential language-effects to be examined. However, the use of picture stimuli also introduced limitations. Previous animacy studies have utilized word stimuli because they are more easily controlled. Whereas the pictures used in the current study had been normalized along a variety of factors (Brodeur et al., 2014), it was impossible simultaneously to control for every potential influencing factor. Additionally, the BOSS stimulus set has been standardized for humans and not for monkeys, which may have influenced monkey results in unknown ways. The preliminary pilot testing with the monkeys addressed some of these potential ways, for instance by showing that monkeys could distinguish between the images, and could categorize them as animate or inanimate. The stimuli used with each monkey were the ones that he had individually categorized reliably as animate or inanimate, but may well have differed between other images on other variables that affected memorability.

I plan to continue this line of research by first rerunning the SMTS task with humans using words instead of picture targets and again with pictures with added written labels. These two tasks should replicate findings by Bonin et al., (2016), and any differences in results might indicate an issue in my SMTS method. I will then reevaluate how best to find the animacy effect in humans before reintroducing pictures, without labels, and before introducing the task back to the monkeys. I could then parse out why some researchers found the effect and others did not, looking not only at the type of stimuli (picture versus word), but also to explore systematically the individual stimuli and possible confounding variables (e.g., imagability) that may influence

performance. I also plan to continue to train the monkeys that were dropped from the antisaccade task and hope to reanalyze the data if and after they master the task.

Acknowledging the limitations of the present study and the additional research that is needed, taken together with my pilot research, I tentatively suggest that the animacy effects found by other researchers are limited to linguistic stimuli for humans. In contrast, I have found, with qualifications on interpretability, preliminary evidence for the animacy effect in primates. Across species, there is no evidence of an attention bias toward animate versus inanimate stimuli. However, given that the antisaccade task did not produce the pattern of results typical in the antisaccade literature, researchers may want to use other attention-capture paradigms to investigate this in the future.

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# APPENDICES

The pilot studies were exploratory; thus, multiple variables were manipulated in an attempt to find tasks and parameters that were best for this investigation. The tasks are described in general terms in the body of the thesis, and the results are reported there. The details below augment that discussion with the specific conditions administered.

## Appendix A

*Participants.* A total of 75 undergraduates from Georgia State University participated in a one-hour study as partial fulfillment of a course requirement. From this sample, 61 completed a SMTS pilot task (see Table 2) and 14 completed the SPR task.

*General Procedure.* Testing took place in the Individual Differences in Executive Attention laboratory at Georgia State University. Participants were individually seated at computer testing stations consisting of a computer, joystick, and headphones. For correct responses, participants heard a “whoop” sound, but for incorrect responses, participants heard a “buzz” sound and received an 8-s timeout. Before beginning the task, participants received simple written and instructions about the task, including information about feedback and trial initiation procedures. All tasks were written in Python 2.7 with Pygame 1.9.1 library installed.

### *Appendix A.1 SMTS*

The General Procedure for this pilot study was identical to that described in detail in Experiment 1 (see Figure 1). The studies were varied on the number of stimuli presented, number of training blocks, and whether food was included as a stimulus type. Additionally, the first two pilot studies used stimuli from the Snodgrass and Vanderwart’s set of standardized line drawings (1980), in an attempt to reproduce the animacy effect found in free recall with pictures from Bonin, Gelin, and Bugaiska (2013). All proceeding experiments in this study used a

selection of picture stimuli taken from the Bank of Standardized Stimuli (BOSS; Brodeur et al., 2014).

**Table 2.** Procedural differences in the three SMTS pilot studies.

| | **Pilot A** | **Pilot B** | **Pilot C** |
|---|---|---|---|
| **Number of human participants** | **41** | **13** | **7** |
| **Number of animate stimuli used** | **5** | **10** | **13** |
| **Number of inanimate stimuli used** | **5** | **10** | **13** |
| **Number of food stimuli** | **2** | **10** | **0** |
| **Number of training blocks of XX trials** | **5** | **5** | **3** |
| **Number of training trials (total stimuli X blocks)** | **35** | **150** | **39** |

***Appendix A.2 SPR Procedures***

To begin a trial, participants used a joystick to move the cursor to an “X” on the screen. Next, a series of picture stimuli flashed consecutively on the screen for 250ms each. Each series consisted of 6, 8, 10, or 12 stimuli with an equal number of animate and inanimate stimuli. Number of stimuli in a series and order of stimulus presentation varied randomly. After the series completed, a choice screen appeared that presented participants with two stimuli: one that had been seen in the preceding series and a foil. Participants were asked to choose the stimulus that they had seen before. The experiment consisted of 60 total trials. This study used a selection of picture stimuli taken from BOSS (Brodeur, Guérard, & Bouras, 2014).

**Appendix B**

*Subjects.* Five adult male rhesus macaques housed at the LRC participated in the study using the LRC Computerized Testing Apparatus (Rumbaugh et al., 1989; Richardson et al., 1990). The test station consisted of a computer, joystick, pellet dispenser, and speaker. The macaques had 24-hour access to the test station, working when they chose. The experiment took place during multiple half-day sessions, intermixed with a variety of other cognitive tasks during the course of this study. Subjects were not food or water deprived for this testing.

*Procedure.* Monkeys were trained to categorize animate and inanimate stimuli. Each trail consisted of one stimulus being presented in the top-middle of the screen, with a red X in the lower left corner and a blue O in the lower right corner. Monkeys were trained to move the cursor to the X when an animate stimulus was present, and to the O for an inanimate stimulus (see Figure 7). For correct responses, the monkey heard a “whoop” sound and received an automated delivery of a 45-mg banana-flavored pellet via a pellet dispenser. For incorrect responses, the monkey heard a “buzz” and received an 8-s timeout.

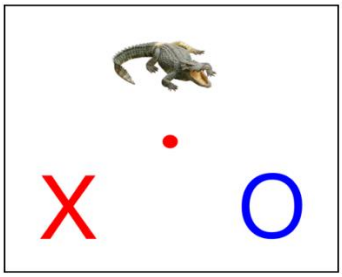


**Figure 7.** Screen view of classification pilot task. Participants learn to categorize animate stimuli as “X” and inanimate stimuli as “O”. Participants move the cursor to the correct letter to make their selection.

During training, 150 training stimuli were used, 75 of each category. Training stimuli consisted of non-standardized stimuli from the BOSS database (Brodeur et al., 2014), as well as

other backgroundless photos found on the internet. Each session consisted of 3000 trials, in random order.

After each monkey was categorizing training pictures correctly for > 90% of trials over a series of at least three sessions, a set of twelve to twenty novel pictures (equal number of animate and inanimate) was introduced in to the mix of potential stimuli for a period of one session. Probe stimuli were presented randomly in 1/6$^{th}$ of the trials. Novel probe stimuli consisted of standardized pictures taken from the BOSS database, a database of color photos with no background that have been normalized across more than 15 dimensions (Brodeur et al., 2014). In total, there were 13 sets of 12-20 novel probe pictures (226 pictures total), resulting in 13 sessions of post-training probe data. The monkeys' categorization of these novel pictures resulted in variable and random feedback to ensure the monkeys could not learn correct responses from the program.

*Results.* The percent correct for novel pictures was analyzed and sets of pictures that resulted in above 80% correct were formed for each individual monkey (Table 3). If a monkey completed fewer than 1,000 trials on a given probe session, that session was repeated. Pictures from individualized sets were then used as the stimuli in Experiments 1 & 2. The same pictures were not used in both experiments for each monkey.

**Table 3.** Proportion of novel stimuli that each monkey received greater than 80% correct on, and thus were included in the individualized stimulus sets.

| | **Proportion >80%** |
|---|---|
| **Chewie** | 0.704 |
| **Han** | 0.77 |
| **Lou** | 0.588 |
| **Luke** | 0.664 |
| **Obi** | 0.752 |