SOME ASPECTS OF MAGPIE (*PICA HUDSONIA*) LEARNING

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ABSTRACT

I reviewed a method and apparatus used to establish a research paradigm studying the free-ranging black-billed magpies (*Pica hudsonia*) that are local to Western Canada. Corvids, like the magpie, have a long history of being fruitful model species in psychological investigations, and their relative abundance in Lethbridge affords an opportunity to conduct meaningful research in a way that is minimally invasive, cost-effective, and logistically practical. Here, I discuss the exploratory investigation of the factors that relate to magpie engagement with custom, automated feeding apparatus, after considering initial oversights in theoretical rationale and logistical failures to implement theory-driven study design that incited the necessary shift in telos. Several results (including failures) provide fruitful insight into how to better implement future investigations with the custom apparatus. In reflection, this investigation has clearly demonstrated that our apparatus affords a minimally invasive way to study the local corvid populations (both magpies and the common crow) and provides relatively high experimental control compared to typical investigations of organisms in ecological valid settings.

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CHAPTER 1: INTRODUCTION

This thesis is about two things: magpies, and how to study them. More specifically, this is about creating and evaluating a research program that affords a viable method for studying the local, free-ranging magpies in western Canada.

There are several acceptable rationales for this project. One such rationale is that magpies belong to the bird family, Corvidae, which have a long history of serving as fruitful model species in psychological investigations (see literature review for citations). Given the relative abundance of free-ranging magpies in western Canada, developing a research program on these birds could pay epistemic dividends. If one could develop a flexible enough method of investigation, one could then subsume the rationale and value intrinsic to any domain of psychological study that the method could capture. Another rationale is that most comparative investigations of corvid psychology are conducted with captive birds, who have quite different histories—and, consequently, possibly quite different "psychologies"—than free-ranging birds. Developing a method to study free-ranging birds would then provide a unique comparative sample.

When the project was recommended to me, I was given no limits on what aspects of magpie behaviour I could investigate, nor any limits on how I was to execute the task (frankly, I was never explicitly limited to magpies, either). The world was my oyster. Moreover, at the time, I was interested in how organisms form associations over gaps in time; so, I thought I could develop a method of investigation with the magpies that allowed me to work on the topic. This did not work out. The actual direction of investigation became increasingly exploratory, and its products are descriptive and material. In meeting the task of developing a method to study local, free-ranging magpies, I, along with a research assistant/collaborator (KB), developed an automated bird-feeder, with a camera mounted to record interactions. The bird-feeder affords many possible arrangements of quasi-experimental study designs. When implementing the feeder in practice to study how organisms form associations over gaps in time, we ran into some troubles, which will be discussed in later sections. The most practical of our concerns was the fact that the magpies did not always engage with the apparatus. So, the ultimate question pursued in this thesis, as relates to evaluating our method for studying the magpies, became: *what factors predict magpie engagement with our automated bird feeder?*

Despite not being the final direction of investigation, the time and effort that went into the original designs—relating to how organisms form associations over gaps in time—are not sunk costs, and they will still be discussed in this thesis for two reasons. 1) It still relates to the demonstration of skills expected of a master's student: theoretical and inferential reasoning, hypothesis testing, experimental design, etc., which are not so obviously demonstrated in the exploratory direction that the investigation moved towards 2) The exploratory aims and direction we arrived at are still a consequence of the original designs.

Given this, the organisation of this thesis is as follows. In Chapter 2, I review the literature that relates to the relevant aspects of the ecology and behaviour of black-billed magpies, and briefly review the theory that relates to the original study designs that inform the development of our actual investigation.

In Chapter 3, I review the apparatus and method involved in the investigation. This entails an overview of the specifications of the bird feeder designed for the task, a review of the original study designs, and a description of the variables that relate to my ultimate exploratory question.

Chapter 4 is a review of the descriptive results; these are broken down into three subsections: one on the general ecological factors that affect interaction; one looking at how foraging related factors affect interaction; and one looking at how learning related factors affect interaction.

I conclude by describing the implications of the results, and outlining possible directions and future applications for my method of study.

CHAPTER 2: LITERATURE REVIEW

“A difference, to be a difference, must make a difference.”

—Gertrude Stein

2.1. I’M WITH THE MONKEY PEOPLE

In the research group of which I’m part, people have a tendency to identify themselves by their study species: “I’m one of the monkey people,” someone might say. As endearing as this is, I would not, however, call myself a “magpie person.” I have no particular interest in magpies. They are merely a convenient model species for theoretical investigation. It is the theory that I have always been interested in—general patterns that relate not only the behaviour of monkeys or magpies, but run the gamut right from bacteria to Bach.

For instance, the generalities of operant (e.g., Thorndike, 1911; Skinner 1938) and associative conditioning (e.g., Thorndike, 1911; Watson, 1924; Pavlov, 1927) are ubiquitous in the animal kingdom (for review, see Warren, 1965; Mackintosh, 1974), and can be seen even in the worlds of protozoa (single-cell organisms; e.g., Gelber, 1952; for review, see Dussutour, 2021) and select plants (e.g., Gagliano et al., 2016). This is the study of learning, relating to the philosophy of behavioural science known as behaviourism, which broadly refers to how an organism’s history shapes its behaviour. If one understands the generalities of behaviourist theory, for instance, —and, more importantly, has some experience applying them—one can shape not only the behaviour of laboratory animals, but the real-world behaviour of pets, people, and the personal contingencies that govern one’s own behaviour.

The thing about generalities, however, is that they are still subject to the idiosyncrasies of a given species, and, moreover, the idiosyncrasies of the individual. For instance, you cannot teach an organism to differentially respond to cues of different colours if those colours do not exist in its umwelt. Of course, the umwelt, a term referring to the perceptual world an organism occupies (von

Uexküll, 1937), is not a fixed thing. The study of learning could just as aptly be described as the study of shaping the umwelt—shaping the world of differences that matter to the organism. Still, limits exist on the extent to which an umwelt can be shaped. These limits exist as a consequence of an organism's evolutionary history and the material realities that follow: the particularities of its sensory system, its structural morphology, its genetic predispositions, and the environment wherein these features were forged. For instance, a person with an extra long-wavelength colour receptor (a tetrachromat) could sort a basket of tomatoes into finer colour categories than a trichromat, because of the differences of their respective sensory systems; on average, a cat is better at solving puzzle boxes than is a dog, not because it is "smarter" or more "cognitively inclined", but because it is far more anatomically dexterous, particularly in the forelimbs required to navigate the differences of the box (Thorndike, 1911); the genetic predisposition of a rat allows it to readily associate a visual cue with a tactile pain and a gustatory cue with a belly ache, but not a visual cue with a belly ache, nor a gustatory cue with a tactile pain, as the prior two contingencies would commonly exist in its evolutionary history, whereas the later two would not (Garcia & Koelling, 1966); and an albatross could not learn to successfully navigate the seas if the seas did not offer reliable cues about the consequences of travel direction as they relate to albatross fitness (van Woerkum, 2023).

If one minds only the general, and ignores the species level idiosyncrasies, one can get into trouble, as I have (multiple times) while conducting this research. Inasmuch, I have conducted a literature review pertaining to the ecology and behaviour of magpies. Unlike my earlier literature reviews pertaining to psychological theory, my review of magpies is admittedly post hoc; however, it still has the benefit of clarifying the research conducted through hindsight.

2.2. PICA (PICA) HUDSONIA

To preface talk of magpies, I find it pertinent to mention that most citations in this section are primary sources cited in T. R. Birkhead's magpie treatise, *The Magpies* (1991). The impression that I gather is that Birkead's book is considered *the* work to be acquainted with, if one is interested in learning about magpies; it is a thorough, descriptive treatment of magpie ecology and behaviour, and I trust his epistemic authority on the topic. Instead of repeating the tedious 'Joe Blow, as cited in Birkhead, 1991,' the reader may assume that I am citing Birkhead's erudite summary of the primary literature when I write '(Blow, 1919).' In this subsection, I will make note of any exceptions to this rule, as they will be far less common than the default.

As mentioned, magpies belong to the corvid family, along with species like crows, ravens, and jays (there are about 102 known species of corvid; Goodwin, 1986). Several species of corvid have a history of being fruitful model species in psychological investigations (citations to follow are not cited by Birkhead). Studies have covered such topics as learning (e.g., Magnotti et al., 2017), memory (e.g., Clayton & Dickinson, 1998), and different domains that relate to behavioural flexibility, including the inhibition of prepotent responses (e.g., Ashton et al., 2022), reversal learning (e.g., Bond et al., 2007; Ashton et al., 2022), and tool use, problem solving, and innovation (e.g., Bird & Emery, 2009; Hunt, 1996). Given the usefulness of other corvids in comparative psychology, and the relative abundance of the local free-ranging magpies in western Canada, devising a research program studying these birds looked like it could pay epistemic dividends.

There are considered to be thirteen distinct species of magpie (Vaurie, 1959); however, there is some contention about whether certain species should be considered subspecies, and vice versa (Birkhead, 1991). For instance, our species of study is the North American subspecies of Black-Billed Magpie, *Pica pica hudsonia*; yet, the growing consensus (as reviewed by Birkhead, 1991) is that it should be considered a wholly different species than its European counterpart, as there are behavioural—and subtle morphological—differences (and, in fact, more recent reviews of the species refer to it as simply '*Pica hudsonia*', instead of the '*Pica pica hudsonia*', which might imply that consensus has shifted; e.g., Trost, 2020). One of the implications of this is that much of the research conducted on the European Black-billed Magpie, *Pica pica*, may not be generalizable to the *P. hudsonia*.

The *P. hudsonia* is characterized by its black plumage, deeply contrasted by patches of white, and its long, thin tail (identification as summarized by Trost, 2020; not cited in Birkhead). The black plumage of the wings and tail may appear to have an iridescent green-purple shimmer. The adult *P. hudsonia* ranges from 45-60 centimeters long, and weighs between 145-210 grams. Female and male birds are similar in appearance; however, the males are on average about 16-20% larger (Linsdale, 1937; Birkhead, 1991; Trost, 2020). In the *P. hudsonia,* juveniles of a year old are closer in size and appearance to adults than is found in other magpie species (Scharf, 1987).

Relative to other bird species, magpies are considered sedentary (Birkhead, 1991); they do not migrate seasonally; nor do they travel far outside of their established territories. The *P. hudsonia* is said to be particularly sedentary and does not display the same territoriality of its European counterpart (European magpies will defend a territory averaging 260 meters across, whereas the *P. hudsonia* will only defend a territory of about 30 meters, if at all; Birkhead, 1991; Reese & Kaldec, 1985). These territories are often centered on a nest for breeding pairs and seem to be established in relation to food availability. Moreover, unlike other magpies, the *P. hudsonia* often abandon established territories in the winter, when food is scarce (Muugas & King, 1981; Buitron, 1988).

Magpies are opportunistic feeders, with a generalist diet (Birkhead, 1991); in the summer, they can eat the available insects, small animals, and carrion; in the winter, their diet consists mostly of plant matter. The *P. hudsonia,* like other magpies, will also cache available food for short periods of time (no more than few days; as reviewed by Birkhead, 1991). This is, they will take food in their throat pouches, carry it to a new location, and hide it for future retrieval. These caches are usually made in the ground and are virtually invisible to the human eye. There is some evidence to suggest magpies relocate these caches through their sense of smell (Buitron & Nuechterlein, 1985). The rate of caching behaviour decreases in the summer when food is more available. Moreover, during this time, when magpies are rearing offspring, their time spent foraging increases (Møller, 1983).

Preparation for offspring begins in mid-to-late spring (usually corresponding with the melt of winter snow and the boom of new foraging opportunities; Birkhead, 1991), and starts with the collection of materials for a new nest; this usually takes the combined efforts of a breeding pair a little over a month. Nest building is followed by a short breeding season, spanning an average of 11 days. During this time, in an effort to deter extra-pair copulations (that do sometimes occur), the male will leave the female only to forage. The female is wholly fed by the male during this time and continues to be fed through a period of incubation that lasts approximately 18-24 days (Verbeek, 1973; Goodwin, 1986; once a clutch is complete, the female will spend between 90-95% of her time in the nest). Once hatched, the mother must continuously care for the chicks for the first 5-10 days of their lives; but, approximately fifteen days after hatching, the mother spends as little as 10% of her time with her chicks (Buitron, 1988). Chicks fledge and leave the nest at a median of 27 days; however, they are still assisted by their parents for a span of 4-6 weeks thereafter (the parents will still find food for the chicks, and will work to deter or avoid predators, etc.; Buitron, 1988). This takes the magpies into the mid-late summer, where the overall rate of foraging declines to the norm (Møller, 1983).

Relative to their European counterparts, the *P. hudsonia* is considered a social magpie, as they are less aggressive and, as mentioned, defend much smaller territories (Birkhead, 1991). A supposed explanation for this is that harsher winter environments force a higher degree of sociality, as foraging cannot be reliably limited to distinct territories; birds must cross paths with one another in the common pursuit of food. With respect to hierarchy, males are said to be dominant to females, and, in the *P. hudsonia*, first-year birds are said to be dominant to adults (Komers, 1989).

Within Western Canada, there is also a great deal of overlap in the territory and ecological niche of the black-billed magpies and the common crow (for review, see Birkhead, 1991). Given as much, these two species often compete for food within areas where their territories overlap; and, as crows are, on average, the larger of these species, they often suppress magpies' access to preferred food sources (e.g., Eden, 1987). Moreover, the presence of crows is usually associated with an increase in the yearly mortality and a decrease in the flourishing of magpies (e.g., Högstedt. 1980a, 1980b; Bayens, 1981; Birkhead, 1991).

Two final points that did not seem to fit nicely with other sections, but are still relevant to my thesis: First, magpies have a large gross mortality rate; in documented populations, it has been recorded between 33-60 % of birds perish year-to-year (as reviewed by Birkhead; highest mortality rates seem to occur in April, around the breeding season; Møller, 1982). Of course, first-year birds account for a high percentage of this; the mortality rate for adult magpies' year-to-year is closer to 25%. Second, contrary to popular belief, magpies seem to have no particular interest in shiny objects (Shepard et al., 2015; not cited in Birkhead); free-ranging birds even present some degree of neophobic behaviour towards them. Moreover, they do not seem particularly fond of the colour blue (Shepard et al., 2015).

2.3. MIND THE GAP

Given some background on magpies, we may turn to a brief review of the theory that informs the original designs for my master's research. As mentioned, the set of questions relating to this theory were substituted for a categorically antecedent set, so we will not cover this topic in exhaustive detail. At the start, I was interested in how organisms form associations over temporal and geographic discontinuities. To understand why the ability to form associations over gaps in time and space is of interest, particularly to the behaviourist, it is first pertinent to address the related concepts of *association* and *reflex* in their simpler forms.

In considering the term reflex, I will use a more fluid definition than that of the early behaviourists, who had adopted their concept from physiology (e.g., Watson, 1924; Pavlov, 1927). We will take the radical behaviourist's conception of the term:

> "The environment enters into a description of behavior when it can be shown that a given part of behavior may be induced at will (*or* according to certain laws) by a modification in part of the forces affecting the organism. Such a part, or modification of a part, of the environment is traditionally called a stimulus and the correlated part of the behavior a response. *Neither term may be defined as to its essential properties without the other,*" (Skinner, 1938, p. 9, emphasis added).

The functional relationship between stimulus and response constitutes a reflex. Another way to express Skinner's conception of reflex is to say that certain classes of responses are correlated with certain classes of stimulations, such that it is more probable that we will see response X when the historically correlated stimulus class is present in the perceivable environment of the perceiving organism (hence, the "or according to certain laws."). Different probabilities of responses are tied to latent variables Skinner refers to as response strength and reflex reserve. Given this conception of reflex, there is no claim made that behaviour is 'stimulus-bound', contra earlier conceptions of the term; moreover, there is no claim that one can carve everything in the world into discrete categories of either stimulus or response, independent of some perceiving organism; it is merely the difference made by some meaningful difference in the organism's environment.

Typically, the term association refers to the meaningful relationship between two things—or stimuli—as they exist within the perception of some organism, which is corroborated by the reflexive relationship between stimulus and response; however, it is not the point to get bogged down in the technical. Moreover, how these terms are used also turns on some debate about whether an organism may be said to 'possess' reflexes or associations (as relates to the literature on dispositional attributions, Skinner, 1938; Ryle, 1949; Dennett, 1978; Skinner, 1974; Place, 2005; Dennett 2005; in that order); however, this is also tangential to the point of our story. Functionally, for this thesis, it is sufficient to say that an organism *has* associations or reflexes, regardless of how literal this language might be (even though, elsewhere I argue that, irrespective of language, it is still the *history* that is the focal antecedent; LaValley, in prep). Still, we can save such a discussion for a time and place where taking on board the extra complexity would make a difference with respect to how we might interpret things.

Given our definitions, we may turn to the functionalist story of how an organism is said to acquire associations. For brevity, we will focus on contiguity's relevance to the story. Aristotle posited that we associate things based on four principles (as summarised by Shaw, 2016): similarity (we associate things that are alike), contrast (we associate opposites, like big and small), contiguity (we associate things that appear at the same place and time), and frequency (we associate things that appear together often). Hume refined this list to three principles (1748): resemblance, contiguity, and cause or effect. Today, we have closer to a single principle of association, contiguity (Pavlov, 1927; Guthrie, 1930), which may be modified by, roughly, three types of condition: exercise, effect, and the evolutionary and ecological constraints previously discussed. This is to say, we can associate stimuli that are spatially and temporally related within the scope of our attention. The more these stimuli are presented together in our environment—that is, the more we exercise the association—the easier it is to develop and strengthen the bond (Thorndike's law of exercise, 1911). The formation of an association can also be facilitated by the qualitative conditions of the associative context. An organism is more likely to associate some pair of stimuli if they relate to meaningful consequences in the organism's world, implicitly or otherwise.

Different from those conditions that mediate the ability to form associations, Aristotle and Hume's principles like resemblance and contrast are, instead, treated as factors that relate to how associations are categorized by the organism, and whether it will discriminate between certain stimuli as to warrant unique responses, or if the stimuli will be generalized to a single reflexive relationship. For instance, an artist might have many responses to the range of light wavelengths that the untrained organism would call 'purple'. The untrained organism has a single reflexive response to the range of stimulation, whereas the artist discriminates between violet and indigo and periwinkle, etc.

It should be said, here, that this story is obviously descriptive, not explanatory. I have said that over the history of the conception of associations, contiguity was the only surviving variable that seemed to be a necessary condition for associations to develop; however, I haven't said why this is the case; nor have I cashed out the mechanisms that would tell us how contiguity and association work together. As I have said, this is a functionalist story, and much is wanting in terms of mechanistic explanations for learning (or, minimally, much is wanting about my knowledge of mechanistic explanations for learning). In the words of Skinner (1974, p. 236-7):

> "Something is done today which affects the behavior of an organism tomorrow. No matter how clearly this fact can be established, a step is missing, and we must wait for the physiologist to supply

> it. He will be able to show how an organism is changed when exposed to contingencies of reinforcement and why the changed organism then behaves in a different way…”

The *start* of an answer to this problem might be captured in the pithy Hebbian mantra “cells that fire together wire together,” which refers to the neurons that relate to the perception of some pair of stimuli wiring together as they are concurrently stimulated (1949). I do not know if this mechanistic story is true (nor does it entirely answer all the questions); however, it sets up hypotheses that are consistent with the functionalist story of contiguity. For instance, this story would explain why organisms can’t form associations if the things associated are separated over some gap in time or space. If the neurons that correspond to the perception of the relevant stimuli aren’t given the opportunity to ‘fire together’, how could they ‘wire together’?

Of course, all this is to say that contiguity is an important part of the story of association and reflex. When early cognitive-behaviourists and cognitivists began to provide evidence of organisms demonstrating the association of temporally discontinuous stimuli (we will come to examples), not only was the principle of contiguity called into question, but so was behaviourism as a whole.

There are several study designs wherein the organism is believed to demonstrate an association between things that did not exist at the same time and space, including paradigms like latent learning (e.g., Blodgett, 1929; Tolman & Honzik, 1930b; Tolman, 1948) and insight or recombination learning (e.g., Tolman & Honzik 1930a; for a great review, see Enquist et. al, 2023, chapter 5). Here, we will focus on those where time can be most cleanly isolated as an independent variable: trace conditioning (examples given: Kamin, 1954; Church & Black, 1958; Taub et al., 1965; Schneiderman, 1966; Black & Black, 1967; Meredith & Schneiderman, 1967) and delay-of-reward (examples given: Wolfe, 1934; Perrin, 1943; Myers, 1958) style studies.

2.3.1. TRACE CONDITIONING

Trace conditioning studies follow the format of standard associative conditioning experiments and track a measure of time between the presentation of the conditioned and unconditioned stimuli—the interstimulus interval. This contrasts with the regular variety of associative conditioning experiment, known as delay conditioning, wherein the presentation of the conditioned and unconditioned stimuli overlaps in time to some extent.

We may better understand the difference between these two types of conditioning experiment by considering an elementary example of associative conditioning, like Pavlov's dogs (1927). Pavlov's original experiments fit the description of the delay conditioning format. If we suppose that Pavlov started a metronome before feeding his dogs, and let it run while they ate, the sound of the metronome and the food experience obviously overlap in the dogs' environment. In time, because of this reliable co-occurrence, the sound of the metronome, an initially neutral stimulus, comes to predict food, an inherently valuable (or unconditioned) stimulus, to the effect that the metronome comes to elicit the drooling (conditioned) response, even without the presentation of food.

The issue arises if we imagine a gap in time in between the experience of the metronome and the food (trace conditioning). Intuitively, small gaps in time do not seem problematic. If we imagined that Pavlov started the metronome for only three seconds and fed the pups a single second after extinguishing the sound, it would not strike us as surprising if the metronome could come to elicit the drooling response in a comparable number of trials to the delay condition. The gap is small enough that the sound event and the food event still seem to occur in an orderly, connected sequence within the scope of the dogs' experience. But what if we extended the dead air between these events? Would the metronome still come to elicit the drooling response if it preceded the food by ten seconds? Thirty? A minute? Perhaps a better question still: what is the limit that one can stretch the interstimulus gap so that half the dogs will demonstrate an association, and half will not—an interstimulus threshold, if you will.

Unsurprisingly, the answer to this question is likely to depend on numerous factors: what stimuli fall on either side of the gap in time, the species of organism in question, the particular individual of the species being studied, and the associative context (appetitive or aversive). We do know that, generally, as the interstimulus gap gets larger, more trials are needed before the conditioned stimulus comes to elicit the conditioned response (Reynolds, 1945; Kimble, 1947). This is to say, the larger the gap, the harder it is to form an association).

It is important to note, here, that the interstimulus gap is *not* the same as the CS-US interval. The CS-US interval measures the time between the onset of the conditioned stimulus and the onset of the unconditioned stimulus. The conditioned stimulus need not be terminated at any point during this interval. This is to say, you may see papers that report eighty second CS-US intervals, but this does not necessarily mean that there is eighty seconds of dead air between the presentation of the conditioned and unconditioned stimuli in these instances. In contrast, the interstimulus gap measures the time from the *termination* of the conditioned stimulus to the onset of the unconditioned stimulus. It is important that we do not conflate these two measurements.

2.3.2. DELAY-OF-REWARD

The delay-of-reward paradigm mimics aspects of trace conditioning, but in a more operant format; the subject is presented with some discriminatory stimulus/stimuli; if the subject performs the correct response, it is rewarded; however, like the name suggests, the reward is given after a delay. One can then gauge if the subject forms an association based on whether the reward serves to increase the probability of the subject performing the correct response. Does the discriminatory stimulus become a difference that makes a difference?

Again, the logic of the design might be more intuitively grasped with an example. Let us imagine a magpie friend, Margo, who is presented with two distinct stimuli, and has to discern which one to respond to in order to obtain a reward. Margo is in a special cage that is fitted with two side-by-side light bulbs (the position of which may be interchanged between trials to eliminate confounding orientation effects). One light bulb emits red light, and the other, green. On the floor beneath these lights are keys that Margo can peck. Let us suppose we want to teach Margo to peck the key beneath the red light in response to *both* these lights being turned on. If she pecks this key, then she can be rewarded after a short delay period; if she pecks the key under the green light, she receives no reward. There is no question that, if rewards were not delayed after correct responses, Margo would quickly come to favour the key under the red light, learning that such a response means food, whereas a response to the green light means nothing. The question is: how long of a gap can be imposed between the correct response and reward before a subject fails to discriminate between the lights in the same manner? If, from the outset, we waited a minute before rewarding Margo for each of her correct responses, would she learn to favour the red-light-key at all, or would these events be too far apart to connect?

2.3.3. THE HYPOTHESES ON OFFER

Given the above, if observations of organisms forming associations over gaps in time are valid, the question becomes: How?

The first class of explanation would fall under the heading of *chaining* (e.g., Skinner, 1934; 1938), which refers to how a discrete set of reflexes might become bound into a longer sequence of stimulus-response relationships. Put more succinctly by Skinner, “the response of one reflex may constitute or produce the eliciting or discriminative stimulus of another,” (1938, p. 32).

From a chaining perspective, we would not consider a gap in time as being truly empty; rather, the gap is associated with what is sometimes referred to as *secondary reinforcement* (Spence, 1947; Grice, 1948), which is propagated inductively, backwards down the chain (also referred to as *backward chaining*; for a great review, see Enquist et al., 2023). An example may clarify this. Let us imagine a rat friend, Bianca. Rats are said to learn mazes "backwards," (e.g., Peterson, 1920). This is, if Bianca is placed at the start of the maze and there is some reward at the end, the first time she is reinforced is in the environment surrounding the end of a maze, which likely includes the final intersection before the endpoint. The unconditioned positive valence associated with the reward becomes conditioned onto the surrounding environment; this is, the environment begins to take on the so-called secondary reinforcement. The next time Bianca reaches the intersection before the end of the maze, she is more likely to make the correct turn, as it will be associated with some positive valence. Or we could say: she will better recognize the intersection than before, as it is now a difference that makes a difference. Soon, the valence associated with that intersection will become associated with one before that, and so on, to the point that, over a number of trials, Bianca will be able to read-and-react appropriately, throughout the maze. So, even though the first turn in the maze exists at a very different time and place in Bianca's experience than does the final reward, there is no actual gap that needs to be overcome. The degree to which Bianca is situated in her environment affords her the opportunity to associate things that *do* exist at the same time and space for her, continually backwards, to bridge the apparent gap.

The second hypothesis on offer regards extending the perception of some stimulus through the gap in time so that, functionally, the things associated do exist at the same time and space, within the scope of the organism's experience (Clark & Squire, 1998; Clark et al., 2002; Dehaene, 2014). The evidence given in such studies is that the brain activity consistent with the perception of the initial stimulation does not change up to the point when the unconditioned stimulus is presented, even when the actual stimulus is removed from the environment. This would, theoretically, provide an opportunity for those cells involved to ‘fire’ and ‘wire’ together. In such research, authors talk about this as the role “awareness” or “consciousness” plays in the association; however, such words are not necessary for describing the situation. My personal interpretation of the matter would be that the organism, as a whole, remains in a state of response to the initial stimulation. That said, nothing in my thesis turns on how we interpret this.

Given these hypotheses, to my view, a clean way to test these would be to design a study in the trace conditioning or delay-of-reward style, wherein some amount of background stimulation (white noise, or a different colour light that comes on during the gap) would be treated as an independent variable. Given the chaining hypothesis, one would predict that the background stimulation could serve as a conduit for secondary reinforcement, and would facilitate the ability to form an association, relative to a control group with no background stimulation. From the perspective of the extended perception hypothesis, one would predict background stimulation over the gap would be detrimental to forming an association, as it might interfere with maintaining the stimulus trace. Given such an aim, the next target was to logistically work out how one could run such an experiment with free-ranging magpies.

2.3.4. WHY I DIDN’T DO THIS

There are basically three reasons that I didn’t formally test the available hypotheses—the first being that my literature review of the topic almost conclusively refuted the notion that there was a problem to begin with (At least, it was conclusive enough that I lost interest in the topic, to the effect that I could no longer pursue it with sincerity). In the literature, examples of organisms forming associations over large gaps in time were almost exclusively cases wherein animals were situated in their environment to the effect that they could easily learn the correct response through some version of backwards chaining (For reviews, see Spence, 1947; Renner, 1964; Enquist et al., 2023). In cases where researchers effectively isolated the organism from relevant environmental cues, organisms seem to be essentially unable to form an association over anything longer than a few seconds. Such small gaps never seemed a problem to me to begin with, as an event that follows another by mere seconds can easily maintain the appearance of being connected within the scope of the perceiving organism's experience. This is, there is—or may not—be any apparent lack of contiguity.

Second, the two hypotheses that I describe need not actually be mutually exclusive. As I said regarding the extended perception hypothesis, I think the distinct brain activity is likely to be indicative that the organism is coupled to the stimulation, as if it were responding to it, or ready to respond to it. If, when tested, it was the case that associations were more easily made with background stimulation, this still wouldn't falsify the extended perception hypothesis. Chaining initial stimulation to an extended state of response, and again to the onset of the unconditioned stimulation, may be necessary to form associations over small gaps in time if there isn't some more conspicuous feature of the environment to take on secondary reinforcement. And if the difference in brain activity is just part of a response state, as I suggest, then the extended perception hypothesis is really just chaining anyway, only less conspicuous. While contrasting these two hypotheses would make for a really nice thesis story, it would be an oversimplification—and it would be insincere—to present the research in such a way.

Still, given the two reasons above, I was still going to give the magpies a chance to prove me wrong; I had planned to give them a large gap in time, in a context where they would be properly isolated from anything that could provide *nondifferential* secondary reinforcement, with no hijinks, no background stimulation, and see if they could learn to demonstrate an association. If the magpies could do that, then there would still be something that needed to be explained. It was in pursuing this that we realized that there were antecedent problems that needed to be addressed first.

One of the biggest of these problems that we first encountered was predicting and maintaining the magpies' engagement with the apparatus. For some periods, the magpies attended the feeder in a manner that seemed to follow some sort of schedule, consistent with our designs, and, in other periods, attendance was very poor or entirely absent. Before we could perform our desired experiments in a sufficiently controlled manner, we needed to know why engagement varied. So, the antecedent question became: what predicts magpie engagement with our automated bird feeder? I have broken down the variables of the study into roughly three categories that might start to provide answers: learning, foraging, and weather; however, we will discuss these following a review of the apparatus and method that got us to that point.

CHAPTER 3: METHODS

I find it pertinent to preface this section by saying that the design and construction of the apparatus for my research, and the subsequent study designs that the apparatus affords, would simply not have been possible without my research assistant, Keiran Bodner (KB). KB was paired with me early in the summer of 2022, when I was first tasked to develop a research program with the free-ranging magpies. At that point in time, my goal was still to study how organisms form associations over gaps in time, so I was keen to implement some version of a trace conditioning or delay-of-reward type study design. My visions for how to achieve this with the free-ranging magpies, however, were a fruitless combination of frugal, naive, and uninspired. KB, on the other hand, wanted to implement a more ambitious design. After some discussion, and after KB prematurely invested in much of the required apparatus, we decided to do things his way.

3.1. PROOF OF CONCEPT

Most of the logistical stylings of our apparatus are copied from the work of a Swiss engineer, Hans Forsberg. Hans built an automated bird feeder that he used to train magpies to recycle bottle caps (the feeder and its application can be viewed on Hans' YouTube channel: https://www.youtube.com/@LabbHasse). Hans' feeder consists of a platform where the magpies land, mounted over top of a sensor housing. Above the platform is another box which houses a "hopper" full of food, along with the electronics required to coordinate the apparatus. A tube connected to the funnel in the top box leads to a bowl-type depression in the platform, where the food is dispensed. Directly next to the bowl is a hole that leads to the metal-detector type sensor below (the hole is too small for a bird to fall down).

After having habituated the magpies to the feeder, Hans would place bottle caps near the edge of the hole in the platform. When magpies came for food, they would sometimes inadvertently knock a bottle cap down the hole to the metal detector. The metal detector would then send a signal to the top box, starting a motor attached to the hopper. This would cause the hopper to shake food down into the funnel, thus providing the magpies with more food. A feedback sensor just below the funnel would turn off the motor when food passed by, so that only a discrete amount would be dispensed at a time.

Eventually, the magpies began knocking the bottle caps down the hole to the censor more "deliberately." Hans, then, began to move the bottle caps farther, and farther from the hole over a number of interactions so that the magpies would have to pick up the caps to deposit them. In a last phase of design, Hans scattered the bottle caps around the yard, so the magpies would find and retrieve and deposit them for the food reward.

3.2. A BOX FOR ALL SEASONS

Given the apparent effectiveness of Hans' bird box in both function and application, our goal was to design a similar product that could be adapted to execute either a delay-of-reward or trace conditioning type study. Construction of our own bird box (BB1) took KB and me the better part of the summer; but we were eventually able to create a product that works in a logistically isomorphic way to Hans', albeit with different materials and architecture.

As can be viewed in **figure 1**, the frame for BB1 is re-fashioned out of an old baker's rack. The top box that houses the electronics, batteries, and food hopper, is a large plastic bin that has been weatherproofed. A video camera—also weatherproofed—is mounted to the side of the frame to record interactions at the food bowl. The landing platform is made out of 1" by 4" planks. Cut into one of these planks is the hole leading to the sensor system below. Instead of the metal detector system that Hans' uses for parts of his project, BB1's sensor system—both in the top box and below the landing platform—makes use of break-beam sensors; this is, when an object falls past the sensor, it interrupts a beam transmitted between sender and receiver components; when the beam is broken, the sensor sends a signal to the computer in the top box, which responds accordingly. The plank the hole is cut from is not fixed in place like the others on the landing platform; rather, it snaps into place so that it can be removed and interchanged with a different plank with two holes (see again **figure 1**), as to give the magpies a choice point (the function of which will be discussed later).

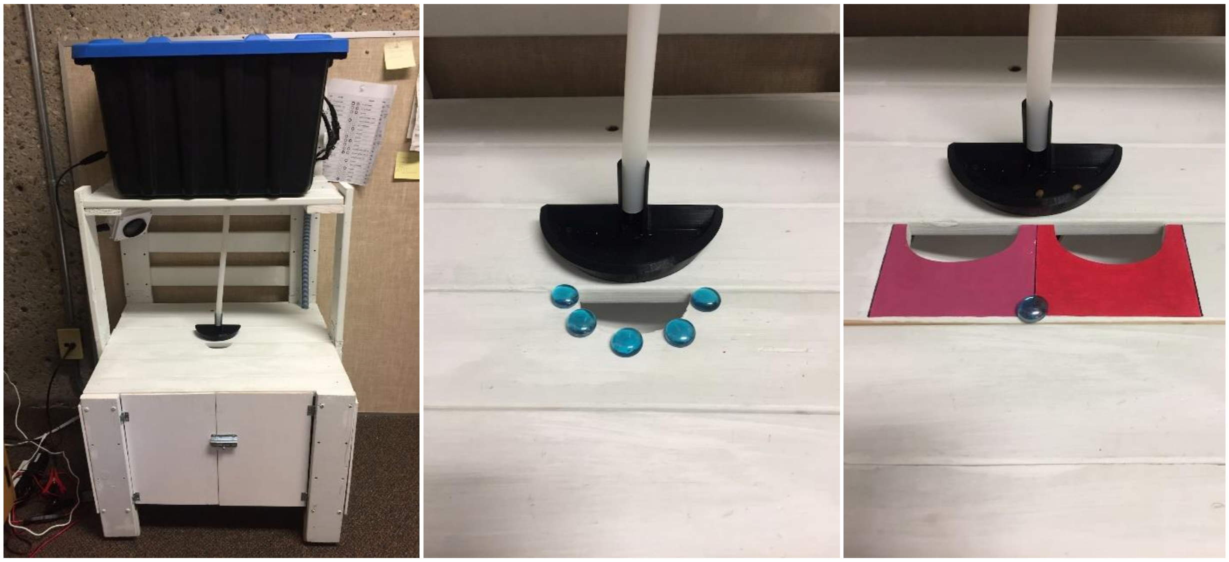

Figure 1: Research Apparatus (BB1): From left to right, we see a portrait of BB1 and what the platform would look like in shaping and testing phases, respectively.

BB1 is entirely battery operated and can run without connection to the internet; nor does it need to be wired into any additional power source. Logistically, this makes BB1 very flexible with respect to where it is set up. We have not tested the top end limit of how long the batteries can last; however, the smaller of the two batteries can run the computer for an entire trial period (between 3.5-4 hours) in -25 degree weather, without dropping below half capacity (informally, I suspect that, during warmer seasons, we could run trials over four or five consecutive days before actually having to recharge the batteries; the car battery that runs the motor could probably go months without recharging). The top box can be disconnected from the frame so the batteries can easily be taken home to be recharged, without having to move the entire frame every day. Moreover, during this time, the video recording from the day can be uploaded to other sources for later analysis.

BB1 seems to operate with little complication in temperatures between -20 and 30 degrees Celsius. There does seem to be a trend of complication in temperatures colder than - 20, however.

When KB moved to Saskatoon part-way through the data collection period, he made a second bird box, BB2. BB2 was made to the same specifications as was BB1; however, subtle differences do exist between them. Though, these differences did not appear consequential, they include improved sound proofing in BB2's top box, as well as BB2's frame missing horizontal back slats to prevent the birds from approaching from all angles.

3.3. STUDY DESIGN

With respect to the original plan, I worked out a method for implementing a delay-of-reward type experiment. The process would be broken down into four stages (described in the following subsections), repeated in two locations with different birds (test and control groups).

BB1 was placed in a back-garden in west Lethbridge. We would run BB1 for approximately four-hour trials, from around 8:00am to 12:00noon on weekdays. This schedule was established in relation to my own convenience, and not in relation to what would have likely been most beneficial for the birds.

When trials were not underway, BB1 was covered with an opaque black cover. This helped establish a discrimination for magpies who had associated BB1 with food. This is not trivial. If we did not cover BB1, magpies might have extinguished interaction with the apparatus during the times that it wasn't dispensing food, as their arrival at the feeder would be continually punished by the absence of the regular reward. This could likely have reduced interaction during the actual testing schedule. Of course, there is the possibility that time might have become a discriminative stimulus for the magpies and that it might not have been overly detrimental to our efforts; however, we opted not to take the risk.

3.3.1. HABITUATION PHASE

The first phase of the study involved habituating the magpies to the feeder—the goal here was to get the magpies interacting with BB1, and to establish a baseline of activity. During this phase, BB1 dispensed food on a fixed-interval schedule, every 15 minutes; although, the use of the term ‘fixed-interval’ should be qualified: The magpie's reception of the food reward (hard cat food, opposed to wet) was not contingent on their behaviour during this phase; they merely needed to show up, and food either would—or would not—be in the bowl, depending on the timing of their arrival relative to the feeder box’s schedule. So, to say that food was dispensed on a ‘fixed-interval’ references only the rate at which reinforcers were available; however, schedules are often defined in reference to the availability of reinforcement, made contingent upon some response (e.g., Ferster & Skinner, 1957). From the birds’ perspective, the schedule presents *almost* more like a variable-ratio schedule. If we imagine the birds coming and landing on the feeder is the target response, they can show up any number of times before food is actually dispensed; however, this is, of course, not really variable-ratio, as in a true variable-ratio schedule, the ratio of responses to reinforcements is under experimental control, and, in the context of this investigation, this obviously is not the case; the birds come and go as they please; and they are not present to experience each and every dispensation (in fact; they were almost never present for dispensations). The problem of naming the reinforcement schedule to which the birds were subject to in our study offers a unique frame into how the typical study of schedules of reinforcement might be complicated by ecologically valid contexts. For the sake of this thesis, I think it is basically fine to refer to the schedule as fixed-interval, and I shall do so hereafter.

During pre-trials, BB1 dispensed a mean of ~ 4 pieces of food per dispensation (SD ~ 1); however, video evidence suggests these numbers went up over the duration of testing (though I didn't formally analyse this).

It might also be worth noting that the first habituation phase seemed to be catalysed by what might be called a pre-habituation phase. The residents of the household had been feeding dog kibble to the magpies and crows that came to the yard. On the first day of running BB1, we made a trail of kibble for them to follow, all the way onto the landing platform. Thus, we had interactions right from the first day. The relevance of this phase was revealed when KB constructed a second bird box, which was located in a garden in Saskatoon. Once this box was operational, we realised the work the pre-habituation phase might have been doing.

3.3.2. SHAPING PHASE

After establishing a baseline level of interaction, the next step was to shape a behaviour on which the dispensation of food could be conditioned. This process, itself, is divided into smaller subphases.

For Hans' magpies, the conditioned response was dropping a bottle cap down the hole on the landing platform. We followed suit; however, given Lethbridge winds, bottle caps might've blown off the landing platform; so, I opted for heavier glass stones, with a flat side (seen in **figure 1**). The phases of shaping the stone dropping behaviour, then, are the same as the bottle-cap dropping previously described with Hans' magpies. We began with several stones placed around the edge of the hole; food would be mixed in around the stones, as well as in the bowl. When magpies went to get food, they would sometimes inadvertently knock a stone into the hole. This would trigger a sensor, and more food would be dispensed.

If the magpie’s behaviour then became more directed towards knocking the stones into the hole, the next step would be to reduce the number of stones to one and position this stone further from the hole. This should then lead the magpie to push or carry the stone to the hole. One would also ideally terminate fixed-interval dispensation, so that the only source of food from BB1 would result from the action of the magpie dropping the stone. For our purposes, we did need to go as far as Hans’ and spread the stones throughout the yard. Getting the birds to push or carry the stones from some point on the landing platform and deposit it in the hole was enough.

During this phase and the next, the experimenter needs to take on a more active role with the box, resetting the stones on the landing platform after they’ve been dropped down the hole, particularly if fixed-interval dispensation is not present. Resetting the stones could be done on any number of schedules: to maximise the opportunities for response, one would reset immediately after an interaction; however, one could do every half hour, or hour, as the prior might not be logistically possible.

3.3.3. TEST PHASE

Given sufficient demonstration of a shaped stone dropping response, the next step was to move to a delay-of reward test phase. This involved two amendments to the logistics of the late shaping phase. First, the magpies would be given a choice between two holes: one is coloured red, and the other purple (see right **figure 1**). Distinct sensors would correspond to each hole. Dropping a stone past one of the sensors would do nothing. The other sensor would dispense food, but only after a delay (say fifteen seconds).

Similar to our previous example about Margo and the lights, we would pick one colour to code for a correct response, and the other an incorrect response. If we wanted our magpies to drop a stone down the purple hole, we would code the sensor below the purple hole to the delay timer, and the sensor below the red one to do nothing. The red-purple board is inversely painted on its other side, so it can be snapped out of place, flipped, and the holes can switch sides between trials to counteract orientation effects (when one does this, one also has to rotate the sensors below accordingly, so the same sensor always corresponds to the same colour).

Given this method, one then verifies over a number of trials whether the magpies come to learn the correct colour. That is, would magpies come to favour the purple hole above that expected by chance, even if the reward was given only after the delay, or would they continue to drop the stone down either hole at random? We would then compare this to a control group (from a different location) who would be given the same choice, but without a delay.

3.3.4. REVERSAL LEARNING PHASE

If the magpies proved able to learn a preference for the correct response, one could move onto a reversal learning phase. This would entail switching the colour of the correct response, and tracking the learning curves that relate to how long the magpies take to adjust to the new contingencies.

3.4. WHAT WE HAVE ACTUALLY DONE

For several reasons, mentioned previously, the plan outlined above had to be substituted for a different approach. In practice, we never made it through the shaping phase (see discussion), and *almost all of our data comes from running BB1 and BB2 on permanent fixed interval schedules* (like in the habituation phase). As mentioned, the variable of interest while running our bird feeders in this condition became the *number of interactions we would record on a given day*: a simple count measure, *operationally defined as each instance a bird would land on the landing platform.* Each interaction would be labelled categorically based on whether the landing bird received food, during its time on the platform. If the bird received food, the interaction was labelled a *true* interaction. If the bird did not receive food, if the bowl was empty or if something deterred the bird from getting the food was there, the interaction was labelled *false*. The labels *true* and *false*, in retrospect, might carry unnecessary connotations; however, they were only ever intended as shorthand for *rewarded* and *unrewarded* responses, respectively.

Moreover, the actual number of interactions on a day were usually substituted for an adjusted measure of daily interactions in both descriptive and inferential analyses. *Adjusted daily interactions* is a derivation of the actual daily interactions, wherein the number of interactions has been standardized to a four-hour trial runtime. This was to remove the potential confound of shorter or longer runtimes on certain trials, particularly in the Lethbridge location, wherein, over the year of data collection, there was a trend in trials being run for shorter, and shorter periods of time. When viewing the figures in the result section, the prefacing word “adjusted” is used to denote those cases wherein the adjusted numbers were used for the graphical representation.

Before considering the list of independent variables tracked in the study, it first pertinent to give special attention to the variable of location, as functionally meaningful differences existed between the contexts of our two locations that will inform later discussion: At the Saskatoon site, a five-gallon dog feeder provided a continuous supply of dog kibble for the local corvids. Inasmuch, the population of birds that frequented to Saskatoon yard-space was much larger than that of our Lethbridge site. We estimate the number of birds at the Saskatoon site to be between 25-35 at a time, compared to the 2-7 that would come visit the Lethbridge site.

Variables that I thought might affect level of interaction were broken down into three non-exclusive categories: learning, foraging, and general ecological factors.

- ***Learning***
    - *Trial*: a count measure of how many days the birds have been presented with the same procedure. Day 1 equals trail 1, day two, trial 2, and so on.
    - *Trial Block*: a measure of how many weeks the birds have been presented with the same procedure. Week one equal trail block 1, and so on.
    - *Inter-Interaction Interval* or *III*: The spectacularly named inter-interaction interval is the continuous measure of time between interactions (in seconds), measured from the end of the previous interaction to the onset of the current one. This is the second dependent variable of interest with respect to learning, in addition to our main dependent variable, daily interactions.
    - *Interaction Length*: The duration in seconds of a given interaction.
    - *Dispensation Schedule*: The rate at which a feeder dispenses food, given as an ordered factor (ordinal variable), with levels including *none* (no dispensations), *low* (~ 1 dispensation an hour), *medium* (~ 2 dispensations an hour), *high* (~ 4 dispensations an hour), and *bulk* (when a mass amount of food was continuously in the food bowl).
    - *Phase*: The current context of the given trial (categorical): e.g., habituation, shaping, or test.
- ***Foraging***
    - *Food Factor*: a binary variable that signifies whether additional known sources of food—over and above "natural" foraging opportunities—are available to the birds

    on a given day. After shifting from our original plan relating to the delay-of-reward study, this became the only real variable that can be called a ‘manipulated’ variable, as the experimenter can change the availability of alternative, local food sources, and implement a proper ABAB repeated measure procedure to get a better notion of causality than with the other correlative variables (more on this later).

  - *Consecutive Days in Trial Block*: How many days in a row has the box been run in its current trial block. As the BB1 and 2 were run weekdays, this measure is ordinal ranging between 1 and 5. There could, however, be repeated integers on a week if days were missed during the trial block. For instance, if data was not collected on Tuesday, as was the case on some Tuesdays when I taught labs and couldn’t run the BB1, the days for data collection on the trial block might have values of ‘1, 1, 2, 3,’ for their consecutive days on trial block score.
  - *Days Since Last Trial*: The number of days since the last trial was run.
- ***General Ecological Factors***
  - *Month*: the month the data was recorded in—categorical, used for comparisons of foraging rate between seasons.
  - *Temperature*: a continuous measure of the average temperature (degrees Celsius) from the four-hour span that data was collected from on a given day. This data comes from https://www.timeanddate.com/weather/canada/lethbridge/historic
  - *Wind Speed*: a continuous measure of the average wind speed (km/h) from the four-hour span that data was collected from on a given day. Data from the above site.

    - *Precipitation*: a categorical value of weather precipitation occurred during the four-hour span of data collection on a given day. Options include ‘none’, ‘rain’, and ‘snow’.
- ***Round***: Round is a unique variable unto itself used for organisational purposes during the investigation. As mentioned, test periods were run on weekdays, and not weekends, giving a typical five-on-two-off study structure. There were, however, a few instances when large breaks were taken in data collection—breaks of two weeks or longer. These breaks are what I have used to organise the data into what I’ve called rounds. In total, data collection was divided into four distinct rounds: round 1, spanning October 5, 2022, to December 1, 2022; round 2: January 9, 2023, to April 27, 2023; round 3: May 30, 2023 to August 25, 2023. These first three rounds contain the total of the data collected from our Lethbridge location, using BB1. Round 4 data was collected by KB, using BB2 in Saskatoon, May 10, 2023, to August 25, 2023. Each round took on a distinct flavour, as can be shown in **figures 2** and **3** in the following chapter; and, when building descriptive plots that regard our other variables, it was often useful to limit analyses to a certain round, or set of rounds, depending on what factors needed to be accounted for. The logic of this will hopefully become more apparent when we discuss the context of the results in the discussion.

3.5. DATA COLLECTION AND INDIVIDUAL IDENTIFACTION

As mentioned, a single camera was mounted on each of BB1 and 2 to continuously record interactions over trial periods. These cameras recorded a limited field of view, including the landing platform and a small amount of surrounding setting.

After every trial, the recording was uploaded to a private channel on the video collection website, YouTube (and separately uploaded to a backup hard drive). Every video uploaded was titled with the date and location of the recording. Every day, we would sift through the new recordings, marking the advent and end of each interaction on separate word processor documents, which we referred to as time sheets. No tests of interrater reliability were conducted regarding recording daily interactions from the videos. The videos were then edited to more viewable time lengths containing only the interactions on the day, including one second before and after each. These edited videos were then also added to the hard drive. From the time sheets, data was entered into two separate spreadsheets: one spreadsheet looked at each interaction as a new row; the second looked at each trial as a new row (each is also backed up on the separate hard drive). In sum, for every day of data collection, the material products include the raw footage, the edited footage, and the derived data at three levels of refinement: the time sheets, the trial spreadsheet, and the interaction spreadsheet: All data sets are available upon request.

There was *no* formal method of individual identification of birds used during the study. Individual identification of the birds is not a variable included in any analysis in the study. Informally, in the many (many) hours spent recording interactions from the raw footage, I feel as if I got a sense of who one (or maybe two) of the magpies was (were) that regularly visited the Lethbridge site. Inasmuch, discussion of the magpie 'FR' will feature later in the discussion.

CHAPTER 4: RESULTS

Across rounds, there were a total of 220 trials (158 from Lethbridge; 62 from Saskatoon), and 1957 total interactions (1215 magpies; 742 crows; we also had eight grackle interactions, several mouse interactions, and some sparrows that have all been excluded from analysis).

Table 1: General Results

Location	*Species*	*Total Interactions*	*Mean Daily Interactions*	*SD Daily Interactions*	*Max Daily Interactions*
Lethbridge	*Magpies*	686	4.34	4.13	19
	Crows	526	3.33	7.51	45
Saskatoon	*Magpies*	529	8.53	8.11	45
	Crows	216	3.48	7.09	53
Total	*Magpies*	1215	5.52	5.84	45
	Crows	742	3.37	7.37	53
	Both	1957	8.90	9.64	78

It might be noted that a considerable portion of our interactions came from crows, not magpies; given the high overlap of these species' ecological niches, this should have been expected; however, it was not. Crows appeared at the advent of the third round of data collection in Lethbridge (they did not appear in the first two rounds), and they were there from the beginning of the Saskatoon data collection, as well (round 4).

The daily interactions by round can be viewed in below in **figures 2** and **3**.

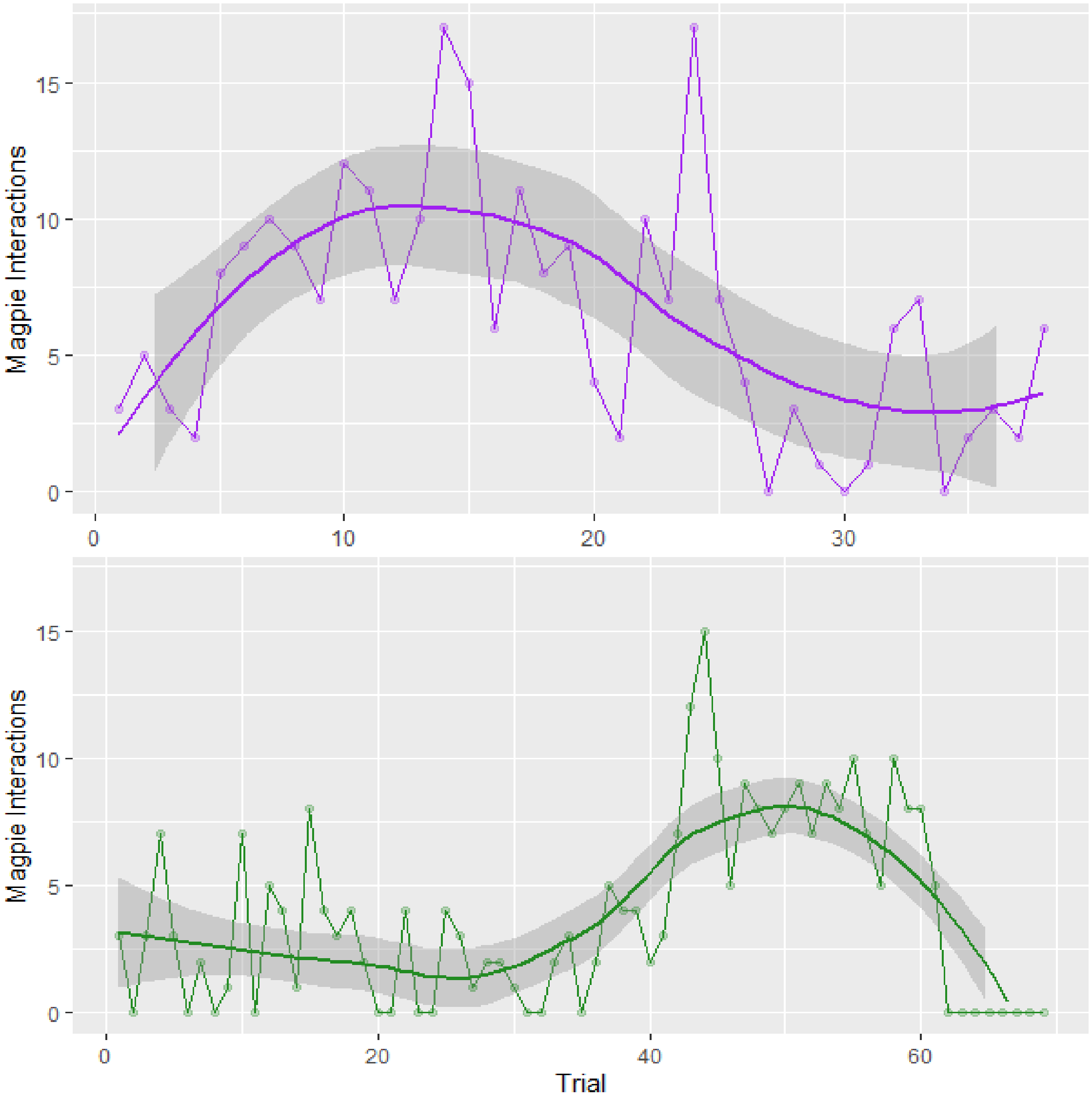

Figure 2: Daily Magpie Interactions by Trial: Rounds 1 (Top) and 2 (Bottom): The fitted line and standard error are produced by ggplot's 'geom_smooth' default smoothing function. The same is true for all line plots hereafter, with a couple exceptions that will be noted.

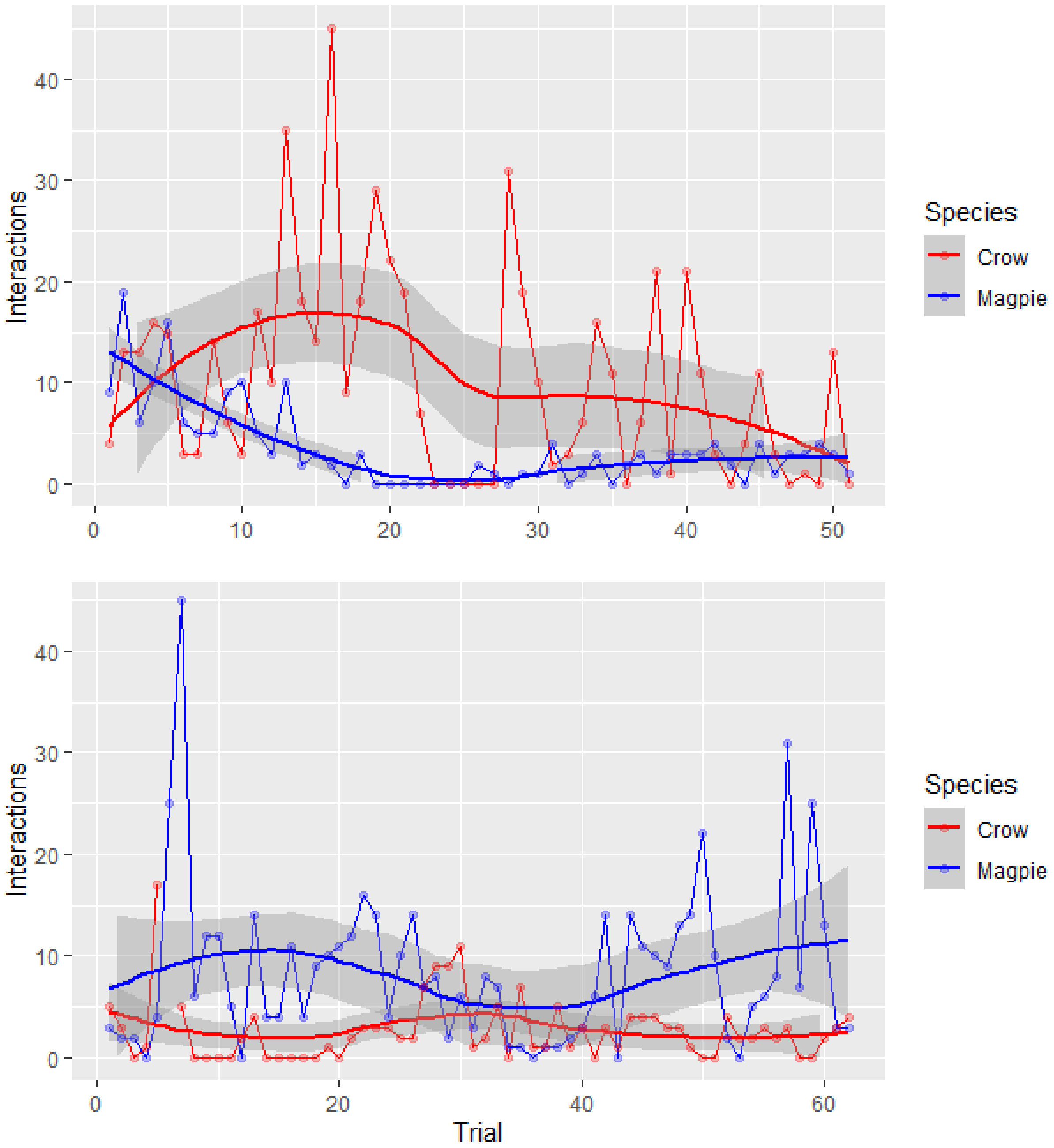

Figure 3: Daily Interactions by Trial: Rounds 3 (Top) and 4 (Bottom)

As can be seen in the above figures, each of our four rounds took on a distinct flavour. One of the most notable differences between rounds observed was the inversion of relative responses of the magpies and crows between locations (**figure 3**): when crows arrived at the Lethbridge site in the third round, they subsumed the majority of responses, whereas, in Saskatoon, the majority of daily responses came from magpies.

Given some background on some of the general results of our investigation, we may now turn to looking at how daily interactions related to our discussed independent variables. As mentioned, independent variables have been organised into three groups: general ecological factors, foraging related factors, and learning related factors. Obviously, these groups are not-mutually exclusive, and are somewhat arbitrary; however, they will hopefully give us some semblance of direction as we survey the descriptive landscape of the investigation.

4.1. GENERAL ECOLOGICAL FACTORS

Location: The first of the general ecological factors to consider are some of the differences in outcomes between our two locations: Lethbridge and Saskatoon.

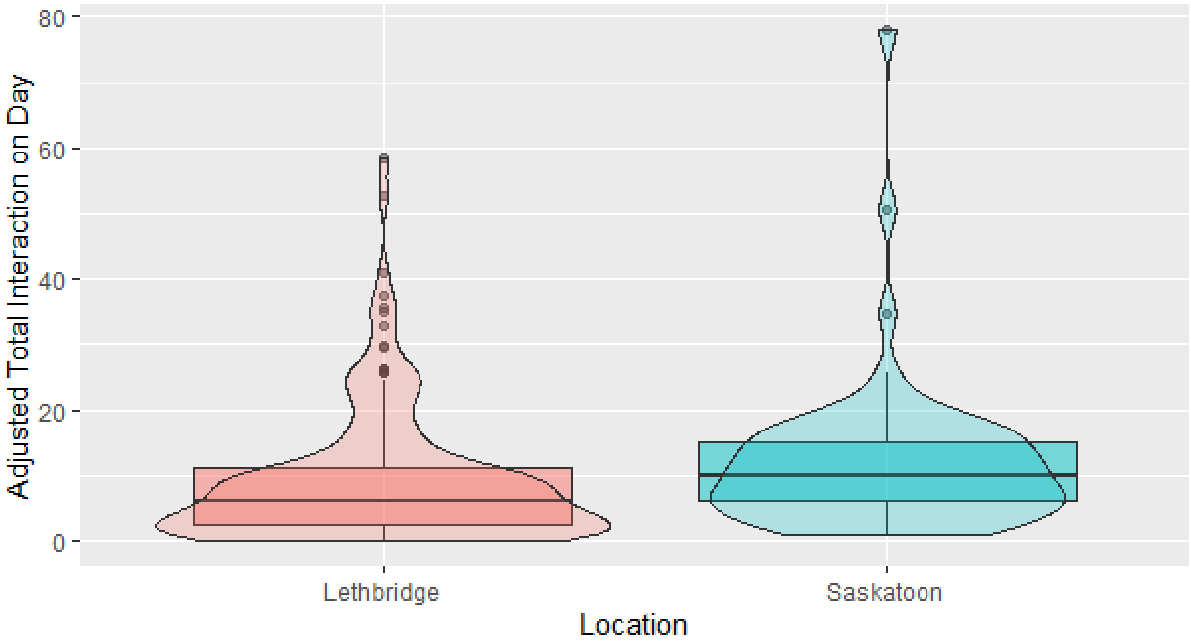

Figure 4: Adjusted Daily Interactions by Location: boxplots and whiskers are produced by ggplot's default 'geom_box' function. The same is true for all boxplots presented hereafter.

Consistent with **table 3** we see that, on average, Saskatoon had more daily interactions than did Lethbridge (7.5 versus 4.6, respectively); however, consistent with what is observed in **figure 4**, the relative amounts of magpie and crow interactions are inverted between locations (**figure 5**).

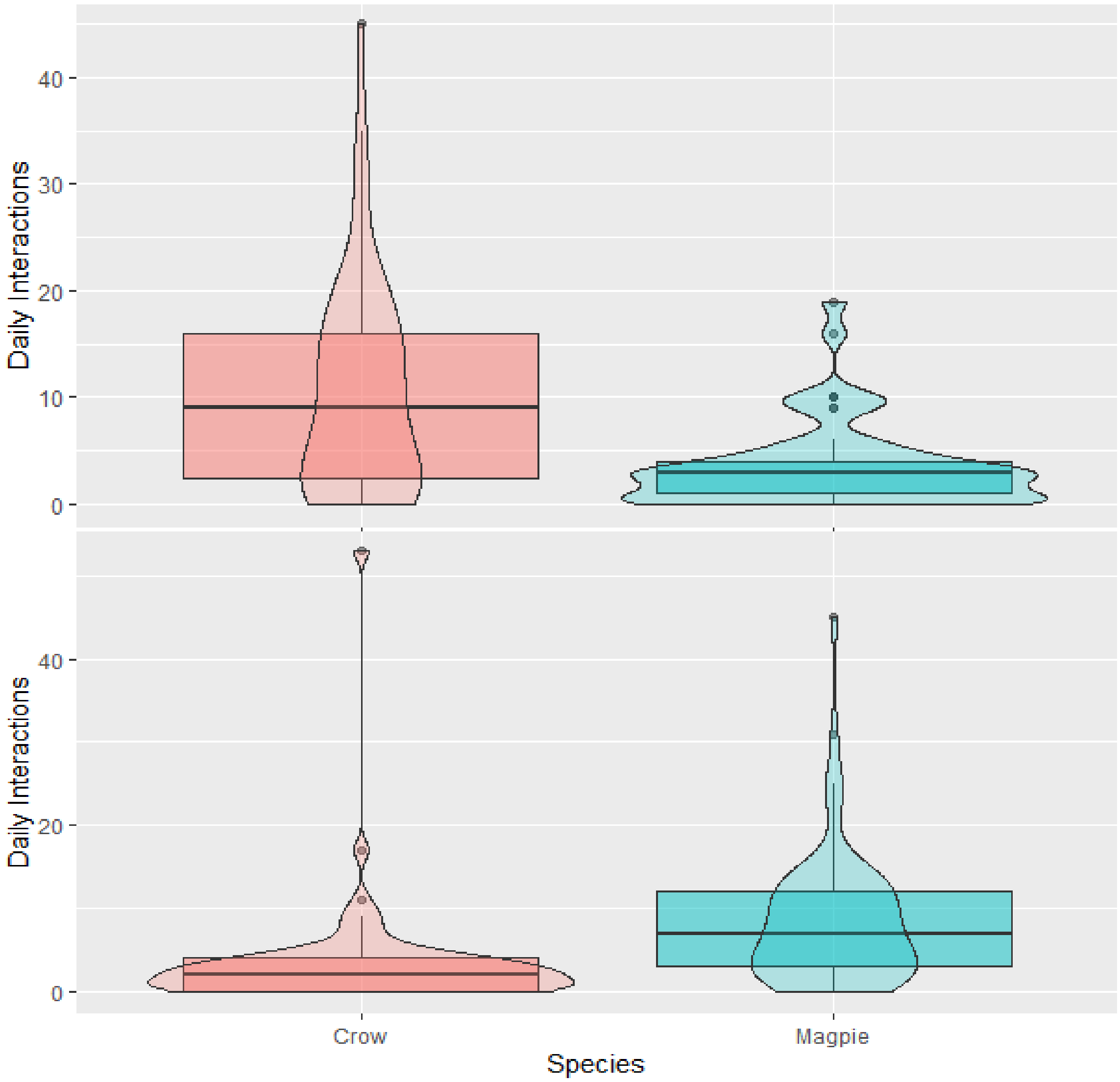

Figure 5: Daily Interactions by Species and Location: Lethbridge (Top), Saskatoon (Bottom)

Month: Figures 6-8 show how daily interactions changed in relation to change in month, and how interactions changed in relation to the magpies breeding season.

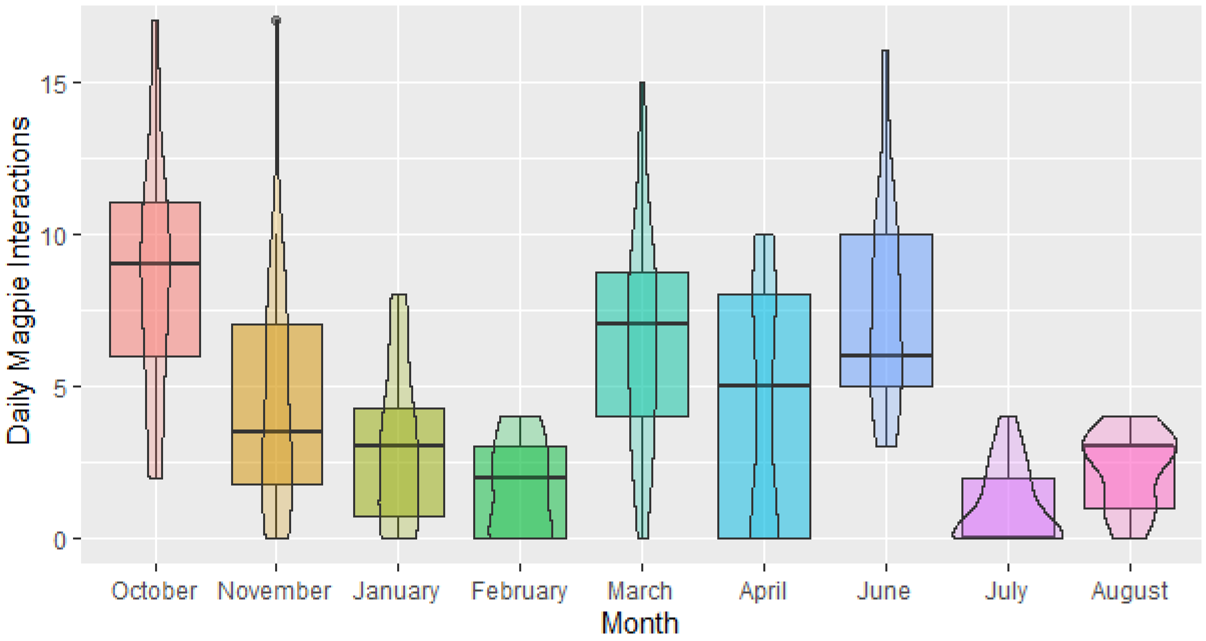

Figure 6: Lethbridge Magpie Interactions by Month: Decemeber and May each had fewer than three trials, and so have been excluded from the plot.

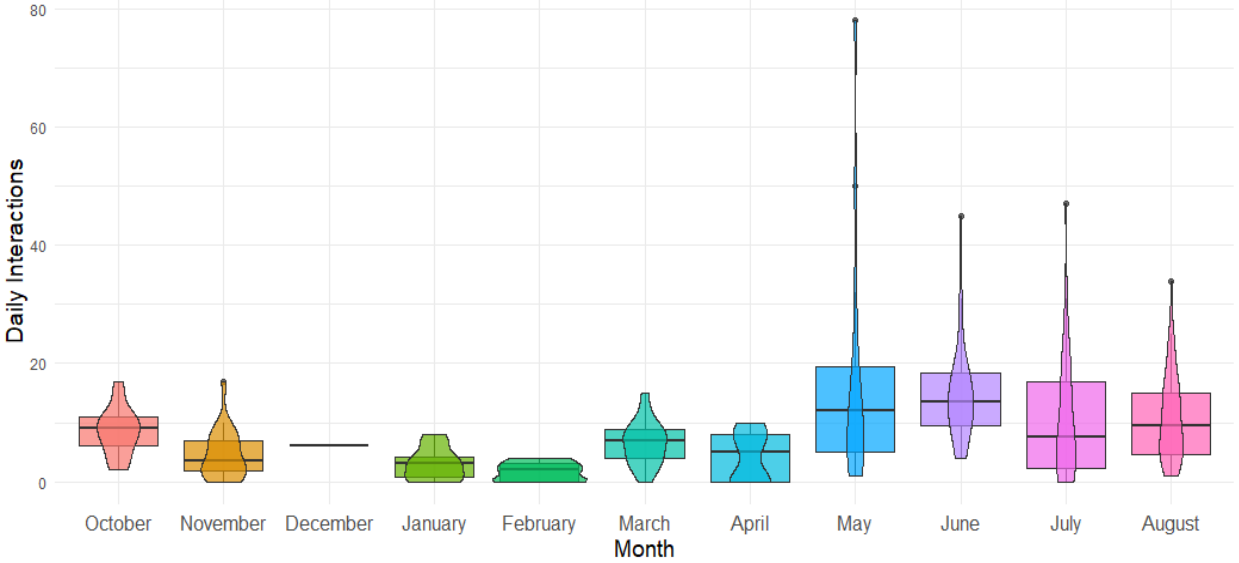

Figure 7: Daily Interactions by Month: Including data from both locations and species.

We can observe that in the Lethbridge location (**figure 6**), magpie interaction decreases from June to July, and remained low in August; however, when we compare this to the total interactions over the months between both locations and species (**figure 7**), the summer months have the highest relative levels of interaction.

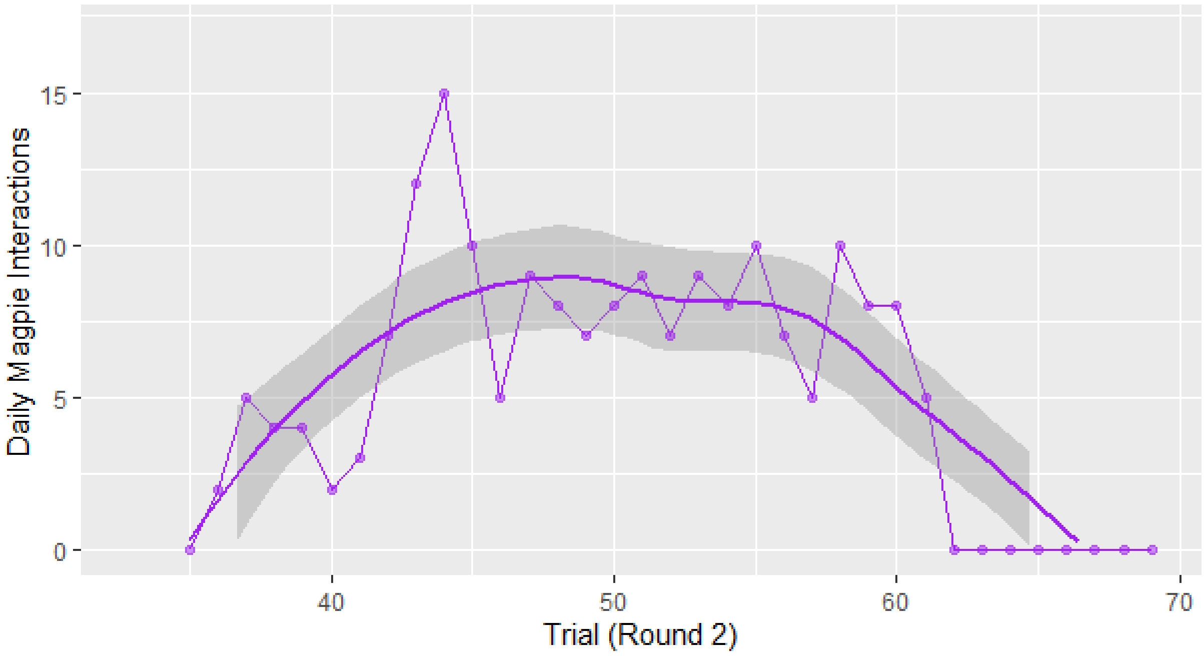

Figure 8: Daily Springtime Magpie Interaction: Lethbridge

In **figure 8**, we can see a decrease in activity during the end of our second round of data collection (around the 60th trial), associated with the advent of magpie breeding season.

Temperature and Wind Speed: Superficially, there appears to be a positive correlation between temperature and the number of interactions on the day (**figure 9**); however, this relationship is confounded by, minimally, two factors.

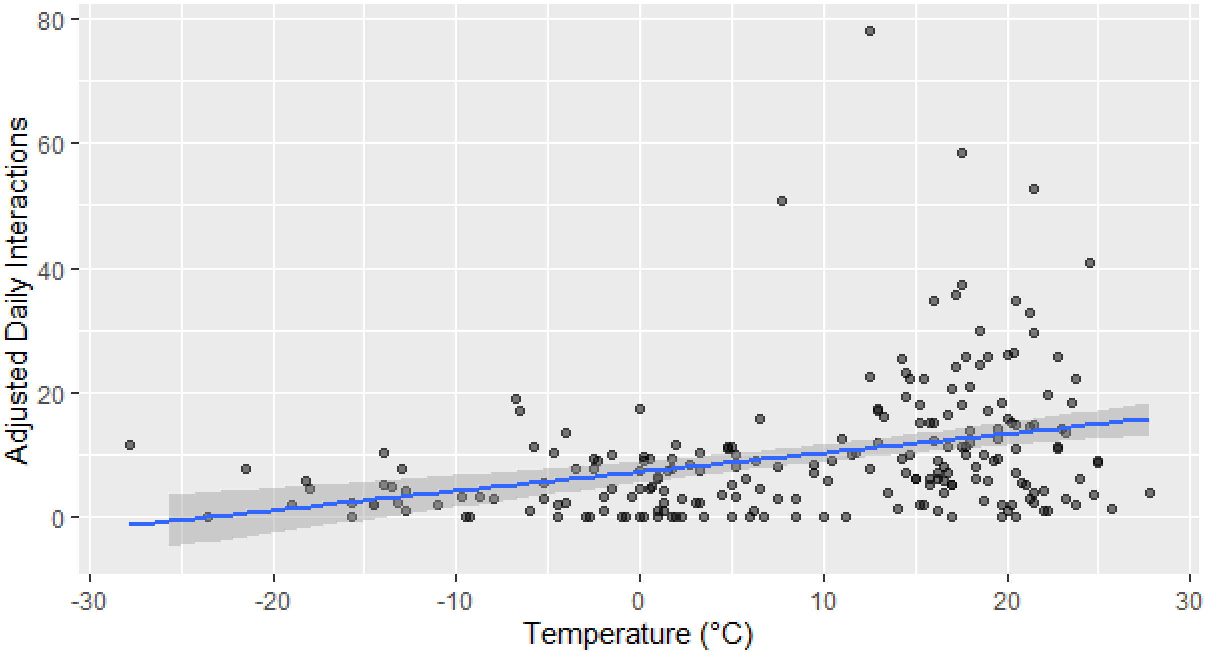

Figure 9: Adjusted daily interactions by temperature: Regression line and standard error produced by ggplot’s default ‘geom_line’ linear model function.

First, we know that, on average, our Saskatoon data was recorded during the months of summer and late spring when temperatures are warmer; moreover, we know that the level of interaction from our Saskatoon sample was not comparable to that of our Lethbridge location: Saskatoon had a higher average rate of interaction than did Lethbridge. Second, we know that—in our Lethbridge location—interactions were inflated in the summer by the presence of the crows. If one controls for both these factors, looking at only the relationship between adjusted magpie interactions from the first two rounds of Lethbridge data, we observe much less of a relationship (**figure 10**).

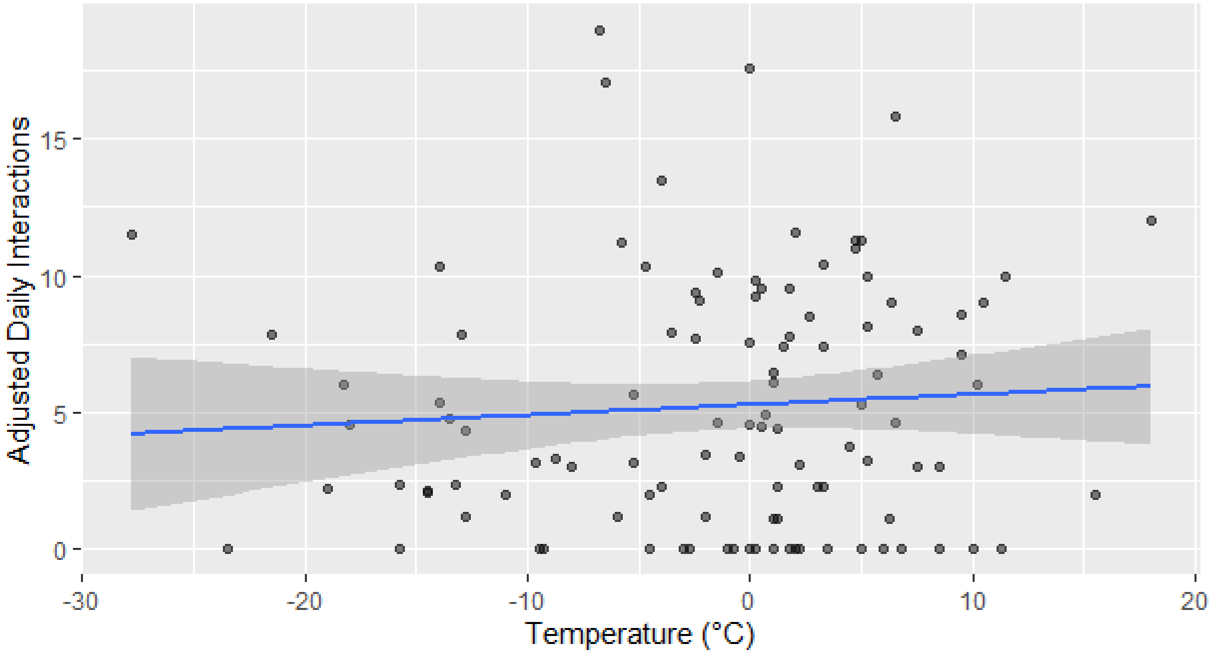

Figure 10: Adjusted Daily Interactions by Temperature in Lethbridge: Rounds 1 & 2

Fortunately, unlike temperature, wind is not assumed to vary in tandem with season; so, it seemed reasonable to include all our data when plotting the descriptive relationship between wind and level of interaction. This relationship can be viewed in **figure 11** below.

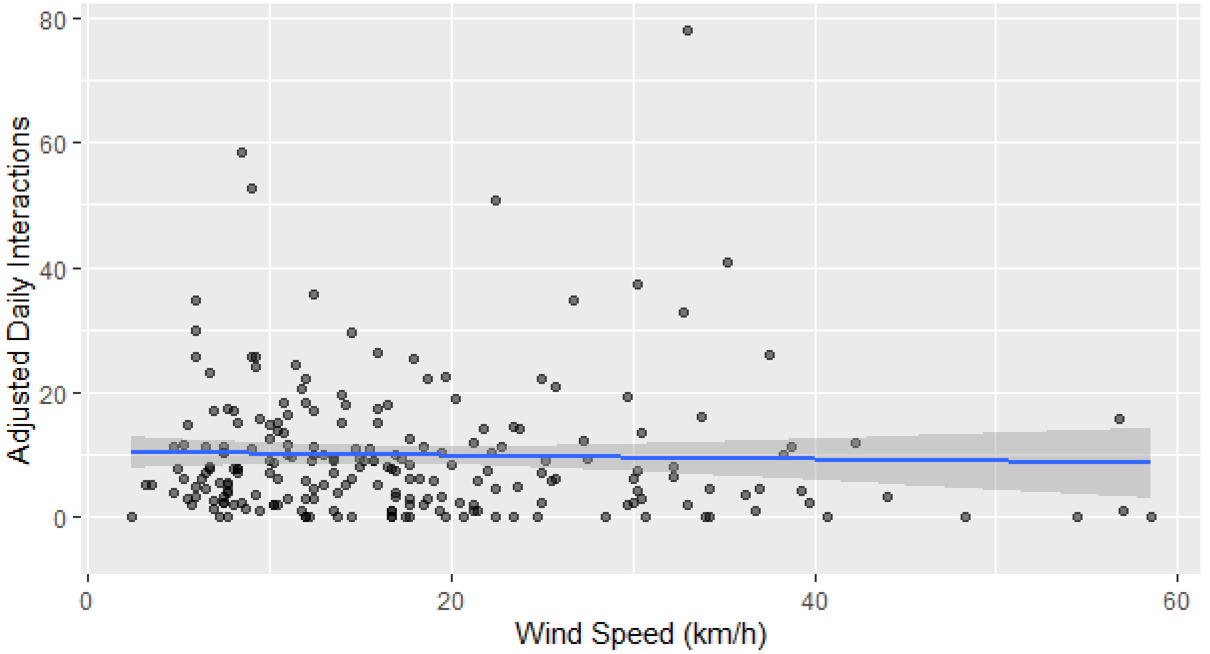

Figure 11: Adjusted Daily Interaction by Wind Speed

4.2. FORAGING RELATED FACTORS

Day of the Week and Consecutive Days in Trial Block: To assess whether the amount of food collected on the week would have an affect level of interaction, we assessed two proxy markers: first, simply the day of the week, seen in **figure 12** below.

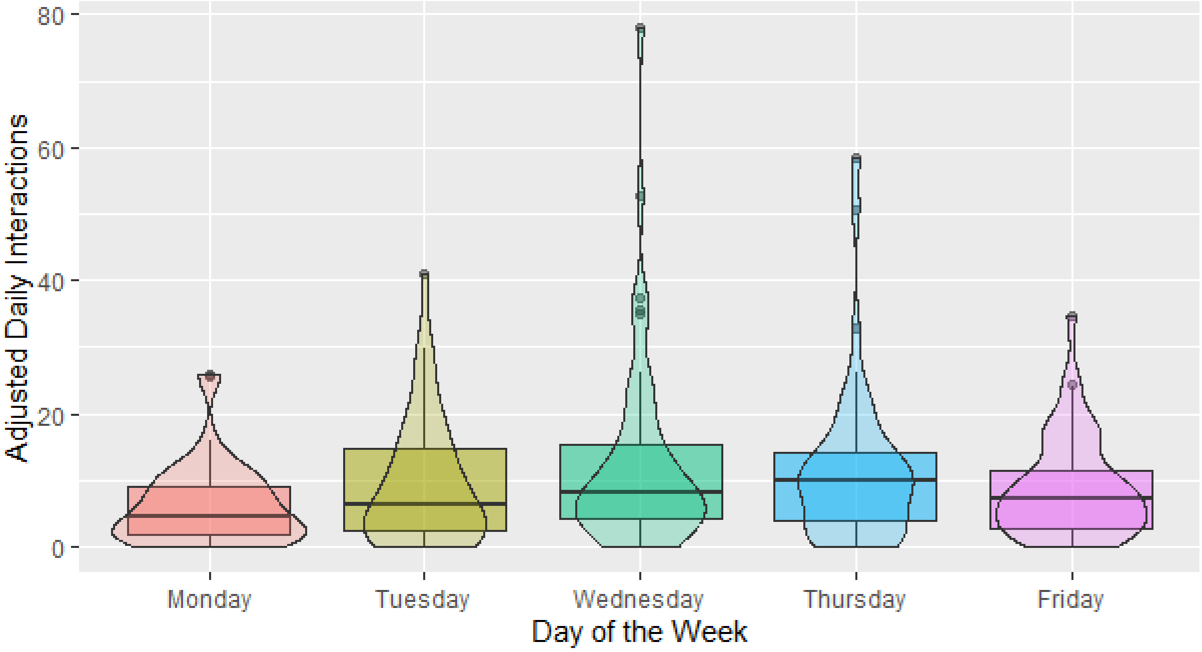

Figure 12: Adjusted Daily Interaction by Day of the Week

A few observations might be made with respect to **figure 12**. First, the spread of the data within each day appears roughly similar between days—they are all rather bottom heavy, which is not surprising, as the overall distribution of interactions is similarly skewed. Second, though the average level of interaction between each day is rather close, there is an apparent arch in the level of interaction throughout the week. This is, on average, we see fewer interactions on Monday and Friday than we do in the middle of week. This general arching shape in weekly engagement is consistent when viewing subsamples of the data, as well (like when comparing Lethbridge and Saskatoon interactions).

Still, it should be noted that the ‘Day of the Week’ variable did not always correspond to consecutive days of running the feeders, and certain days have substantially fewer trials recorded than do others. For instance, in my first semester of running BB1, I would teach on Tuesdays during the time BB1 would normally be taken down, and so I refrained from recording data on these days. To correct this, we can look at the ‘Consecutive Days in a Trial Block’, which ascribes a count value to each consecutive trial on a given week of data collection (**figure 13**).

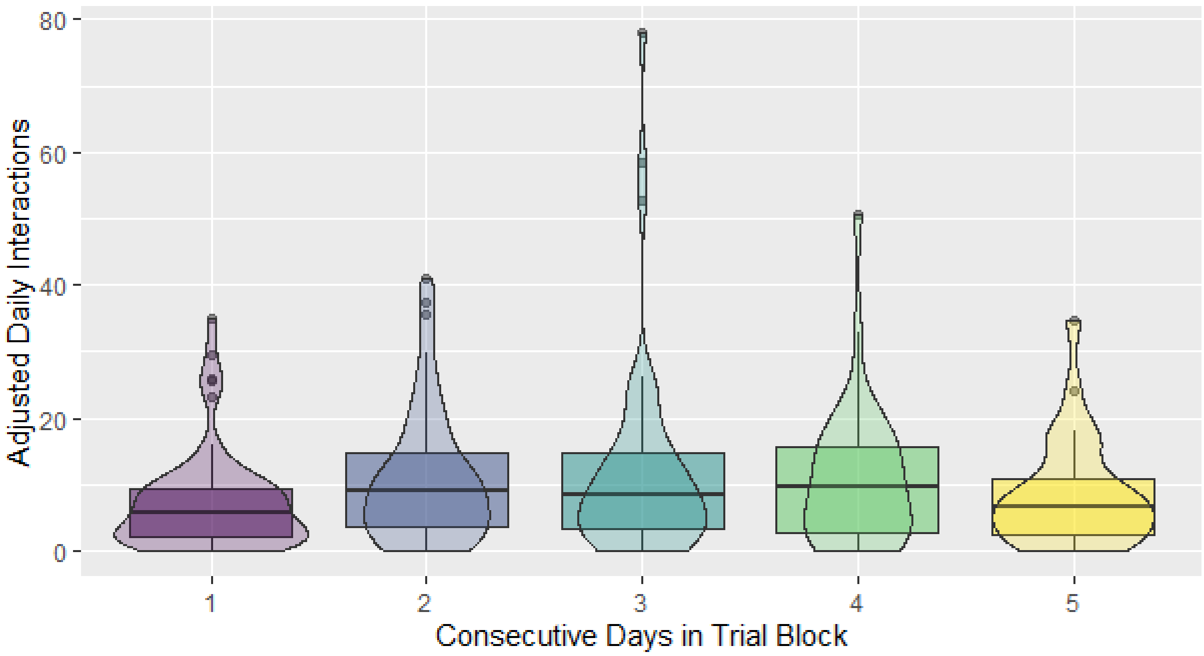

Figure 13: Adjusted Daily Interaction by Consecutive Days in Trial Block

Days Since Last Trial: To assess the possibility that the magpies engage less on the first day after a break because of a readjustment to a change in contingency, we can see how daily interactions change in relation to how long it has been since the last trial. In **figure 14** below, we can see a slight trend in that interactions seem to decrease the more days it has been since the latest trial.

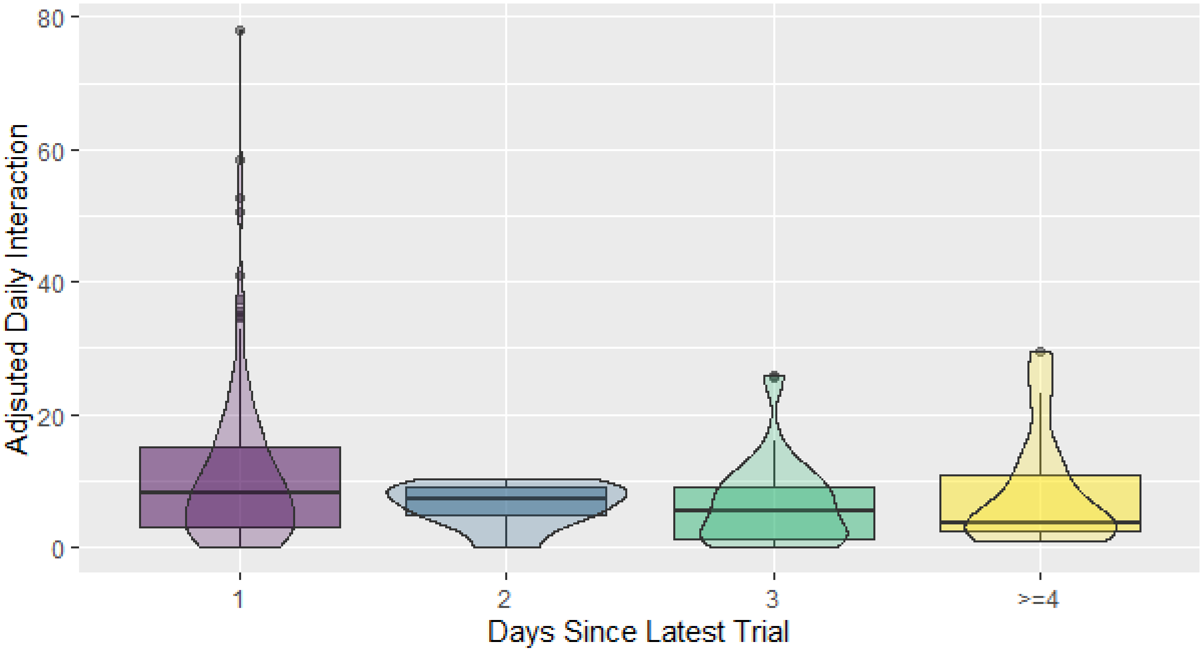

Figure 14: Adjusted Daily Interaction by Days Since Last Trial

Food Factor: For foraging related factors, we might consider how differences in the availability of local food sources—in addition to the experimental feeders—affect the rate responses to our feeders, within a location. As has been the case for several of our variables, I did not make predictions about how the availability of alternative food sources within the yard space might affect daily interaction counts; and we will consider the complexities of this variable and its interrelations again in the discussion. Whether or not alternative food sources were provided to the corvids within the yard sites can be seen below in **figures 15** and **16**.

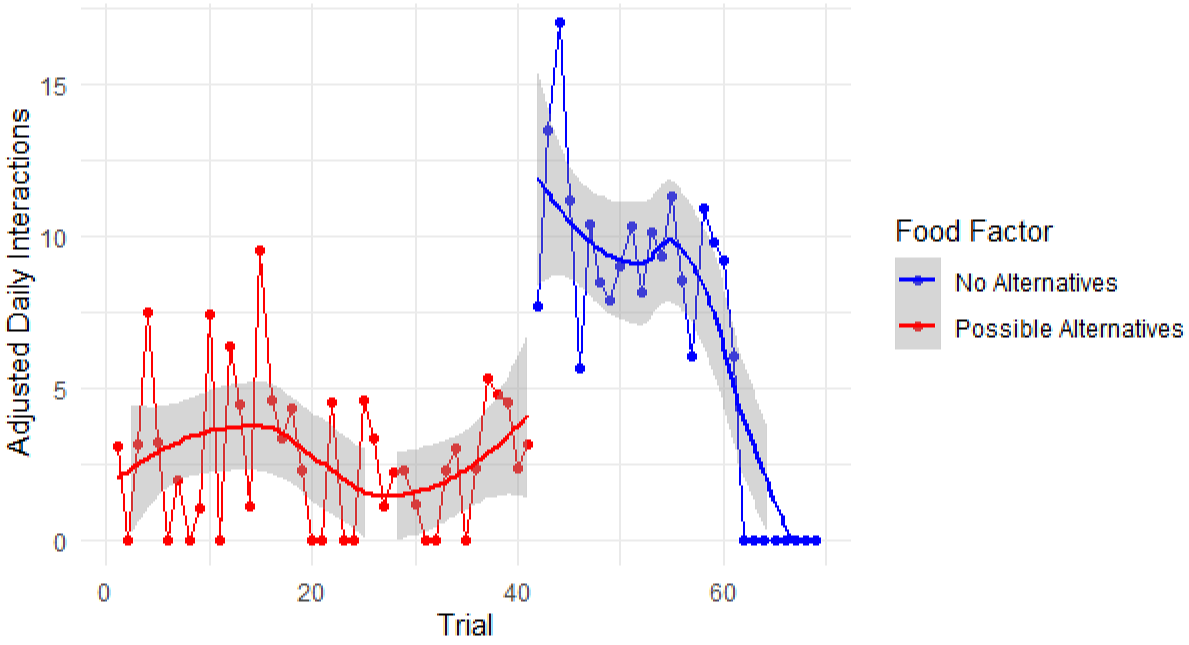

Figure 15: Adjusted Daily Interaction by Trial and Food Factor: Round 2

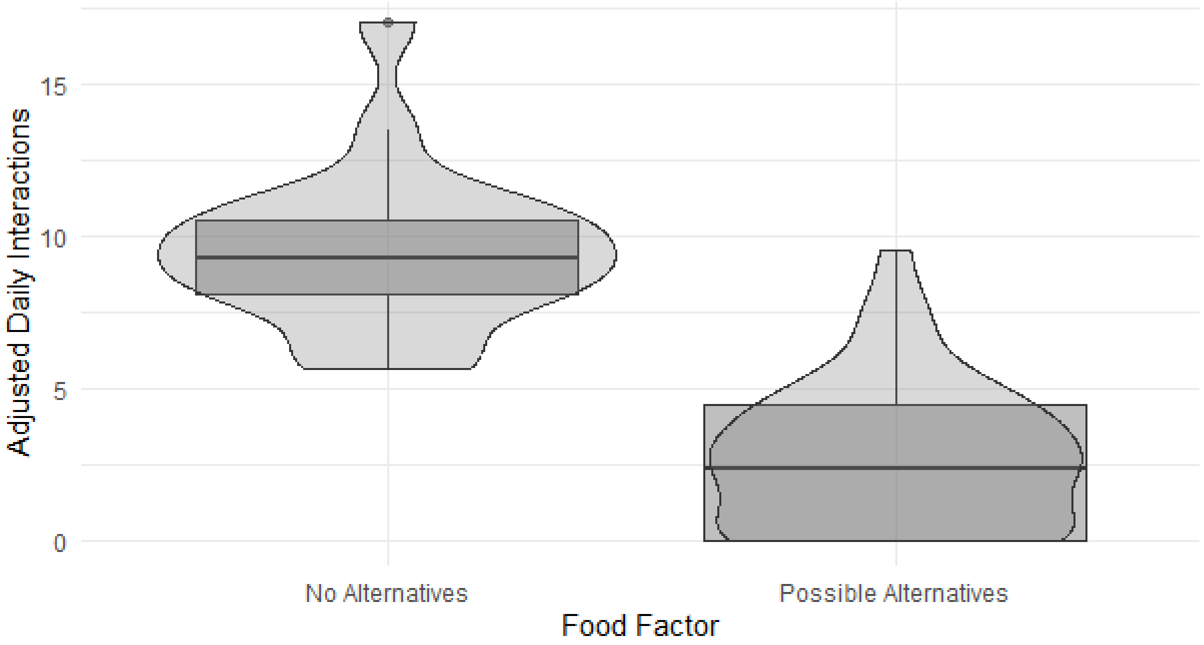

Figure 16: Adjusted Daily Interaction by Food Factor: Round 2

Attempts to replicate results from the Lethbridge location in Saskatoon by removing the aforementioned dog feeder can be seen in **figure 17**, wherein the presence of the dog-feeder constitutes a preferred, alternative foraging opportunity (Possible Alternatives), and its removal constitutes the ‘No Alternatives’ condition, with a new intermediate condition, ‘Some Alternatives’.

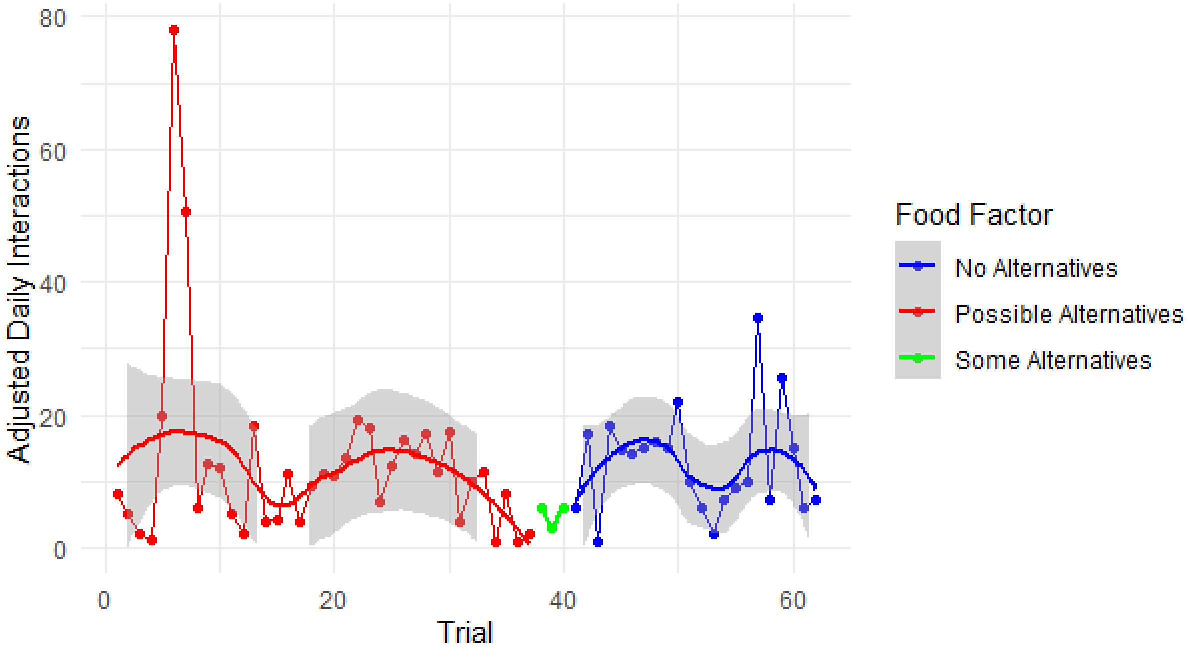

Figure 17: Adjusted Daily Interaction by Trial and Food Factor: Saskatoon:

Dispensation Schedule: We can conclude our section on foraging by considering how the daily interactions change in relation to the feeders’ dispensation schedule (**figure 18**).

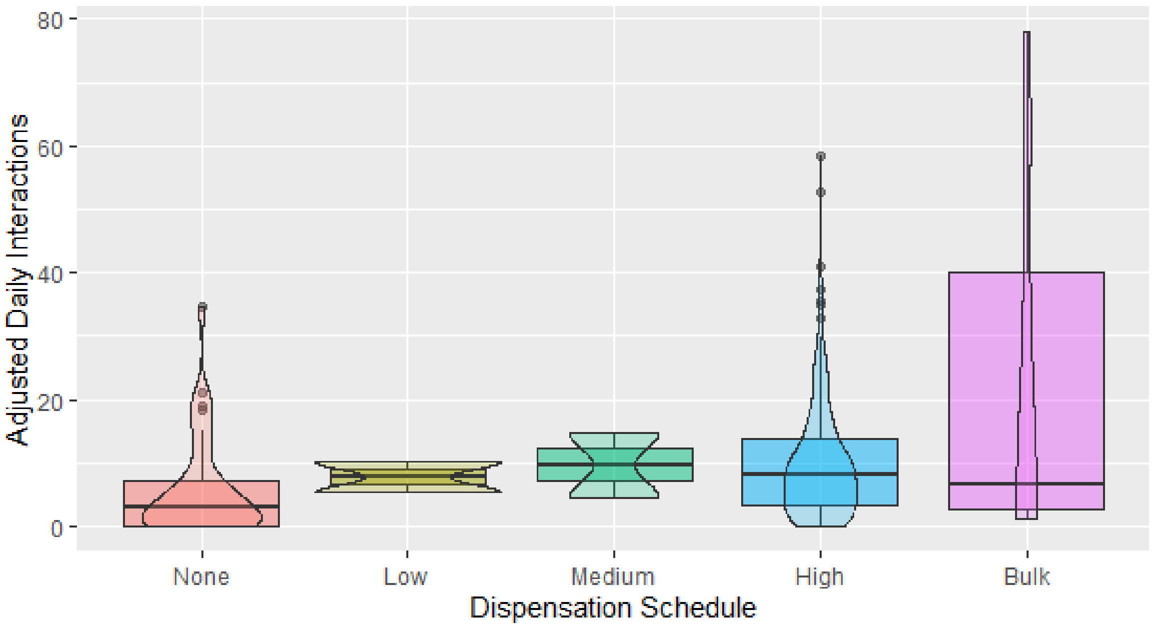

Figure 18: Adjusted Daily Interaction by Dispensation Schedule: none = no food dispensed on trial; low ~ 1 dispensation per hour; medium ~ 2 dispensations per hour; high ~ 4 dispensations per hour; bulk is on mass serving that lasts continuously over the trial.

From the above we can see a slight positive relationship: interactions increase with an increase in food dispensation.

4.3. LEARNING RELATED FACTORS

Learning Curves: Learning curves that will be used for discussion have already been presented in **figures 2** and **3**. Additionally, in attempt to see if the decrease in magpie interaction in Lethbridge during round three was a consequence of a) the crows taking the regularly dispensed food, thus decreasing the number of possible rewarded interactions for the magpies, or b) a consequence of the crows, in and of themselves, I plotted the proportion of rewarded (true) magpie interactions across the trials for round three (**figure 19**).

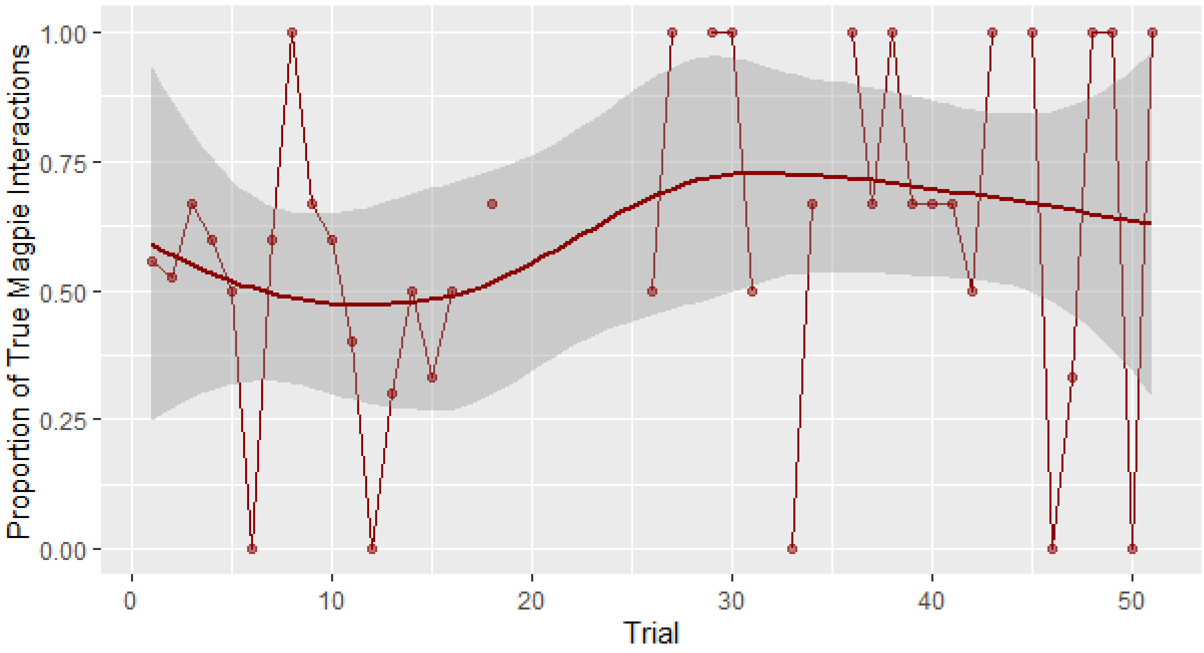

Figure 19: Proportion of True Magpie Responses by Trial: Round 3

Inter-Interaction Intervals: As mentioned, the inter-interaction interval is a measure of the amount of time between subjects' interactions with the feeders. I wanted to use this variable to see if the magpies would ‘tune in’ to the usual fifteen-minute dispensation interval. The average daily Inter-interaction intervals can be seen below in **figures 20** and **21** for rounds 1 and 2, respectively.

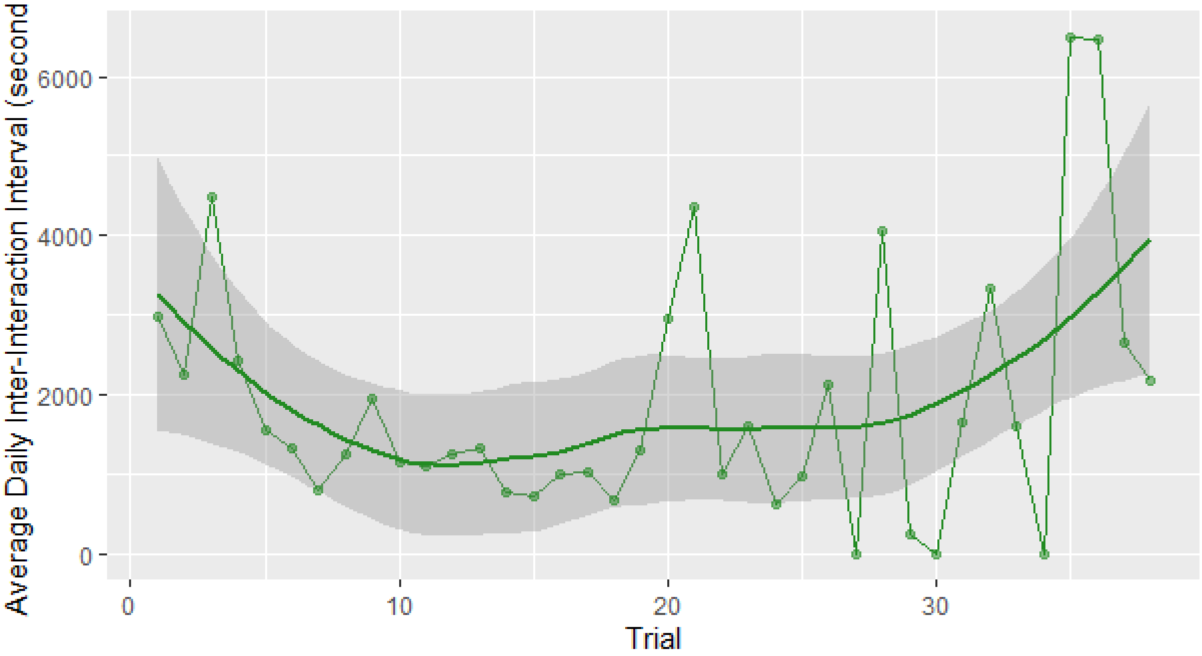

Figure 20: Average Inter-Interaction Interval by Trial: Round 1

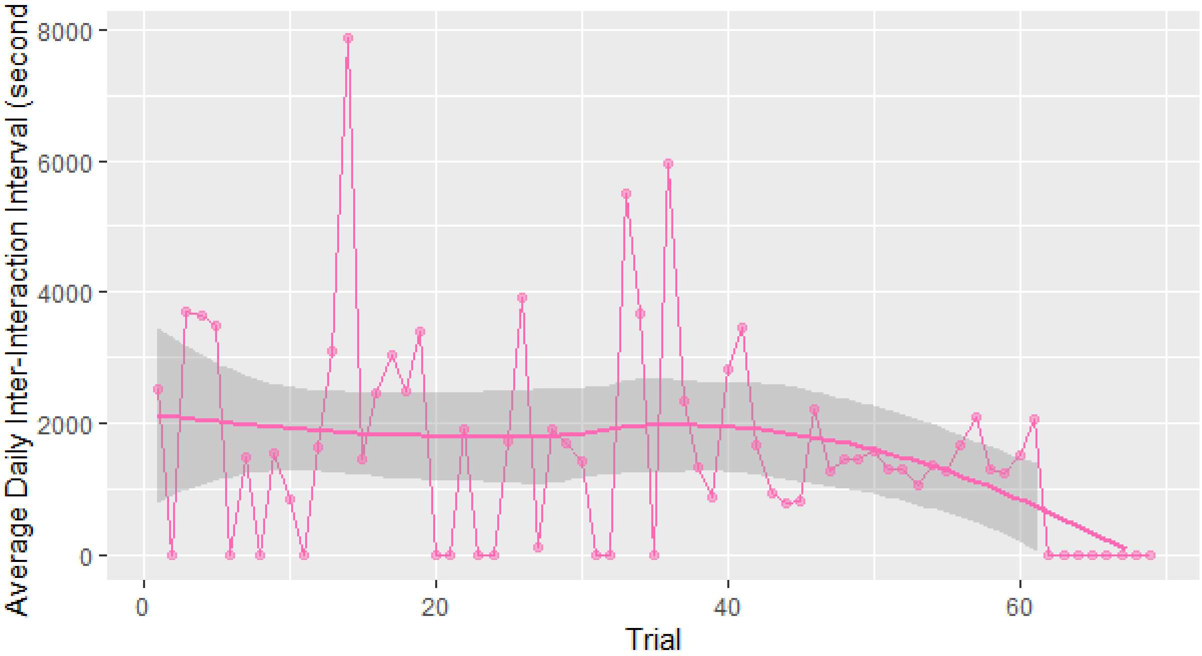

Figure 21: Average Inter-Interaction Interval by Trial: Round 2

Phase: Finally, we come to the learning (or lack thereof) associated with our attempts to train the birds to push stones down the hole in the platform, which is characterised by differences in what we called the 'Phase' variable. As mentioned, the majority of the data was collected when running the feeders on various schedules, wherein the food was dispensed irrespective of the what the birds did; however, there were multiple attempts at ‘Shaping Phases’ wherein food would be dispensed after a stone tripped a sensor.

A review of the exact plan for how the shaping phases were to be conducted can, again, be read in the methods chapter. Here, it is enough to say that the shaping phase was characterised by a) the absence of scheduled dispensations, and by b) the presence of smooth glass stones that outlined the hole in the platform. Food was scattered around these stones such that when the birds went to get the food, they might inadvertently knock a stone down the hole, triggering the dispensation of more food. The story of our first attempt at a shaping phase during the first round of data collection is, I believe, elegantly told in **figures 22** and **23** below. In both of these figures, odd number phases represent the standard fixed-interval dispensations, and even number phases represent attempts at shaping.

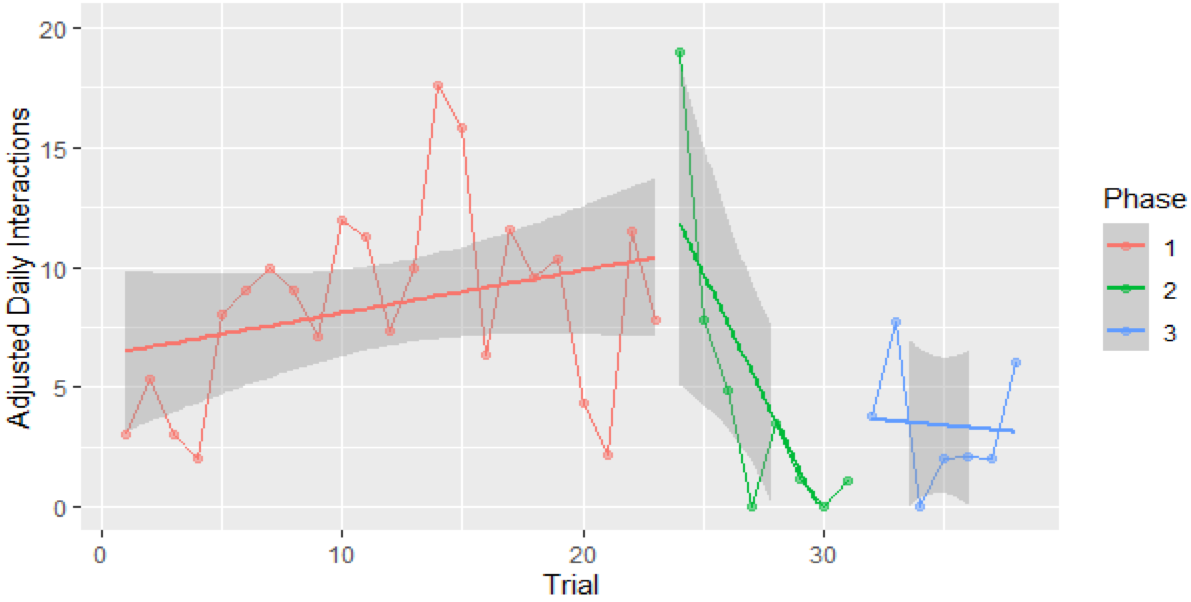

Figure 22: Adjusted Daily Interaction by Phase: Round 1: Phases 1 & 3 Represent the regular fixed-interval dispensations. Phase 2 represents the first attempt at shaping. Regression lines and standard errors are produced by ggplot’s default geom_line linear model function.

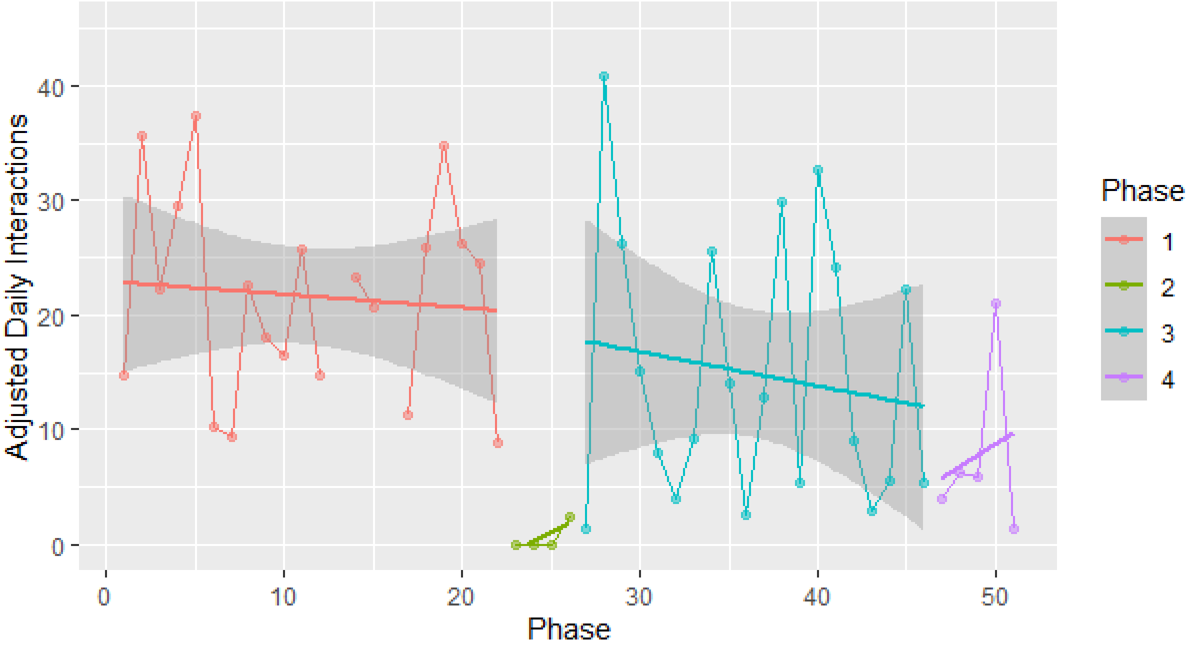

Figure 23: Adjusted Daily Interactions by Phase: Round 3: Odd number phases are fixed-interval; even number phases are attempts at shaping.

CHAPTER 5: DISCUSSION

> "Although it is relatively easy to categorize the factors responsible for nest failures, in reality it is difficult to identify them accurately since many are interrelated. For example, predation may be a consequence of reduced food availability, which in turn may be dependent upon weather conditions. If cold and wet conditions decrease the abundance of ground invertebrates, adult magpies may have to spend more time foraging for themselves and their chicks. During such periods offspring may receive less food, but will also be much more vulnerable to predation, for example by crows, simply because their parents spend more time foraging and less time near the nest. In cases like this it is impossible to identify a single factor responsible for chick deaths," (Birkhead, 1991, p. 196).

To this point, we have seen descriptive figures of how each of our independent variables relate to our outcome variable, daily interactions; however, these results do not exist in a vacuum. The variables of our study are interrelated to the extent that we find ourselves in the same boat as Birkhead, in that it would be impossible to identify a single factor responsible for our outcomes. So, here, I cash out some of the complexity that goes into interpreting our results in what I see to be their proper contexts.

5.1. GENERAL ECOLOGICAL DISCUSSION

Location: Recounting **figures 2-5**, we see two main differences between locations. First, Saskatoon, on average, had higher levels of daily interactions than did Lethbridge. Second, comparing the Lethbridge data that included both crows and magpies (round 3) to the Saskatoon data, we see an inversion in the relative response rates of the two species between locations: In Lethbridge, when the crows arrived, they dominated access to the feeder; in Saskatoon, Magpies had more daily interactions at BB2 than did the crows. There are a couple factors that we believe shoulder some of the responsibility for these differences.

As mentioned in the methods, one of the main differences between locations was the number of birds that frequented either yard-space. Based on the informal method of individual identification developed while behaviour coding (see again, methods), it seemed that the majority of Lethbridge interactions came from what were assumed to be a single breeding pair of magpies, wherein the vast majority of these interactions seemed to come from a single individual, FR. And, in the third round, a group of crows, ranging from two to five in number, would come and hang out in the yard space where BB1 was set up (or in a tall tree two backyards over, from which they could still monitor the yard). BB2, however, was set up in the backyard space of a "birding" couple, who had spent the last several years cultivating a relatively large population of corvids, which harboured approximately 30 assorted crows and magpies over the period that the Round 4 data was collected. So, even though the total daily interaction at Saskatoon were higher, on average; given the base-rate of birds, it might be assumed that the Lethbridge location had higher average daily interaction per bird.

The difference in the number of birds between yard spaces is assumed to be a construct of the respective differences in available foraging opportunities. In the Lethbridge yard, there was a bird feeder present, offering grain-based feed, which attracted many songbirds (mostly sparrows); however, it did not seem to be a fixation of the magpies' attention or diet, whereas BB1 and its scheduled supply of cat-food certainly was. In contrast, in addition to BB2, the Saskatoon yard space also had the aforementioned five-gallon dog-food style feeder, which provided a continuous supply of dog kibble. This feeder had been a permanent feature of Saskatoon yard space for several years and is assumed to be why the yard had attracted (and could sustain) such a large resident population of corvids. We estimated that this feeder would receive over four hundred bird interactions a day, predominantly from the yard's crow population (KB, personal communication).

As seen in the literature review, given the large degree of overlap in the ecological niches of crows and magpies, and, given that crows are the larger species, crows often dominate access to usually preferred magpie food sources (Eden, 1987). This is consistent with our observations from our Lethbridge location (e.g., **figure 3**); and, though it may not intuitively seem so, this may also be consistent with the results observed in the Saskatoon location. In Lethbridge, the magpies had a revealed preference to BB1 over other *continuously* available food sources (insofar as they would attend BB1, but rarely, if ever, the grain-based feeder). When the crows arrived in round three, they then dominated access to the best continuously available food source, BB1. In Saskatoon, however, BB2 was not the best continuously available food source within the birds' territory—the dog feeder was. And (although, we don't have hard numbers), the crows seemed to largely dominate access to the dog feeder (KB, personal communication); and so, the magpies may have been pushed to less preferred options, like BB2—hence the inversion of interspecific interaction rates observed between locations (e.g., **figures 3** & **5**).

Going forward, it would seem that best practice would be to treat Saskatoon's distribution of interactions as being exceptional. If we were to set up BB1 or 2 in a new location, the odds are that it would be the most preferred, continuously available food source within the territory of the local corvids; and so, one would predict to see distributions of responses that are more consistent with the Lethbridge observations. This is, we will probably see magpie interactions coming from only a few birds, perhaps a single breeding pair in the territory; and if crows inhabit the same territory, they will likely suppress magpie interaction and dominate access to the feeder.

The fact that the Saskatoon location was able to support so many birds in such a small area was itself exceptional, as the geographic distribution of food availability usually means birds must spread out or compete (as summarised by Birkhead, 1991). In personal communication with

Birkhead, Trost and Webb reported thirty-five magpie nests within a square kilometre, which, at the time, was regarded as being an exceptionally densely inhabited study site. To my view, the congregation of at least thirty birds at a time in a single backyard might be viewed as equally impressive—all the more when one remembers that the presence of crows usually relates to an increase in yearly magpie mortality and a decrease in markers of magpie flourishing (e.g., Högstedt. 1980a, 1980b; Bayens, 1981; Birkhead, 1991).

Our findings regarding the differences in location, taken together with the relevant literature, may be useful in a handful of ways: first, as mentioned, we have a baseline as to what to expect for interactions if we set up BB1 or 2 in a new location. Second, if we make it the case that the experimental feeder is not the most preferred food source in a given area, we may be able to escape the possibility of crows suppressing magpie interaction with the experimental feeder, given that the new location falls within the territory of both species. Third, one could conduct future studies, wherein relative differences in food availability might be treated as an independent variable; and one could then gauge how differences in competition (within and between species), mortality, and flourishing (e.g., proportion of successfully reared offspring, birds per square kilometre, etc.,) vary in respect to changes in local food availability.

Note: discussion of the location variable has centred on interlocal differences in food availability, as this was the most consequential difference between our locations that we measured; however, more might have been said if our locations varied substantially in climate or within other factors. Moreover, while the discussion has related to foraging opportunities *between* locations, we will still be discussing how interaction relates to changes in foraging opportunities *within* locations later (section 5.3.).

Month: When thinking causally, month is an interesting variable. Initially, one might assume that the effect of month on the level of magpie interaction is going to be largely an indirect consequence of changes in things that have a direct effect, like weather, or the presence of crows (which may or may not be seasonal), as the magpies are obviously not checking their calendars and changing their behaviour according to the monthly fashions. Still, there are interesting results that exist within the month variable, in and of itself, that run counter to what one would expect to be the assumed direct effect of its covariates.

For instance, as can be viewed in **figure 6**, our April interactions are relatively fewer than the neighbouring months. Given what Birkhead (1991) reviews about Magpie foraging, one might have predicted that magpie interaction would increase in a linear manner across spring and summer, consistent with the rise in temperature and the increase in available food sources; however, in April, magpie interaction dropped off completely for a number of days that aligned with their breeding season (recall **figure 8**).

This finding is also contrary to what one might predict with respect to how the breeding season could affect our interaction levels. Given that over the breeding season, the female's diet consists only of what the male brings her, and, given the male's tendency to try and mate guard, one might assume that the reliability of the feeder relative to other food options would make it a very popular choice during that time period; however, we obviously found the opposite to be true.

Still, this might be subject to context. The observed results make sense if one assumes the female in the territory was not stationed close enough to BB1 for the male to continue to keep an eye on her *and* utilise BB1 as a food source. A different result might have been observed if BB1 had been stationed closer to the nest site. In the future, it may be worth exploring the relationship between the distance between the feeder and the nest site and how it affects the level of engagement with the feeder during mating season (or generally).

The second observation of note with respect to month is the level of magpie engagement in July and August (see again, **figure 6**). As summarised by Birkhead (1991), magpies spend a larger proportion of time foraging in the summer relative to other seasons, as the availability of quality food sources (bugs, grains, etc.) is highest during the summer, and as breeding pairs might be rearing and feeding offspring (particularly through July). Given this, one might have predicted to see an increase in magpie engagement with BB1 through the summer. Of course, we observed the opposite, as the crows took an interest in BB1 over the summer and suppressed magpie engagement. Recalling **figure 7**, however, when we look at total interaction for both crows and magpies, we do see a trend consistent with Birkhead's summary in that total interactions do seem relatively highest in the summer months.

Moving forward, it seems pertinent to keep track of month to account for seasonal effects (such as breeding season) that aren't accounted for within more direct seasonal factors (like temperature). Moreover, I have found the month variable affords the same function as does the season variable, but with more precision, effectively rendering the season variable moot. And, as mentioned, one could do more formal studies of breeding season, specifically, to see if location of the feeder relative to nest location affects the level of interactions in the manner described.

Temperature & Wind: As, after accounting for confounds, neither temperature nor wind (**figures 10** & **11**) were found to correlate with interactions, considerations regarding how to use these variables moving forward pertain merely to the logistics of conducting research. As BB1 and 2 are more likely to sustain damage or malfunction in extreme temperature or wind conditions (and, to be colloquial, as they are frankly a pain to operate in these conditions), one might forgo data collections on such unsuitable days. Though, given the ease to which the data on temperature and windspeed can be obtained for days where trials have been conducted, I see no harm in including it in possible future studies.

5.2. FORAGING RELATED DISCUSSION

Foraging related variables regard how differences in foraging opportunities *within* a location might affect the daily interaction levels. Because the exploratory nature of the study was not my first intention, as for all variables, I had no predictions about how differences in foraging related variables would affect our outcomes leading into the investigation. Given the after-the-fact nature of my reasoning, I present the quasi-predictive thinking that relates to this section, here (in contrast to the introduction). Because magpies are known to be a caching species, there were a lot of unknowns with respect how foraging behaviour might change as caches accumulated and were emptied of trial blocks. For instance, as reviewed previously, magpies are short-term cachers and collect their caches usually within a day of making them. Would it be fair to expect a zig-zagging pattern of interaction, wherein the birds visit the feeder regularly for a day and clean out their caches on the next, neglecting the feeder? Or, perhaps we would see consistent feeding, irrespective of cache collection. Given the five-on-two-off pattern on which our feeders operated, it seemed possible that we would we see a spike in interaction on the first new day of a week, as if the birds would be making up for the lost foraging opportunities of the weekend, or would we see a dip in interaction, as if they were relearning that the feeder was once again open-for-business?

Day of Week and Consecutive Days in Trial Block: Observations across several variables have, in part, answered these questions for us. Recalling **figures 12** & **13**, there is a slight arching shape in interactions throughout an average week of data collection. This pattern has incited jokes about both 'Monday effects' and '"Hump" day effects', referring to how the magpies could be said to have a ‘case of the Mondays’ at the start of the trial blocks in regard to the relatively low activity, and regarding the relatively high activity at the middle—or hump—of the week, respectively.

Two things might be said about the recurring pattern observed in **figures 12** and **13**. First, we have a description of a difference, which is useful for establishing expectations for future studies; and it might also be useful for making predictions; however, we do not have an explanation for the observed difference. As I mentioned earlier, it could be the case that—on the first day of the week—we see a decrease in engagement because the birds have had to spend the weekend (BB absent days, from the birds perspective) organising their behaviour and movement around foraging from other sources; thus, the dip in first-day interactions could be a result of readjusting to the new contingency. Of course, this is one supposed antecedent, and many might fit the bill; further work is required to corroborate or falsify this possibility. Moreover, it does not account for why we see the dip in interaction at the end of a trial block.

I am at a loss as to why the last-day-dip occurs, generally, and across subsamples of the data. Of course, the difference might not be meaningful at all (as goes for our Monday difference); though, if I were to speculate, it could relate to caching: perhaps it takes roughly four or so days for the magpies to build up sufficient cache reserves as to forgo foraging for new food, given the amount a consistency of food dispensed. To test this, one would need only observe how the weekly distribution changes in relation to the quantity of food given per trial. Or perhaps the cat food dispensed from our feeders did not perish as fast as typically cached items, and, so, does not need to be retrieved as often, etc. More work would be required to answer these questions.

Days Since Last Trial: As seen in **figure 14**, the ‘days since last trial’ variable *corroborates* the notion that the magpies engage less on the first day after a break because of a readjustment to a change in contingency: the more days that have past since the magpies had access to the feeder, the fewer interactions they had, on average, upon regaining access.

Food Factor: The availability of alternative food sources, referred to as the food factor, was not a variable I was tracking from the beginning of the study. I began tracking this when a particular event made it clear that alternative food sources might contribute significantly to differences in interaction levels. Although, even if one had been tracking alternative food sources within locations from the beginning of the study, one still could not make clean predictions without accounting for the magpies' relative preferences for different foods, as we have discussed in relation to interlocal differences.

For instance, if we assume a) that the cat food dispensed from our feeders is the most preferred food source on location, and b) that it is a sufficient quantity to sustain the birds, irrespective of alternative foraging opportunities, then we might predict that alternative foraging opportunities on location will make little differences to our daily interaction levels. Though, if either premise (a or b) is false, we would predict that the presence of alternative foraging opportunities on location would lead to a decrease in our daily interactions; but, in neither case can we say how—directionally—the presence of alternative foraging opportunities on location, measured in a binary sense, will affect our interactions, irrespective of context. Moreover, these predictions might be complicated by confounding factors, like whether or not crows are present and dominating access to whatever the most preferred food source is, on a given location, or whether diversity of food intake plays a role; it could be the case that one cannot make an absolute fixed preference hierarchy for magpie food sources; maybe if one has had cat food every weekday for months, a grub might hit the spot, even if the cat food is generally preferred.

This is also why it was difficult to make predictions with respect to how our interaction levels would change in the summer. We know, generally, that magpies spend more time foraging in the summer, as more preferred food sources are available (chiefly, insects and other invertebrates); and we know that breeding pairs will also need to collect more food to feed their fledglings; but, given the tangled web of context in which foraging behaviour is situated, I could not predict whether summer would see an increase or a decrease in foraging. And, unfortunately, with the advent of the crows over our summer of data collection, I still cannot say.

As mentioned, the presence of alternative foraging opportunities within a location was measured in a binary sense: are there or aren't there possible alternatives (to the best of my knowledge). The possibility that any alternative foraging opportunities might be affecting interaction came to the fore when additional provisioning of food ceased in the second round of data collection**,** and I observed an abrupt jump in interaction levels (**figure 15**). From the time I started tracking this variable, I could be certain when *no* additional provision was occurring (hence, no alternatives); however, I could *not* retrospectively account for which trials additional provisioning had occurred (hence, possible alternatives).

Obviously, the implications of a difference to the observed scale were major (see also, **figure 16**). Though, the number of trials supporting the observation were limited; and we had not implemented proper ABAB procedure to be confident that the relationship observed was causal. More work was needed, wherein we could more cleanly track days where alternative foraging opportunities were available. The Saskatoon location was perfect for this, as the five-gallon dog feeder was ever-present in the yard. Given that we had estimated hundreds of daily crow interactions at the dog-feeder, one might have predicted that removing this source would cause a sharp increase in interaction at BB2, particularly among the crows; perhaps the magpies would be pushed out, as was the case in our Lethbridge site.

As we shown in **figure 17**, we saw nothing of the sort. Relative interaction levels were basically the same at BB2, regardless of whether or not the dog-feeder was present in the yard. This outcome puzzled me. One condition that might account for what we observed could be that the crows left to search for other food sources, having lost what had tied them to the yard to begin with. If one goes back to **figure 3**, they might also notice that during this time, magpie interactions at BB2 were trending upwards (and crow interactions, slightly downward), which would be basically what one would expect if the crows were leaving to find other food. Though, of course, this is speculative, and more work is needed. Moreover, we once again failed to implement proper ABAB procedure. It should also be noted that we ran twenty trials without the dog-feeder in the yard, which (as will be discussed in the next section on learning) was more than enough to establish learning curves with regard to other patterns in the corvids' behaviour; yet, the crows did not seem to adapt to utilise BB2 as a replacement food source.

Still, given what was found at our Lethbridge site, the implication seems to be that alternative foraging opportunities should be controlled—to the extent that this is possible—so that variation in the factor does not interfere with future investigations. Alternatively, as mentioned, avenues for future studies exist, looking at the consequences of variation in foraging opportunities both between- and within locations. Moreover, establishing the relative preferences of food items for magpies could be empirically tested, which could aid in these types of investigations.

Dispensation Schedule: We can conclude our section on foraging by considering how the daily interactions change in relation to the feeders' dispensation schedule. Recalling **figure 18**, we see that as the number of dispensations per hour increase, so does the number of daily interactions, on average. This makes sense, particularly if the birds can see when food is or is not present in the feeder bowl without landing on the platform. If the birds can see the food, and food is dispensed more often, they will make more trips to collect it. The fact that birds show up when the bowl is empty would strongly suggest, however, that, in the least, they cannot tell when food is *not* present. In reviewing daily cases, I informally observed that it might also be the case that when the first few interactions on the day are false (unrewarded), the birds did not seem to invest the energy to return as often throughout the day.

Of course, it should be noted that the data for different dispensation schedules comes almost exclusively from days wherein the feeders' malfunctioned. The different categories of schedule were not controlled; and the number of trials in each bin are far from equal. For instance, when the weather dropped below minus twenty, the motor on the food hopper would sometimes freeze part way into a trail day. As mentioned, the feeder was set to dispense every fifteen minutes on a four-hour trial (~ sixteen times a day; which constitutes the 'High' dispensation schedule); however, on these cold days, the motor would usually freeze after the first hour, completing only about four dispensations over the four hours (constituting the 'Low' dispensation schedule). The regular correlation of the lower output days and cold weather further confounds our ability to interpret our outcome. This is, do we see relatively few interactions on these days because of the cold weather or because of relatively low food output? Or both? The evidence from **figure 10** might push us to believe the low food output is the more likely antecedent; however, we cannot discount the interrelatedness of our variables.

Still, the fact that the data for this variable comes almost exclusively from days of malfunction does not mean the description is without value! As mentioned, the results clearly fit with intuition on the subject. Moreover, these results lend themselves to obvious practical application: if we want more birds, increase the dispensation schedule. There may be trade-offs: the higher the schedule, the more money needs to be spent on bird-food, the more motor-use, and the more wear on other parts, etc.; however, these questions of application are separate from those of observation, and can be dealt with in accordance to how one wants to organise future studies. Finally, if one is not confident enough in this initial outcome to let it guide application, it would be very simple to design a more controlled follow-up study.

5.3. LEARNING RELATED DISCUSSION

Given my assumptions, learning related factors (as can be understood in the associative and operant sense) should have made for straight-forward analysis; however, like every other variable domain in this exploratory investigation, results came in a mixed bag.

Learning Curves: The first assumption I made was that the number of interactions per day should increase along with the number of trials, before plateauing at a number of interactions wherein the birds are basically getting maximum value out of the feeders. This description would constitute a typical ‘s-curve’ or ‘learning curve’; however, when we go back to **figures 2** and **3**, we see that we do not observe learning curves in all instances that we would expect to. Moreover, we observe such curves in places that were not expected, but make sense in retrospect.

From the Lethbridge data, there appear to be four curves. First, in round one, as expected, we see the magpies' interactions per day increase steadily with the first ten-or-so trials, before plateauing. This is, the birds appear to have learned how to integrate the novel food source into their foraging routine. After several more trials, the number of interactions then descends from the plateau, which would be counter to what one would expect; however, other interventions (that we will talk about a bit later) do well to account for this change; so, the initial learning curve from trial one was still promising.

One would expect to see a similar curve to this at the start of round two: the birds would not have had access to the feeder for roughly a month; and they would have had to re-learn to integrate the feeder into their foraging routines (though, we might expect this re-learning would have a steeper slope than in the first case). Though, of course, looking at **figure 2**, we see nothing of the sort. We see a flat line of few or no interactions per day, until around the time additional food provisioning ceased (see again, the food factor), wherein we observed the expected, steep reintegration of the feeder into the birds' foraging.

In the third round, we observe the crows' learning curve, as they came to integrate the feeder into their foraging behaviour; and, we see the concurrent inverse, where the magpies learn that the feeder is no-longer a viable feeding option, given the presence of the crows (**figure 3**). Regarding the magpies, the question remained as to whether they were learning to avoid the feeder because of the crows, in and of themselves, or because the crows were taking all the food?

I tried to tease the above question apart by looking at the daily proportion of responses in which the magpies got food (rewarded or 'true' responses), versus interactions where they left the feeder empty handed (unrewarded or 'false' interactions; see again, **figure 19**). My logic was this: if the magpies were responding to the fact that a typically rewarded behaviour was no longer fruitful, then one would expect to see the proportion of true responses drop prior to the decline of interactions. If the proportion of rewarded responses was not driving the change in interaction level, it would be safer to assume the alternative (that the magpies are avoiding the crows, in and of themselves).

Recalling **figure 19**, the proportion of true magpie responses per day actually increased, on average, over the course of round three, which would imply that the decrease in magpie engagement was not a consequence of an unfulfilled schedule of reinforcement. Of course, a description that runs counter to one hypothesis does not confirm the other. Still, given the above, it seems more likely that the decrease in magpie engagement in round three was a response to the crows, in and of themselves; however, I am sure there are alternative interpretations that fit the same description. It should also be noted that, given the relatively low number of magpie responses per day, there is large variability in proportion of true responses; and it is more probable that the law of small numbers rears its ugly head to present unlikely patterns.

Inter-Interaction Intervals: As mentioned, the inter-interaction interval is a measure of time between the end of an interaction and the onset of the next at the feeder. I wanted to use this variable to see if the magpies would 'tune in' to the usual fifteen-minute dispensation interval.

If it were the case that the magpies were learning the schedule of dispensation, one would expect to see a learning curve wherein the variability in the average inter-interaction interval per trial would decrease and converge around 900 seconds (15 minutes) over the course of several trials. There are cases where this interpretation seems plausible (recall **figure 20**).

As can be seen in the results, consistent with the learning curve for the number of interactions in round one, the average daily inter-interaction interval decreases and plateaus around roughly 1000 seconds for about ten trials, before the advent of the shaping phase threw the rhythm into disarray. This struck me as promising; however, like the curves for interactions per trial, we do not see this in all instances where expected. In the second round, we can see large variability in the average daily inter-interaction interval for more than the first forty trials, despite no interventions of phase in the round (recall **figure 21**). It can be observed, however, that the variability decreases markedly, consistent with the cessation of additional feeding. Unfortunately, given the circumstances of rounds three and four (that we will discuss shortly), we cannot evaluate changes in the inter-interaction interval the same way as the first two rounds.

When reviewing the daily videos to collect data, I got a sense of two versions of how the birds might tune to the schedule of dispensation. The first is relatively boring: but in the third round, one of the crows essentially set up shop next to the feeder. This crow obviously developed an association between the sound of the motor and the presence of food, as every time the motor would dispense food, the crow would race to the platform, arriving mere seconds after dispensation. Two more crows developed a similar behaviour pattern; although, it is not clear if they were responding to the motor, or responding to the original crow's movement toward the feeder. Regardless, reviewing the videos at this time of the third round was sometimes humorous, as one would observe a dispensation, followed by a three-crow race for food.

The second manner of tuning seems, to me, more subtle, and only worked over periods of time where *one* subject was using the feeder but wasn't hanging around close enough to develop the same conditioned response as were the crows: for instance, like in the first two rounds, where the vast majority of interactions were from who we believe to be a single subject (FR). The shaping of the inter-interaction interval seemed to go as follows: FR arrived sometime following a dispensation. This interaction is rewarded (i.e., FR receives food). If FR's next interaction is before the next dispensation (necessarily less than 900 seconds from the previous interaction) this interaction goes unrewarded, as there is no food yet. This pattern sets up obvious operant selection against revisiting the feeder too often. If a) it costs energy to visit the feeder to check for food, and b) a visit occurs too soon after the previous one, then consecutive interactions in a short period of time are effectively being punished. So, the bottom-end constraint is fairly obvious.

We might ask ourselves, then, what is constraining the inter-interaction interval from becoming too long? If a) it costs energy to visit the feeder, and b) no other birds are around to steal the food, then why wouldn't a magpie allow multiple dispensations pile up and refrain from making trips for every dispensation? If I were to guess, there could be several factors that contribute to a top-end constraint on the inter-interaction interval. 1) Even if other birds are not currently using the feeder, other birds may still be competing for the food, and it does not mean that the bird's history of foraging behaviour isn't shaped in competitive contexts, wherein one would learn to behave as to eliminate the possibility of arriving too late and risking food being taken by a competitor. 2) As already noted, magpies are caching birds, and can only carry so much food in their throat pouch at a time. As observed when reviewing the daily videos, if too many dispensations built up, it still resulted in a magpie making multiple trips to collect all the food.

Obviously, the inter-interaction interval ceases to be an indicator of the birds learning the schedule of dispensation when more than one bird is using the feeder over a period of time, because the presence of multiple birds means that the empty food bowl takes on new meaning (i.e., is setting up different contingencies). This is why we cannot interpret the inter-interaction interval results from the third and fourth round in the same way as the first two (although, the III might become a viable variable for analysis again in multi-bird contexts if one had an exact method for individual identification). Still, in contexts where a single subject is being researched, I maintain that the inter-interaction interval shows promise with respect to demonstrating how the birds tune to a given schedule of reinforcement. Future work could be done wherein we track how controlled changes in the schedule of dispensation affect the inter-interaction interval.

Phase: The outcome of our attempts to train birds to drop stones down the hole in the landing platform (referred to as the shaping phase, in contrast to the fixed-interval "habituation" phase wherein most of data were collected) can be recalled in **figures 22** and **23**.

As was seen, the first attempt at a shaping phase in round one practically extinguished all interaction at BB1 over the eight days it was attempted. Several things might be said about this. In this first attempt at shaping, shiny blue stones were used. The magpies' reactions to these stones could be described as neophobic. For instance, the initial spike in interaction on the first day of the shaping phase was a consequence of one of our magpie subjects hopping up on the platform, seeing the stones, and immediately hopping off the platform again, repeatedly, as if hoping each time that the stones might be gone so as to allow it access the food. This is, the multiple interactions that built up on the data sheet would probably be better described as just one extended interaction, wherein one magpie seemed to be trying to work up the mettle to risk taking a chance at the food, in spite of the presence of stones. Of course, I am anthropomorphically projecting here; however, even though we had not operationally defined fearful responses, it was clear that the magpies' behaviour in the shaping phase was markedly different from that of the system to which they were accustomed. There were increased head movement, twitchier strides; instead of strutting upright to the food bowl, they hunkered low, advancing skittishly from the perimeter of the platform before diving in for the food and flying away, all at once. Only later did we discover a study that found that magpies, contrary to popular belief, tend to reflexively avoid shiny things, and seemed to especially dislike the colour blue (Shepard et al., 2015).

The behaviour of fleeing immediately with whatever food had been grabbed could easily have been shaped—or at least exacerbated—by the noisy consequences of knocking a stone down the hole. The glass stones were heavy and would clunk down the tunnel before triggering the apparently aversive vibrations and sounds of the motor dispensing more food. Instead of conditioning the sound of the motor as a signal for reward, we seemed to do quite the opposite.

Given the disaster that was the first attempt at a shaping phase, a couple of adjustments were then undertaken. First, I painted the glass stones a matte black, and they took on a much more "natural" appearance. Second, when KB built BB2, he did a better job insulating the hopper box, to reduce motor sound and vibration, and better padded the funnel box where the stones fell. Unfortunately, KB never tested a shaping phase on BB2; though, I did get to try two more shaping phases in the third round of data collection with BB1.

As we can recall in **figure 23**, these went about as well as the first attempt, but were further confounded by the number of magpies and crows present in the third round. It seemed promising that the crows who had learned to associate the sound of the motor with the food might do well at the shaping phase; however, to run the shaping phase, one needed to be in the yard-space to reset the stones and food after each interaction (unlike the regular operation, wherein the feeder could run without supervision). The magpies seemed unaffected by my presence in the yard during the shaping phase (one of the magpie pair's apparent chicks even came and pecked me on the foot and on the hand); however, the crows—who normally dominated access to the feeder in the third round—did not seem to come to the yard while I was present. In effect, I was a literal scarecrow. I eventually ran the shaping phase without supervising the yard (if the birds knocked all five stones in, then that was what they got for food); however, I am not confident I tried enough trials to give the crows an adequate chance to learn before I decided to stop data collection. On the bright side, when magpies did arrive in these later attempts at the shaping phase, they seemed less afraid of the black stones than the original blue stones; although, this interpretation is confounded by the possibility that the birds were merely habituating to more trials with the stones. To determine whether it was the change in colour and reflectivity of the stone or a change in habituation that incited the difference would require more work (although, this presupposes that there was an actual difference, as neophobic responses were never formally analysed).

In sum, despite the disappointing outcomes of our three attempts to shape stone-dropping behaviour, I remain confident that it is still possible (Hans' aforementioned success training magpies to recycle bottle caps is proof of concept). This said, adjustments will probably have to be made: perhaps I should try shaping for more than five trials at a time; perhaps I need to simplify the behaviour being shaped. One possible alteration would be to manufacture a sensor system that responds to the birds merely landing on the platform. This would hopefully condition an association between the motor sound more easily and explicitly than with the current system, wherein the dispensation is contingent on the birds arriving, dropping a stone, and then being rewarded.

For as much as has been said about how learning related factors have shaped our outcomes, there exists opportunity, even within the existing data, to do so much more. For example, I have data on both the total number of false interactions and true interactions on a given trial day, as well as a ratio variable that reflects the proportion of true interactions over the total number of interactions for a given trial day. These data could theoretically be used to attempt to make predictions about daily interactions, based on, say, the proportion of true responses received the day prior. Or, even on individual days, we might be able to make predictions about inter-interaction intervals based on the previous interval and whether its end response was true or false.

5.4. CONCLUSION

So, what has come of all this? How does the end reflect on the start? Well, I started with misguided theoretical assumptions, and, given these assumptions, I wanted to contrast two competing hypotheses in such a way that I now view to be insincere (inasmuch as they were not mutually exclusive, and one could obviously account for the differences of interest; I was creating a faux problem to solve, instead of doing the work to find a real problem). Moreover, even after I adjusted my telos (to something that still wasn't wholly sincere), I was still unable to shape stone-dropping behaviour in magpies in a way that might bear on this adjusted aim. So, reflecting on our initial theoretical motives and logistical requirements, I have failed rather spectacularly. This is not a bad thing.

Of course, that does not mean the project has been without value. The most obvious contributions of the project are the automated feeders, BB1 and 2. These feeders have demonstrated quite clearly that it is possible to study the local corvid populations in a manner that is non-invasive and that affords, perhaps, a higher degree of experimental control than is typically found in investigations of free-ranging animals, hopefully, without sacrificing too much of the external validity. Moreover, these feeders are highly flexible, and can be operated without supervision, internet, or supplementation from fixed power sources. These feeders can, theoretically, be adjusted to allow one to implement a limitless array of study designs, given any kind of theoretical rationale from psychological or ecological sub-disciplines. This is, the feeders afford a powerful modus operandi for scientific investigation, in and of themselves.

Moreover, as mentioned in the methods, I have established databases of various levels of refinement (with some assistance from KB, of course), that capture the entire contents of our exploratory investigation. First, the videos of each trial are saved in multiple locations; if one wanted to revisit the raw data from a new theoretical perspective, it would be very easy to do so. Second, there are refined versions of the videos from each trial, depicting only the interactions (also saved in multiple locations). And third, there are two clean datasets that contain the information of focus for my exploratory investigation: the first looks at each trial as a unit of observation; the second, more refined version looks at each interaction as a unit of observation (saved in multiple locations and available upon request).

And, finally, to my view, this thesis is not as large a contribution as are the feeders or their products; however, the exploratory investigation was not a fruitless endeavour! We have established good baseline information about engagement with the feeders; given this, we know how to better adjust for desired outcomes in future investigations. Taken together, the practical applications of our analysis are essentially these: if, moving forward, we want our baseline of activity to be relatively high compared to our interaction levels in this exploratory investigation, then we should conduct research in and around the summer months, operate with relatively high dispensation outputs from the feeder, and be mindful of alternative foraging opportunities. This prescription does not, however, take into account crow-avoidant interventions; although, the implementation of these would depend on what exactly one is trying to do moving forward.

Moreover, we have identified several candidates for potential future investigations, which can be broken down in several ways. We could continue to investigate ways to optimise interactions with our feeders. If anything, the system we have developed, here, is tailor-made for ABAB type analyses; and a replication of the results described here could be conducted to see what holds up given this more formal method of analysis. These investigations could regard further looking into how additional foraging opportunities, number of local birds, nest distance from feeder, feeder location, etc., influence interaction. Of course, several of these plans might have independent theoretical rationales, too. Validating a method for individual identification of birds could allow better means to test theories that relate to personality psychology, behaviour reaction norms, etc. And, as mentioned, manipulation of schedule of dispensation still offers limitless possibilities for investigation of learning related phenomena.

More could always be said; however, with the validation of the here discussed modus operandi, perhaps it can be said be people far more competent (and ornithologically inclined) than myself.

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