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Neural Investigations into Causal Interventions and Reasoning in Rats

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in Psychology

by

Jared Wong

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ABSTRACT OF THE DISSERTATION

Neural Investigations into Causal Interventions and Reasoning in Rats

by

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Doctor of Philosophy in Psychology

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Professor Aaron P. Blaisdell, Chair

The ability to learn and effectively manipulate causal structures in the world is an important aspect of cognition that allows us to interact with the environment in meaningful and often biologically significant ways. While many have considered such a key cognitive faculty to be one that divides humans from animals, recent evidence has suggested that rats are capable of learning causal structures and making correct inferences of these structures in a way that is effective (e.g., to obtain a desired outcome), whether from passive observation of the causal structure, or intervention on an event within the causal structure (Blaisdell, Sawa, Leising, & Waldmann, 2006). Furthermore, this ability appears to be more than merely an approximation via basic associative processes. However, the nature of causal interventions is not well-understood. The observation that rats that intervene to evoke an outcome of a common cause do not also expect the other co-outcome to occur (i.e., the attribution of the outcome to their own action as opposed to the occurrence of the common cause, referred to in this dissertation as the

causal intervention effect) suggests that causal interventions may require sensitivity to action-outcome contingencies. If so, brain areas that are known to govern goal-directed action (i.e., the dorsomedial striatum) should be necessary for causal interventions. After determining experimental perimeters required to obtain some basic effects (namely, the causal intervention effect and goal-directed/habitual instrumental behavior), an experiment was done wherein rats were either pre-trained to lever-press in a manner that was either outcome-sensitive (i.e., goal-directed) or outcome-insensitive (i.e., habitual) prior to training on a common cause model (i.e., Tone ← Light → Food). During test, rats either intervened on the lever to produce, or merely observed Tone. If causal interventions require outcome sensitivity of actions, then only rats that behaved on the lever in a goal-directed manner (i.e., outcome-guided) should have shown the causal intervention effect and thus discounted Food; whereas rats that behaved habitually (i.e., outcome-insensitive) on the lever, as well as all subjects that merely observed the Tone should have attributed it to the common cause (i.e., Light) and thus expected Food. This study was followed up with another involving a direct neural manipulation, wherein subjects were given either a lesion of the posterior dorsomedial striatum (pDMS) or a sham lesion before being trained on the common cause model, and subsequently tested as before. Since the pDMS has been shown to be necessary for the acquisition and expression of action-outcome associations in instrumental conditioning (e.g., Yin, Ostlund, Knowlton, Balleine, 2005), pDMS-lesioned rats that intervened on (as well as all rats that merely observed) the Tone should not have discounted Food. Conversely, sham-lesioned rats that intervened on the Tone should have discounted Food, thus showing the causal intervention effect. Unfortunately, neither predictions were supported by the data from both experiments. Future avenues of research about causal interventions, inferences, and reasoning are discussed.

The dissertation of Jared Wong is approved.

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2013

This dissertation is dedicated to my parents,

who have given me more than what most children

receive over one lifetime, and all my

fellow students of learning.

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LIST OF ABBREVIATIONS

ANOVA	Analysis of variance
CR	Conditioned response
CS	Conditioned stimulus
DA	Dopamine/dopaminergic
DLS	Dorsolateral striatum
DMS	Dorsomedial striatum
IP	Intraperitoneal
ITI	Intertrial interval
LiCl	Lithium chloride
LP	Lever press
M	Mean
NMDA	N-methyl-D-aspartate
pDMS	Posterior dorsomedial striatum
PBS	Phosphate buffered saline
RI	Random interval
RR	Random ratio
SEM	Standard error of the mean
SNc	Substantia nigra pars compacta
US	Unconditioned stimulus
UR	Unconditioned response
VTA	Ventral tegmental area

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CHAPTER 1

General Introduction

1.1: Overview

Cognition is an important ability that allows many animals, including humans, to survive, operate, and thrive in the environment. It is crucial that animals learn from experience, store the relevant information into memory, and integrate that memory with ones acquired previously, so that they can act appropriately when faced with similar situations. Of particular interest is the capacity to acquire causal knowledge, which has been claimed by some to be among our most central cognitive features (e.g., Waldmann & Hagmayer, 2005). Such knowledge allows the organism to predict outcomes, and perform appropriate actions in order to attain desired goals. In other words, it allows us to interact with the environment in meaningful – and quite often biologically relevant – ways. A simple example would involve flipping a light switch to illuminate a dark room: Knowing the causal relationship between the switch and the lights allows us to manipulate the environment as desired.

The psychological processes underlying learning has most traditionally been studied under the framework of Pavlovian conditioning and instrumental conditioning. In Pavlovian conditioning, if a conditioning stimulus (CS; e.g., a light or a tone) is paired repeatedly with an unconditioned stimulus (US; e.g., food presentation or a footshock), the animal will learn to produce a conditioned response (CR; e.g., salivation or freezing) upon being presented with the CS, as if it had predicted the occurrence of a US from the CS. In instrumental conditioning, if an animal repeatedly presses a lever that is followed by a food reward, it will eventually learn to press the lever in order to attain the food reward. However, the quality of this knowledge is less clear: In the Pavlovian case, do the animals learn that the CS is the cause of the US? Similarly, in the instrumental case, do the animals learn that their lever pressing is the cause of the food presentations? While there is little debate that human beings are capable of causal learning and

reasoning, what are the psychological and neural bases of how we are able to learn and effectively diagnose cause-and-effect relationships? And most interestingly, are non-human animals such as rats capable of acquiring and effectively manipulating causal knowledge? The goals of this dissertation are: (a) to review relevant background information on the study of causality; and (b) to present experimental data that will hopefully shed some light upon the aforementioned questions.

1.2: Historical Background: Philosophical and Psychological Theories of Causality

Drawing back to the example of the light switch used earlier: If we flipped a switch and the room lights came on, we would be rather quick to conclude that the switch was the cause of the lights. However, we would have arrived at this conclusion without having actually seen the physical way in which the flipping on the switch triggered the illumination of the room. We probably arrived at the conclusion from the temporally contiguous relationship between both events (as well as similar experiences with switches that we have had in the past – this important caveat will be discussed later). However, while we would have made the correct general causal relationship between the switch and the lights, the actual underlying causal structure is actually rather involved: Flipping of the switch closed the electrical circuit, which promoted the flow of electrical current in the form of moving electrons. The flow of electrons heated up the tungsten in the light bulbs, which ultimately caused them to glow brightly. In many cases, it is even impossible to observe physical causality – we cannot actually *see* the flow of electrons. How is it then that we were still able to construct a correct causal relationship between the switch and the light?

This question of how exactly it is that we are able to learn about causal relations and events in the world has been the subject of an age-old debate. The British Empiricist David

Hume (1711-1776) hypothesized that causality is nothing but an illusion based on associations that are occasioned by constant conjunctions of events (Hume, 1739/1964). In other words, according to Hume, observed spatio-temporal contiguity is the key – we infer causation from correlation, which would also enable us to make predictions from prior observations in the absence of any explicit knowledge of physical causal relations. In accordance to this view, some of Hume's followers have even deemed causality as undeserving of scientific attention (e.g., Pearson, 1911; Russell, 1913).

1.3 Associative Theories

Associationists have since adopted this view to explain causal learning. If this account were to hold true, and the illusion of causation is nothing more than a marriage of covariational cues (i.e., stimulus-stimulus learning), then it would imply that causal learning involves rudimentary implicit associative learning systems. If so, then the CS and the US association as established via simple Pavlovian conditioning would enable the learner to attribute predictive properties to the CS to invoke the US; and likewise, in the case of instrumental conditioning, the learner would attribute predictive properties to its responses on the lever to invoke the food reward. In fact, early cognitive views of causal learning attributed the primary goal of associative learning to the discovery of causal textures in the world (e.g., Tolman & Brunswik, 1935). According to this view, perhaps what Pavlov's dogs learned was to predict food from the bell; and perhaps the salivation reflex to the bell was merely a consequence of evolutionary responses that served only to facilitate the learning of that casual prediction.

In an attempt to formulate associative learning, Rescorla and Wagner proposed a mathematical model that has now come to be known ubiquitously as the Rescorla-Wagner model (Rescorla & Wagner, 1972):

$$\Delta V_X = \alpha_X \beta \, (\lambda - \Sigma V)$$

where ΔV_x is the change in the strength of association of CS X, α_x is the salience of CS X (bounded between 0 and 1), β is the rate parameter for the US (bounded between 0 and 1), λ is the maximum conditioning possible for the US (1 if a US is present, and 0 if a US is absent), V_x is the current associative strength of CS X, and ΣV is the total associative strength of all CSs.

The model takes into consideration changes in associative strength between a CS and a subsequent US as a result of a conditioning trial. One essential feature of the model lies in its assumption that the simple co-occurrence of two events in itself is not adequate for any modifications in associative strength. Rather, the co-occurrence must be unanticipated on the basis of the associative strength before the conditioning trial. Moreover, the model allows for the consideration of more than one CS:

$$\Delta V_A = \alpha_A \beta \, (\lambda - \Sigma V)$$

and

$$\Delta V_X = \alpha_X \beta \, (\lambda - \Sigma V)$$

where

$$\Sigma V = V_A + V_X \qquad \text{(A and X are two separate CSs)}$$

From the model, it is possible to conceive how a CS can develop a predictive relationship with a US, thus creating an illusion of causality, as postulated by Hume – the learner will come to associate two temporally contiguous events together with no need for knowledge of exactly how the first event physically causes the second event. Associative knowledge is all that is needed.

One of the greatest successes of the Rescorla-Wagner model at its time of conception was its ability to explain phenomena involving cue competition, such as Kamin's blocking effect

(Kamin, 1968), where if a pretrained Stimulus A is subsequently trained in compound with a second Stimulus X (i.e., A+, followed by AX+, where '+' indicates the presence of an outcome), responding to Stimulus X would be weaker than a scenario where Stimulus A was not pretrained (i.e., AX+ only, then tested on X). According to the model, during A+ training, Stimulus A has already gained adequate associative strength to predict the US (i.e., there is no unanticipated outcome). Thus, during subsequent AX+ training, Stimulus X was in essence redundant in predicting the occurrence of the US. As such, Stimulus X developed little associative strength, as reflected in its poor predictive power of the US during test trials of X alone, relative to the case where Stimulus A was not pretrained prior to AX+ trials (wherein Stimuli A and X developed equal associative strength with the US). The notion that the concept of causality could be governed solely by associative learning processes gained further support when Dickinson, Shanks, and Evenden (1984) showed evidence that causal judgement could also be subjected to Kamin's blocking effect: They first pretrained human subjects with a cue as the cause of an outcome. Thereafter, they gave further training with that same cue, but compounded with a second cue. They found that the pre-training of the first cue attenuated the extent to which the second cue could be judged as an effective cause of the outcome. Thus, perhaps associative learning theories were indeed sufficient to constitute a basis through which causal learning and judgement are made possible.

However, while in the process of replicating the blocking effect observed by Dickinson et al. (1984), Shanks (1985) found that the predictive power of a cue Y that was trained in compound with another (Y; i.e., XY+) could be altered by further training with just the competing cue alone (i.e., either X+ or X-, where '-' indicates the absence of an outcome), even though no further training with cue Y was received. Specifically, the experiment, conducted by

Shanks (1985) under the guise of a video game, was as follows: In the first phase, subjects received training with AB+ and CD+. According to the Rescorla-Wagner model, both cues A and B, as well as C and D, should have acquired equal associative (and thus predictive) strength, assuming equal saliences. In the second phase, training was given with A+ and C- alone. This should have resulted in an increase of predictive strength of cue A, as well as a reduction of predictive strength of cue C. Because cues B and D were not present during the second phase, their predictive strengths should have remained equal to each other, as well as unchanged by the second phase of training. However, when tested with cues B and D alone, B was actually rated to be less predictive of the outcome than D. Thus, the training involving A and C during the second phase retrospectively affected the predictive efficacies of B and D – a phenomenon that has come to be known as retrospective revaluation, and one that could not be accounted for by associative learning paradigms such as the Rescorla-Wagner model as they stood.

Van Hamme and Wasserman (1994) later performed a study that shed further light upon the dynamic nature of retrospective revaluation. Under the framework of assessing foods that could case allergic reactions, they established a common cue (food) X with two other cues so as to establish two compound cues with X in common, i.e., AX and BX. While it came as no surprise that AX+ trials increased and AX- trials decreased the predictive rating of A (where '+' and '-' indicated the presence and absence of an allergic reaction, respectively), they found that the rating of B actually decreased after AX+ trials, and increased after AX- trials. Thus, they concluded that a within-compound cue that is absent during subsequent training will undergo a change in causal judgement in the direction that is opposite of the other within-compound cue that is present. In addition, they proposed a formal revision of the Rescorla-Wagner model so as to account for retrospective revaluation: The α term for a particular compound cue should have a

negative value in trials in which it is absent, and a positive value in trials in which it is present. Thus, if a within-compound cue is trained alone in a second phase in the presence of an outcome after being trained in compound in the first phase, the α term of the absent within-compound cue would be negative, while the error term ($\lambda - \Sigma V$) would be positive because λ will be 1 (because the outcome is present). As a result, the associative strength of the absent within-compound cue will decrease. Similarly, if the within-compound cue is trained without an outcome in the second phase, then the α term would be negative, but the ($\lambda - \Sigma V$) term will also be negative because λ will be 0 in the absence of an outcome. As such, the associative strength of the absent within-compound cue will increase (the former and latter effects have come to be known as discounting and augmentation, respectively; Castro, Wasserman, & Matute, 2009). With this revision, the Rescorla-Wagner model remained a robust model of associative learning.

Not surprisingly, there was contention as to whether or not retrospective revaluation effects could be in nonhuman animals. Another well-known example of the retrospective revaluation effect is the backward blocking phenomenon, where compound cue training (i.e., AB+) is followed by further training with a within-compound cue alone (i.e., A+). When tested on B, less conditioned responding is typically observed, relative to a group that had not received the A+ training. Using rats on a conditioned lick suppression task, Kawai, Nishida, and Imada (1998) gave separate groups of rats the forward as well as the backward blocking procedure, but were only able to obtain the forward blocking effect. Indeed, there were suggestions that the retrospective revaluation effect simply could not be observed in nonhuman animals (e.g., see Holland, 1999; where it was concluded that blocking and overshadowing – another cue competition phenomenon – were due to deficits in acquisition, as opposed to deficits in performance). However, Miller and Matute (1996) performed an experiment which suggested

that retrospective revaluation indeed existed in nonhuman animals – just that the biological significance of the cues involved also played an important role. They first trained a compound cue with a neutral surrogate stimulus (i.e., AX→S). Following this, a within-compound cue was paired again with the surrogate stimulus (i.e., A→S) for one group, while a control group received no such training. The surrogate stimulus was then paired with a biologically significant US (footshock) before all subjects were tested on X alone. Responding to X was weaker in the backward blocking group relative to the control group, thus obtaining the backward blocking effect. Additionally, it is important to note that numerous studies have demonstrated retrospective revaluation effects that do not require the involvement of a biologically significant stimulus, such as recovery of responding to an overshadowed stimulus after the overshadowing stimulus has been extinguished (e.g., Kaufman & Bolles, 1982; Matzel, Schachtman, & Miller, 1985), or the recovery of blocking following extinction of the blocking CS (e.g., Blaisdell, Gunther, & Miller, 1999).

As such, evidence exists to suggest that even rats can alter the perceived likelihood of an outcome following a within-compound cue, based on prior experience in which the other within-compound cue is absent. This would appear analogous to the evidence gathered from studies with humans as reviewed above, and if so, may suggest that true human causal reasoning could actually be broken down to simple associative processes that can be explained by models such as the Rescorla-Wagner model. However, the causal structures that have been investigated in the literature reviewed above (both human and nonhuman) are extremely simple compared to the extent of true human causal reasoning on a daily basis. Clearly, more rigorous tests are needed to ascertain if causal learning is indeed adequately governed by associative theories.

1.4: Nonassociative Theories

In stark contrast to Hume, the German philosopher Immanuel Kant (1724 – 1804) posited that there exists some a priori knowledge about cause-and-effect relationships. In this view, the percept of causality is an intuitive product of inductive reasoning processes, built from relevant contingency information that is gathered from the environment through experience (Kant, 1781/1997). Implicit in this notion is the idea that we have *some* innate idea of the existence of mechanisms through which a cause may bring about an effect. Thus, unlike the associative accounts which attribute causal judgement to a rather automatic and bottom-up nature, the Kantian view embraces a more top-down perspective that is more rational in essence.

Central to this theory is the assumption that causal judgements are made only after we have been provided with all pertinent information with regards to the contingency and covariation between events. A traditional referent for the processing of statistical contingency based on covariation is the ΔP model (Jenkins & Ward, 1965; Van Hamme & Wasserman, 1994):

$$\Delta P = p(E|C) - p(E|\sim C)$$

where the change in contingency (ΔP) is the difference between the probability of the event given the presence of a candidate cause, $p(E|C)$, and the probability of the event given the nonoccurrence of the candidate cause, $p(E|\sim C)$. If ΔP is positive, then an excitatory association is formed between E and C; and if ΔP is negative, an inhibitory association is instead formed. Note that in the absence of the Kantian assumption that there exists some a priori knowledge about cause-and-effect relationships, the ΔP model is mathematically similar to the original Rescorla-Wagner model. It would not be difficult to imagine why ΔP is, by itself, not a sufficient means through which to accurately construct causal inferences in the world. Cheng (1997) provided a

good example to illustrate this shortcoming: A rooster may crow every morning before the sun rises, but we would not draw a causal connection between the rooster's crowing and sunrise. However, the ΔP model would be insensitive to this. Contingency and covariation alone are insufficient tools with which to determine if an agent has the causal power to bring about an effect. Thus, Cheng (1997) proposed that the generative and preventive power of candidate causes also need to be taken into consideration, and introduced the power PC theory (short for a causal power theory of the probability contrast model), which factors in the base rate probability of the effect occurring under the absence of the candidate cause:

$$p = \frac{P(E|C) - P(E|\sim C)}{1 - P(E|\sim C)}$$

where *p* is the causal power, the numerator denotes the statistical contingency between events (which is exactly ΔP as mentioned above), and the denominator denotes the base rate probability of the outcome. Thus, if the outcome always occurs in the absence of the candidate cause, then P(E|~C) would equal to 1, and the equation will be undefined. Consequently, it would be impossible to assess the causal power of the candidate cause. In the case where the outcome never occurs in the absence of the candidate cause, the equation will reduce to the ΔP model.

The power PC model actually makes a slightly different prediction than the revised Rescorla-Wagner model when it comes to retrospective revaluation (Castro et al., 2009). In the case of augmentation, the power PC model makes the same prediction as the revised Rescorla-Wagner model: If two cues are trained in compound with an outcome, followed by the presentation of one of the within-compound cues in the absence of the outcome (i.e., AX+; followed by A-), the perceived causal power of A will become dubious. As such, one should be able to infer that it must have been X that brought about the outcome, and confidence to attribute the outcome to X would increase. However, in the case of discounting, if AX+ is followed by

A+, the power PC model predicts that during the presentations of A+ alone, the base rate probability of X has not changed because there has been no explicit circumstance to warrant any meaningful revaluation. To illustrate the rationale behind this prediction, Cheng (1997) provided the following analogy: Suppose a patient consults with an allergist to determine if he is allergic to a certain Food X. The allergist will make multiple punctures on his skin in order to deposit various samples of suspected allergens – if hives develop on any particular puncture spot, it would imply that the patient is allergic to that particular allergen. Suppose the allergist observes that hives develop in every spot containing Food X. He would then conclude that the patient is allergic to Food X, since P(Hives|Food X) = 1. However, if the allergist also observes that hives have broken out on every other puncture spot as well (even that for a neutral control sample like saline), it would imply that the act of puncturing the skin alone would be sufficient to trigger an allergic reaction, since P(Hives|~Food X) = 1 as well. The allergist would conclude that the patient is allergic to punctures of the skin. At the same time, it is still entirely possible that the patient is allergic to Food X; it just happened to be unobservable from the test. Putting this back into the framework of discounting: If AX+ presentations are followed by A+, there is insufficient information to gauge if X alone can still bring about the outcome. As a result, the perceived causal power of X would remain unchanged from the AX+ trials. This is in contrast to predictions made via the revised Rescorla-Wagner model, where a discounting of X should occur. As an alternate explanation for the lower causal ratings of X from subjects as reflected in the empirical data, Cheng (1997) argued that it could have merely been due to uncertainty about the casual status of X. In sum, one substantial difference between the power PC theory and the revised Rescorla-Wagner model is that both discounting and augmentation should occur in the latter, but only augmentation should occur in the former.

Wasserman and Berglan (1998) performed an experiment that coincidentally put this difference to empirical test on humans in a causal judgment task where different types of food were paired together, and were either followed by an allergic reaction or no reaction. Subjects were given training with AW+/BX+/CY+ in Phase 1 (where each letter represented a different food, and '+' indicates an allergic reaction that followed; they also included a fourth condition that is irrelevant to this discussion and will hence be omitted). In Phase 2, they were given A+ and C- trials (where '-' indicates no allergic reaction; incidentally, they also included filler pairings that were of no theoretical relevance, which will hence be omitted from discussion). Note that Food B was not subjected to any trials in Phase 2, which meant that no further information for Food X (nor Food B) was provided outside of Phase 1. The subjects were then asked to rate the probabilities that Foods W, X, and Y were the causes of the allergic reactions. According to the power PC model, Foods W and X should be rated equally low due to low confidence in evaluation, because despite the A+ trials in Phase 2, no specific information was given to evaluate the causal power of Food W (as was the case for Food X). Their results revealed that ratings of Food Y were higher than that of Food X, demonstrating an expected augmentation effect. More interestingly however, ratings of Food W were actually revealed to be lower than Food X, which indicated a clear discounting effect. Hence, their results reflected more support for the revised Rescorla-Wagner model than the power PC model (this procedure and effect was later replicated by Castro et al., 2009).

1.5: Interventions in Causal Learning

The models mentioned thus far share one commonality, in that they require prior information that distinguishes between potential causes and effects. Only with this information can they begin calculating the associative strengths between causes and effects, or the causal

power of a candidate cause to bring about an effect. In and of themselves, they are incapable of distinguishing between causes and effects merely from observations of events. One consequence of this is that these models are insensitive to cause and effects that occur at the same time, due to the lack of perceivable contiguity that is usually used to discern between cause and effect. For instance, oxytocin has been implicated in pair-bonding from studies on prairie voles (e.g., Insel & Hulihan, 1995). But does the hormone result in monogamous pair-bonding? Or does the bonding result in secretion of the hormone? Because both hormone secretion and pair-bonding appear to occur concurrently, it is difficult to correctly determine which event is the cause and which event is the effect (i.e., correlation is not causation). One would have to perform appropriate interventions for proper diagnosis, such as injecting oxytocin and observing if pair-bonding follows. Essentially, this would be the scientific process. Another scenario wherein an intervention would be integral in the diagnosis of a causal model would be a common-cause model, in which one cause results in multiple outcomes. Take, for instance, the following example as mentioned by Sawa (2009): Suppose you attend a party (Event X). You drink too much wine (Event Y), and you become sleepy (Event Z). There would be two possible causal structures – the party caused you to drink too much wine, which caused you to become sleepy (i.e., X→Y→Z); or the party caused you to drink too much wine, but the party also wore you out with activities, and you become sleepy as a result (i.e., X→Y; X→Z). The former scenario would adhere to a causal chain model, whereas the latter would embody a common cause model. How would you diagnose which is the correct causal structure (assuming that drinking wine and becoming sleepy occur too close together in time for any perceivable temporal difference to be detected)? The simplest method would be to deliberately drink wine outside of a party and see if sleepiness ensues, because wine causes sleepiness in the causal chain model, whereas wine and

sleepiness are independent of each other in the common cause model. If after the intervention (via drinking wine), you still do not feel sleepy, then you would be fairly confident that the causal relationship between wine and sleepiness is spurious. The common cause model would be adopted as such.

The modified Rescorla-Wagner and Power PC models do not discern between causes that are triggered by intervention, or by other independent causes. Consider this next scenario, as described by Blaisdell, Leising, Sawa, & Waldmann (2006): Atmospheric pressure, probability of rain, and barometric readings are three covarying events. A drop in atmospheric pressure causes a drop in barometric reading. At the same time, it also increases the probability of rain. Hence, atmospheric pressure is the common cause of lower barometric reading, as well as a higher probability of rain. While there is a positive correlation between barometric reading and probability of rain, the causal relationship between them is spurious at best, for if one manipulated the barometer, the probability of rain would still remain unchanged. According to associative theories, if one had merely observed a change in barometric reading, its positive correlation relationship with the probability of rain should be retrieved. However, in the scenario involving intervention on the barometer, associative theories would predict the same retrieval. Thus, if causal relationships were indeed reducible to mere associations acquired from the observed correlation between the barometer and the probability of rain, one would be led to the conclusion that tinkering with the barometer would also bring about a change in the probability of rain. Clearly, this is not what one would anticipate, and it is important to note that one does not need to have undergone prior instrumental training with the barometer to arrive at the correct conclusion. Thus, there is something qualitatively different about seeing and doing when applied to such a causal model, producing diverging outcomes that cannot be explained by associative

theories which do not distinguish between covariation and causation (and hence contributing to the ubiquitous phrase "correlation does not imply causation").

One theory that can explain the difference between seeing and doing is the causal Bayes net theory, which has recently been put forth as a theory of causal cognition (e.g., Gopnik, Glymore, Sobel, Schulz, & Kushnir, 2004). Originally developed by computer scientists and statisticians as a rational tool for causal discovery, Bayesian networks represent relationships between events in the world through nodes and connectors that make up directed acyclic graphs, where the nodes represent variables (or events), the connectors are directional and represent causal dependencies between linked variables (usually represented by arrows), and the strengths of these dependencies are represented by conditional probabilities (Pearl, 1988). It operates on the Markov assumption, which states that for any variable X in the network that does not contain any effects of X, X is independent of all other variables, conditional on the variable(s) that is/are the direct cause/causes of X. The resulting network that is formed represents a unified graphical causal structure, and can be used as computational architecture with which to direct data flow towards accurate predictions and effective interventions. The essential advantage of causal Bayes nets is that the unified causal structures can be inferred from passive observation, intervention, and prior knowledge. Along the veins of the causal Bayes nets theory, Waldmann & Holyoak developed the causal model theory, which can be viewed as the psychological variant of the Bayes net theory (Waldmann & Holyoak, 1992). According to this view, people form representations of complex causal models with directed causal links, not unlike the directed links in causal Bayes nets. Central to this theory are three assumptions: (a) The directed links are asymmetric, which characterizes the strong predisposition by people to ascribe effects to causes and not vice-versa, even if effects are presented prior to their causes; (b) The strength of the

causal links represent the contingency between the causes and the effects; and (c) despite the asymmetrical links, people are able to reason both predictively (from a cause to its likely effects) and diagnostically (from an effect to its likely cause or causes). In contrast to associative models that attribute causal learning to bottom-up processes, the causal Bayes nets and causal model theories postulate top-down learning as the modus operandi for the acquisition of causal knowledge.

Figure 1.1 (top panel) illustrates the atmospheric pressure/barometer/rain example in graphical form consistent with the causal model theory. As discussed previously, if we understood the underlying causal structure, we would know that manipulating the barometer would not alter the probability of rain. The causal model theory (and by extension, the causal Bayes net theory) can capture this effect by what Pearl (2000) termed as "graph surgery" (Figure 1.1, bottom panel), which entails a temporary deletion of the causal arrow between atmospheric pressure and the barometer to reflect the statistical independence between these two variables. That is, if the cause of the effect (i.e., the deterministic manipulation) is completely independent of all the other variables in the causal structure, then the effect is viewed fully as an outcome of the intervention instead of its usual cause, resulting in the discounting of atmospheric pressure. In the case of merely observing a change in barometric reading, we should diagnostically infer that atmospheric pressure has probably changed. As such, we should predict that the probability of rain – the other effect of atmospheric pressure change – would also change.

In the framework of causal reasoning, the causal model theory explains how we are able to choose appropriate actions after learning causal structures, instead of engaging in meaningless behavior (such as manipulating the barometer in an attempt to change the weather). Recent research by Waldmann and Hagmayer (2005) has shown that humans are able to distinguish

between observation and intervention upon different causal models without any prior instrumental training. Using fictitious hormones in chimpanzees, participants either learned about a causal chain or a common cause model. In the causal chain model (Figure 1.2, top panel), Sonin causes the secretion of Pixin, and Pixin in turn causes the secretion of Xanthan. In the common cause model, Pixin is the causes the secretion of both Sonin and Xanthan (Figure 1.2, bottom panel). During the learning phase, participants were presented with probabilistic data that reflected the respective causal models. The participants were then presented with questions asking them to imagine another set of chimpanzees whose levels of Sonin had increased or decreased artificially (i.e., intervened), or whose levels of Sonin just happened to be elevated or normal (i.e., observed). Their task was to gauge the number of chimpanzees who had increased levels of Xanthan. According to the causal model theory, increased levels of Sonin in the causal chain model should result in increased levels of Pixin and therefore Xanthan, whether the Sonin levels had increased via intervention or merely observed to have increased. In the case of the common cause model, if Sonin was merely observed to have increased, participants should reason that its cause – Pixin – must have also increased. As a result, levels of Xanthan should have also increased. In contrast, if levels of Sonin had been increased via intervention, participants should have attributed its increase to the intervention, thereby resulting in graph surgery of the causal link between Pixin and Sonin. Just as intervening on a barometer should produce no change in the weather, the levels of Xanthan should be predicted to remain unchanged as such. Their results corroborated this pattern of results. This sensitivity to the difference between interventions and observations pertaining to different causal structures were also observed in young children (Gopnik et al., 2004).

1.6: Causal Reasoning in Nonhuman Animals

While it may be no surprise that human adults and children are capable of reasoning causally (e.g., Waldmann & Hagmayer, 2005; Gopnik et al. 2004; Waldmann & Holyoak 1992; Leslie 1982; Leslie 1984; Leslie & Kreeble, 1987), much contention has been made about such capabilities in non-human animals, wherein studies have generally showed a lack of understanding of physical causal mechanisms that underpin tool use (e.g., Visalberghi & Limongelli, 1994; Limongelli, Boysen, & Visalberghi, 1995; Tomasello & Call, 1997; Povinelli, 2000). Blaisdell et al. (2006) conducted a series of studies to investigate rats' ability to reason about causal interventions. In their Experiment 1, they established a causal structure as follows (See Figure 1.3): Through Pavlovian training, a 10-second flashing Light was paired with a 10-second Tone (i.e., Light→Tone), and on separate trials, the same light was paired with Food in the form of a 10-second delivery of sucrose solution (i.e., Light→Food). Thus, the Light was established as a common cause of both the Tone and Food for the Common-Cause condition (i.e., Tone←Light→Food; the two pairings were trained separately so as not to establish a direct link between Tone and Food). As a control, a direct cause component was also incorporated into the causal model, in which a 10-second white noise was simultaneously paired with a 10-second presentation of Food (i.e., Noise+Food; the Direct-Cause condition). The Tone and Noise were counterbalanced across subjects, but only the aforementioned causal structure (i.e., Tone←Light→Food←Noise as in Figure 1.3) will be discussed for the sake of simplicity.

To determine if rats could correctly differentiate between the roles of observation and intervention in causal relations, a lever that was not present during training was inserted into the conditioning chambers at test. For half the subjects in the Common-Cause condition, pressing the lever brought about the presentation of Tone (i.e., LP→Tone; lever presses made during the

presentation of Tone were of no consequence), while the other half of the subjects in the same Common-Cause condition merely observed the Tone independently of lever presses as described above (the levers in the chambers of such subjects were non-functional). Subjects in the Direct Cause condition received similar trials: For half of them, pressing the lever brought about the presentation of Noise (i.e., LP→Noise), while the remaining half merely observed Noise. The stimulus presentations for the groups that merely observed either Tone or Noise (i.e., subjects that received stimuli independent of lever presses) were yoked to that of the respective groups that intervened on the stimuli (i.e., subjects that were allowed to invoke either Tone or Noise upon pressing on the lever), and thus the yoked subjects that observed the cues from both causal models (Common-Cause and Direct-Cause) experienced the same number and distribution of events as did their counterparts in the intervention condition. Expectation of Food was measured by monitoring feeder activity in the food niche where Food was delivered during training – whenever a rat placed its nose into the food niche (i.e. a nose poke), it disrupted an infrared photo beam that projected across the entrance of the niche. Their results revealed that, for rats in the Common-Cause condition, the group that produced the stimulus (Tone) through lever presses searched for Food in the food niche significantly less than the group that had merely observed the Tone. For rats in the Direct-Cause condition, the group that produced the stimulus (Noise) through lever presses elicited an amount of nose pokes that did not differ significantly from that of the group that had merely observed the Noise (See Figure 1.4).

The pattern of results was consistent with the notion that the rats had successfully learned and integrated the causal links into a coherent causal structure. The higher number of nose pokes in the Common-Cause observation test condition suggests that they understood the common cause component, and reasoned diagnostically from the Tone to the Light, and predictively from

the Light to the food. However, for rats that intervened on the Tone (i.e., LP→Tone), there was comparatively less nose poking, which suggests that they attributed the effect of the lever press (i.e., Tone) to their own actions and therefore discounted the common cause (Light), and thus did not expect Food to be present. This pattern of results would be analogous to the aforementioned barometer example, where subjects that tamper with the barometer should not also expect a change in weather. On the other hand, for the rats in the Direct-Cause condition, there was no difference in rates of nose poking between the rats that intervened on Noise and those that merely observed Noise. This was a predicted outcome, as the direct causal relationship between Noise and Food during training should have led the rats to expect Food whether Noise was observed or intervened on at test.

The data obtained by Blaisdell et al. (2006) suggested strongly that the rats were able to correctly distinguish between interventions and observations within the context of causal events. Furthermore, it also implied that rats were capable of understanding and discerning between common cause and direct cause models. This would explain the different patterns in nose poking rates between groups that intervened on and observed Tone from the Common-Cause condition and Noise from the Direct-Cause condition. As such, there seemed to be compelling evidence that rats are capable of reasoning causally to a degree. Moreover, other previous studies have suggested that stimulus-outcome (Pavlovian) and action-outcome (instrumental) modes of learning interact with each other in accordance to the nature of the cause of the response (Dickinson & Balleine, 2002), and the data from Blaisdell et al. (2006) showed that rats are capable of going beyond mere interactions between these modes of learning.

As far as models of causal learning go, the causal model theory comes closest to explaining the pattern of results obtained by Blaisdell et al. (2006). According to this theory, the

presence of alternative causes (i.e., the lever press) upon a common effect (i.e., the Tone) results in the discounting of its other previously established cause or causes (i.e., the Light). Furthermore, the degree of this discounting should be maximal if the alternative cause is perceived as being a deterministic cause of the effect (i.e., full attribution of the cause to the intervention), thereby establishing statistical independence from its previously established causes. The hypothesis that can be drawn is that actions are special, in that they create the impression of causality very readily, which imparts the sense of determinism necessary for the discounting of non-active causes (recall that there was no instrumental training involving the lever prior to testing).

1.7: The Single-effect Learning Model

While the test data obtained by Blaisdell et al. (2006) are consistent with the causal model theory, the same could not be said about that from the learning phase. If the rats had successfully learned and integrated the individual causal links into a unified causal model, then they should have employed the full covariational information between all the nodes (events) when making causal judgments. During the learning phase of the common cause, Light→Tone and Light→Food pairings were given on separate trials, thus establishing a negative contingency between Tone and Food. This would predict that rats that observed Tone should not have expected Food presentations, which was not what the actual data reflected.

Waldmann, Cheng, Hagmayer, & Blaisdell (2008) thus proposed the single-effect learning model, an attention-based theory which posits that during learning of causal structures, the focus is on single effects. According to this theory, the mental representation of the causal structure is updated one link at a time. This implies that due to attention limitations, when the rats learned the Light→Food causal link, the Light→Tone link was ignored. As a result, they

were able to learn the Light→Tone and Light→Food causal links without drawing assumptions about how Tone and Food were related to each other. This account would be consistent with data from second-order conditioning experiments, where animals do not appear to notice the negative correlation between indirectly related events until they have experienced a larger number of trials, more than the number of training trials provided by Blaisdell and colleagues (Yin, Barnet, & Miller, 1994; As a tangent, perhaps subjects would have noticed the negative correlation between Tone and Food if the training phase was to be extended).

During the test phase, rats that observed the Tone reasoned link by link from Tone to Light, and in turn from Light to Food, effectively leading to behavior that was consistent with a positive correlation between Tone and Food. Rats that intervened on the Tone appeared to view their own actions as deterministic (i.e., attributed the Tone to their own actions) and thus independent of its alternative cause, resulting in discounting of Light. This led to the decreased expectation of Food, the other outcome of Light.

Thus, unlike the causal Bayes net theory, the single-effect theory presents a much simpler and parsimonious account of causal learning. Since at a glance, the task of simultaneously parsing down complex networks of causes and effects into a coherent mental representation might seem too challenging for rats (and perhaps even for humans), the simplicity of the single-effect model makes it an attractive candidate for explaining causal learning.

1.8: Flexibility in Causal Reasoning

Stemming of the findings of Blaisdell et al. (2006), Leising, Wong, Waldmann, & Blaisdell (2008; Experiment 3) subjected rats to a more stringent test of causal reasoning. If a person tinkers with a barometer on one occasion and does not expect a corresponding change in weather, but later observes a change in barometric reading on a subsequent occasion (assuming

of course, that the barometer was not damaged), (s)he would still expect a change in weather; because if that person truly understood the causal structure, (s)he would reason that intervening on an outcome does not alter its cause, nor the causal model. Likewise, a person who observes a change in barometric reading on one occasion and then tampers with the barometer on a subsequent occasion should expect a change in weather on the first occasion, but not expect a change in weather on the second. Leising and colleagues replicated the basic common cause design in the Blaisdell et al. (2006) study, but tested two groups of rats in two possible orders: One group intervened to invoke Tone by lever pressing on the first test day but merely observed Tone on the subsequent test day, while the other group received the reverse order. They found that regardless of order, rats that intervened on the Tone always nose poked less than rats that observed the Tone. This provided another piece of evidence suggesting that rats were indeed capable of learning and operating around a common causal model correctly. Furthermore, they even appeared capable of reasoning flexibly with the causal model to a seemingly sophisticated degree.

1.9: Possible Alternative Interpretations

1.9.1: Retroactive Interference. However, an alternative account for the pattern of results was also possible obtained by Blaisdell et al. (2006). While causal model theories hold that causal learning occurs in a top-down manner from previously-experienced cause-and-effect pairings, associative learning theories attribute causal learning to a bottom-up mechanism that is guided simply by event pairings (Tangen & Allan, 2004). Moreover, just because current associative theories do not distinguish between events that are intervened upon or merely observed does not mean that causal learning is most definitely not governed by associative processes. For example, the fact that the Rescorla-Wagner model as it stands cannot account for

sensory preconditioning (due to the absence of a US) does not necessarily mean that sensory preconditioning must be a nonassociative process. Thusly, in terms of associative theories, the results obtained by Blaisdell et al. (2006) could be interpreted as the formation of a LP→Tone association in the lever press group resulting in two antecedent cues (i.e., LP and Light) being paired with the same outcome (i.e., Tone) that interfered with each other for behavioral control – a phenomenon called retroactive interference. Matute & Pineño (1998) demonstrated retroactive interference in humans using a procedure designed as a computer game. During training, two cue-outcome associations were established one after another in separate phases of training, both of which signaled the same outcome (i.e., Cue A→Outcome in Phase 1, followed by Cue B→Outcome in Phase 2). In another group, the orders were reversed (i.e., B→Outcome in Phase 1, followed by A→Outcome in Phase 2). During testing, the former group (i.e., the A→Outcome followed by B→Outcome group) showed decreased responding to Cue A. This effect did not occur when the latter group (i.e., the B→Outcome followed by A→Outcome group) was tested with the same Cue A. Matute & Pineño thus suggested that the more recently-acquired association retroactively interfered with retrieval of the association learned earlier. This effect has been replicated in rats (Escobar, Matute, & Miller, 2001). Retroactive interference might also explain the results of Blaisdell et al. (2006), in that the LP→Tone relationship experienced at test could have retroactively interfered with retrieval of the previously established Light→Tone association. As a result, less nose poking was observed in the intervene group than in the observe group for which no LP→Tone relationship was experienced at test. Retroactive interference of the Light→Tone association in the intervention group would prevent a rise in the expectation of Food. To rule out this alternative associative account, Leising, Wong, Waldmann, and Blaisdell (2008) replicated the Blaisdell et al. (2006) study (with just the common cause component, i.e.,

Tone←Light→Food) and introduced another group with an exogenous cue (A 10-s click train; i.e., Click→Tone) that, if the retroactive interference alternative explanation were true, should have also attenuated responding to T (i.e., the LP→Tone from the intervene group was analogous to the Click→Tone for this third condition). A group that only observed the Tone during testing was also included, but was split into two subgroups in order to control for the difference in onset of the Tone between the LP→Tone group and the Click→Tone group (since the Click train lasted for 10-s, while an average LP typically ranged between 0.4 s to 2 s; the data for these two subgroups were found to not statistically differ from one another, and were pooled for analysis). Their results indicated that subjects in the group with the exogenous cue (i.e., Click) responded no differently than those in the group that observed the Tone, and the number of nose pokes for these two groups were significantly higher than the group that intervened on the Tone (See Figure 1.5). They also addressed an important caveat: The fact the Click train was considerably longer than the average length of an LP could have served as a potential confound. This was addressed in their Experiment 2, where they replicated the previous experiment; but during the test session, they equated the length of the LP of the intervening rat with the length of the Click train that preceded the Tone for the group with the exogenous cue (i.e., the duration of the LP and Click was therefore identical for the groups that received LP→Tone and Click→Tone). The pattern of results did not change – feeder activity was comparatively higher for the group that observed the Tone and the group that received the exogenous cue before the Tone as compared to the group that intervened on the Tone. Responses between the group that observed the Tone and the group that received Click→Tone did not differ. Taken together, this suggested that retroactive interference was probably not responsible for the pattern of the results seen in Blaisdell et al. (2006).

1.9.2: Response Competition. More recently, Dwyer, Starns, and Honey (2009) argued that the pattern of results obtained by Blaisdell et al. (2006) and Leising et al. (2008) could have been due to mere response competition instead of a faculty as complex as causal reasoning. Because Blaisdell et al. (2006) did not record the number of lever presses elicited during Tone, Dwyer and colleagues replicated the Blaisdell et al. (2006) experiments, but analyzed the number of lever presses throughout the entire test session. Simply put, Dwyer and colleagues contended that rats in the Intervene condition could not be at the lever and the food niche at the same time. Rats would have had less time available to spend at the food niche after pressing the lever, and this factor probably contributed to a lower number of nose pokes compared to rats in the Observe condition, which did not have to spend any time at the lever.

To assess this argument, Wong & Blaisdell (Unpublished data) modified the common causal model by creating a situation in which nose poke responses would be divergently different, depending on whether a rat is in the Intervene condition or the Observe condition. Rats were first trained with periodic Food delivery in the chambers via a variable time schedule (i.e., rats should thus keep searching for food while they were in the chambers). They were then trained with the same common cause model used in Blaisdell et al. (2006), except that Food was changed to a 10-s train of footshocks (Shock; five 0.5 s footshocks spaced 2 s apart). In other words, Light was established as a common cause of Tone and Shock (i.e., Tone←Light→Shock, see Figure 1.6). Thus, a situation was created wherein rats that intervened by pressing the lever during test to invoke the Tone should attribute it to their own actions and therefore not expect the common cause (Light) to have occurred, which would have subsequently invoked the other cause (Shock). As a result, they should not anticipate Shock, and spend more time searching at the food niche (i.e., low levels of search suppression). In contrast, rats in the Observe condition would

infer that since Tone occurred, Light must have occurred, which also implies that Shock would occur. As such, they should freeze in anticipation of the shock, and suppress nose poking into the food niche. This pattern of results were exactly what was obtained (see Figure 1.7): Rats that pressed the lever for the Tone actually spent more time at the food niche than rats that did not have to interact with the lever for the Tone, thus serving as compelling evidence that response competition was probably not responsible for the pattern of results obtained by Blaisdell et al. (2006). Moreover (and perhaps more importantly), it also serves to demonstrate that the ability of rats to reason causally extends to aversive paradigms as well.

1.10: Overview of Current Project

With the many studies already performed in the purely behavioral realm, the next logical step would be to perform experiments that delve deeper into our understanding of the neural underpinnings of causal interventions. Based on previous studies, when rats were given an opportunity to intervene to produce one outcome of a common cause, they seem to attribute that outcome to their own actions, as opposed to the common cause. Consequently, they do not expect the other outcome to also occur (i.e., the causal intervention effect), and thus discount it. It would thus make logical sense to suppose that causal interventions require the actor to be sensitive to the outcome of their actions, and if this sensitivity was somehow to be abolished (either by behavioral or neural means), we would expect these rats to attribute the outcome instead to the common cause, and therefore expect the other outcome to occur. Indeed, this dissertation presents a series of experiments that were performed to gain more insight into this hypothesis. As a behavioral means of eliminating action-outcome sensitivity, we pretrained a group of rats to lever-press in a manner that was no longer goal-directed; and as a neural means of establishing the same goal, we made lesions in the part of the brain that has been shown to be

necessary for the acquisition and expression of action-outcome associations in instrumental conditioning (i.e., the posterior dorsomedial striatum). The specific aims are listed as follows:

Aim 1 (Chapter 2). Prior to the experiments proper, we needed to first ensure that we were able to obtain some basic effects that subsequent experiments would be based upon. Specifically, we needed to be sure that we could replicate the causal intervention effect (as in Blaisdell et al., 2006 and Leising et al., 2008), as well as develop a reliable protocol that will produce instrumental behavior that is either goal-driven (i.e., goal-directed) or outcome-insensitive (i.e., habitual) in rats. Once the appropriate experimental parameters necessary to produce these effects have been determined, they were used in the designs of all subsequent experiments.

Aim 2 (Chapter 3). A behavioral experiment was conducted to investigate if causal interventions depended on outcome-sensitive, goal-directed behavior (which would implicate the brain regions known to drive this behavior). We first trained rats to lever-press in a manner that was either outcome-sensitive (i.e., goal-directed) or outcome-insensitive (i.e., habitual). Following, we trained them on a common cause model (i.e., Tone ← Light → Food) and gave the intervene/observe test as in Blaisdell et al., 2006 and Leising et al., 2008). If effective causal interventions required sensitivity of actions to outcomes, then rats trained to lever-press habitually should no longer attribute the Tone to their own actions, and thus show minimal discounting of Food (as all rats that merely observed the Tone should). Conversely, rats trained to lever-press in a manner that was goal-directed should still show the causal intervention effect and discount Food.

Aim 3 (Chapter 4). As a more direct way to target the region of the brain that is implicated in action-outcome sensitivity, a pre-training lesion study was performed, where the

posterior dorsomedial striatum (pDMS) was targeted. Along with sham controls, rats were then given training on a common-cause model and subsequently tested as before. Given the role of pDMS in the acquisition of action-outcome associations during instrumental task learning (Yin, Ostlund, Knowlton, & Balleine, 2005b), and if the effectiveness of interventions depends on knowledge of outcomes from the action of lever pressing, then pDMS-lesioned rats but not sham lesioned rats should fail to attribute the Tone to their own actions, thus producing high rates of nose-poking into the Food niche, despite their intervention on the Tone. All subjects that merely observe the Tone should not show discounting of Food.

Figures and Tables

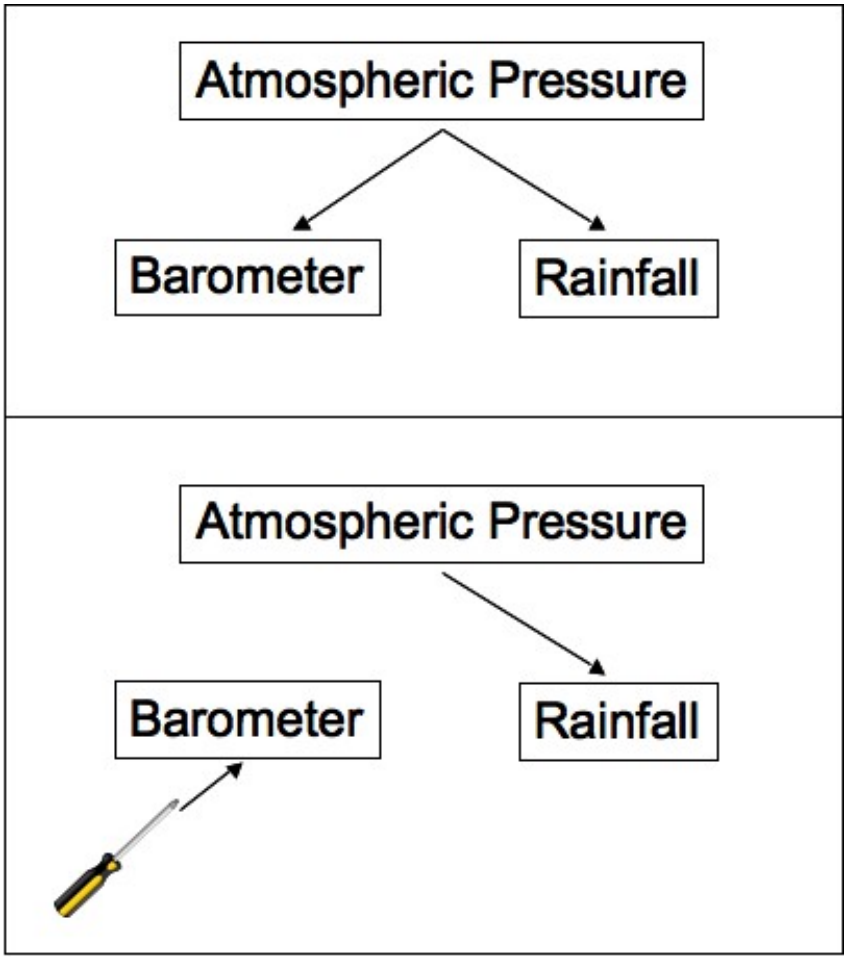

Figure 1.1. (Top Panel) A common cause structure as represented by the causal Bayes nets and causal model theories. The arrows represent causal directions from cause to effect. *(Bottom Panel)* Intervention on the barometer (represented by the screwdriver) results in a temporary deletion of the causal link between Atmospheric Pressure and Barometer (Graph surgery).

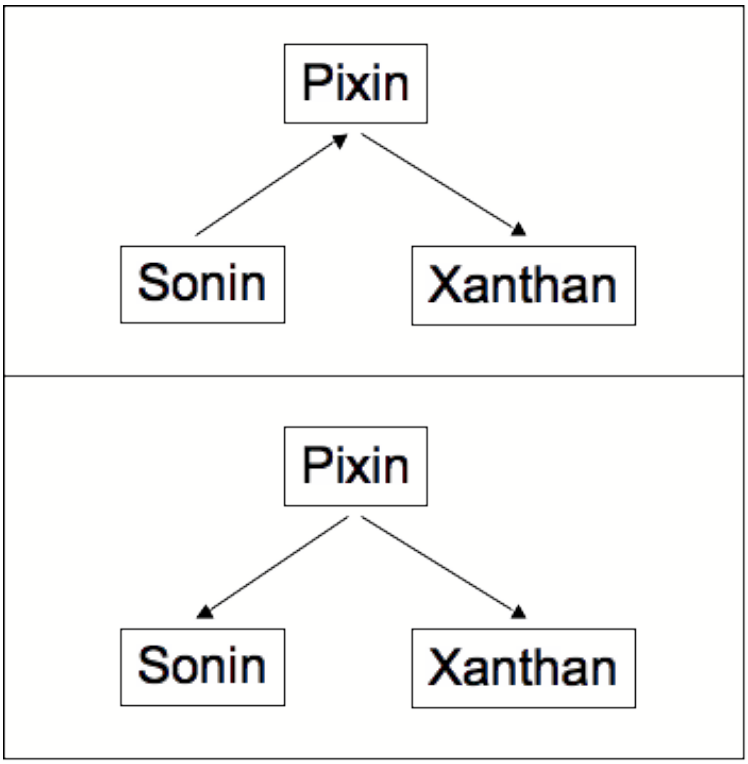

Figure 1.2. (Top Panel) A causal chain model, and *(Bottom Panel)* a common cause model involving fictitious hormones. The arrows represent causal directions from cause to effect.

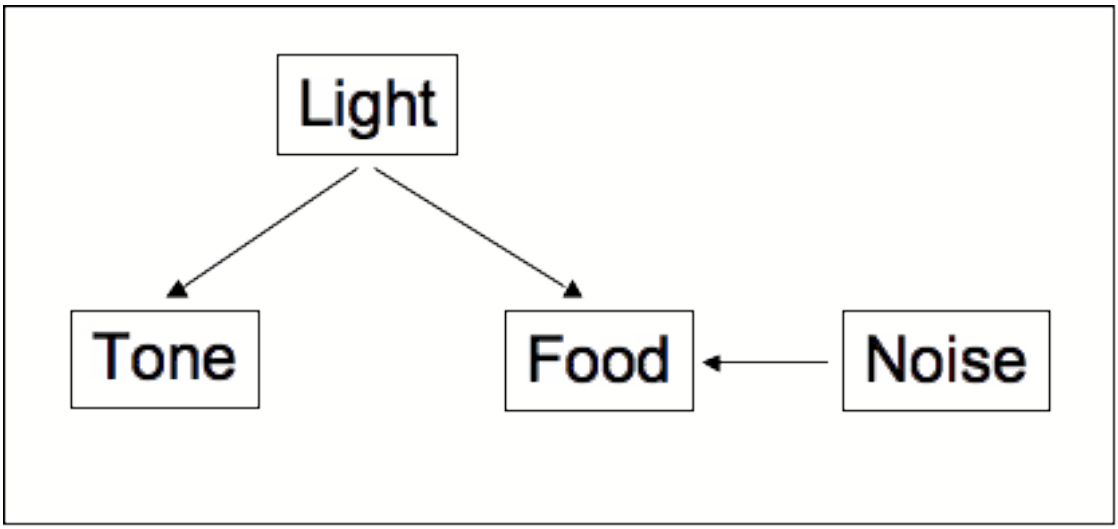

Figure 1.3. The causal model used in Blaisdell, Sawa, Leising, & Waldmann (2006), consisting of both a common cause (Tone←Light→Food) and a direct cause (Noise→Food) component. The arrows represent causal directions from cause to effect.

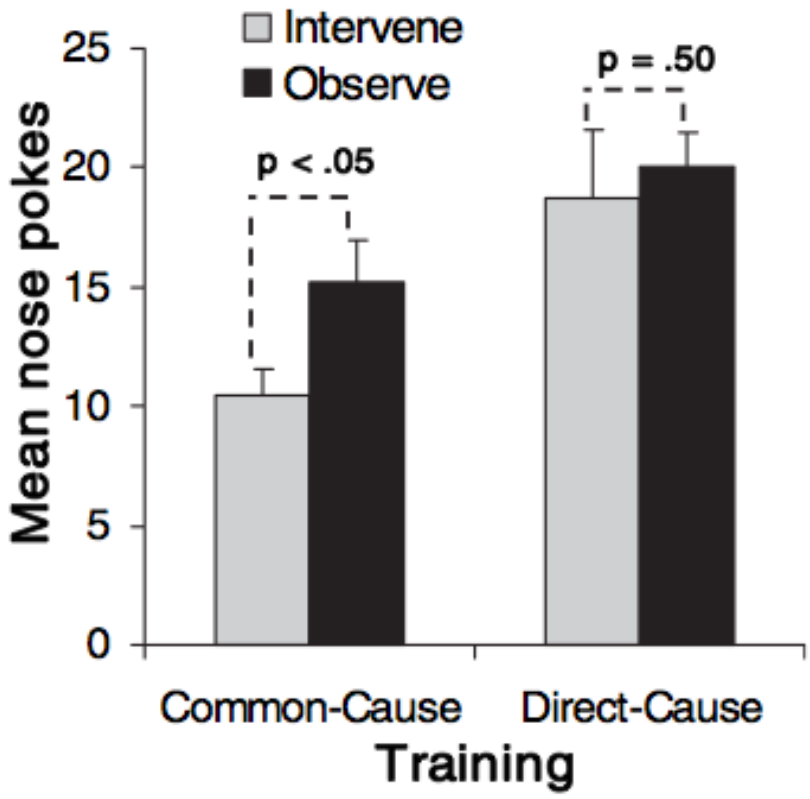

Figure 1.4. Results of Experiment 1 in Blaisdell, Sawa, Leising, & Waldmann (2006): Mean nose pokes in response to Tone in the Common Cause and Noise in the Direct Cause condition. Error bars indicate ± 1 standard error of the mean. Indicated p-values were obtained from planned comparisons from a two-way mixed ANOVA.

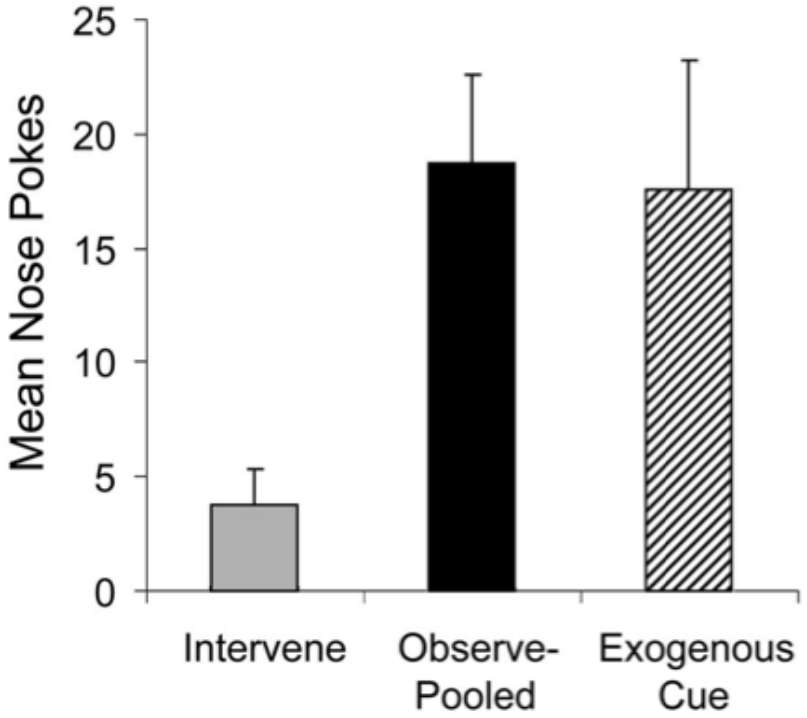

Figure 1.5. Results of Experiment 1 in Leising, Wong, Waldmann, & Blaisdell (2008): Mean nose pokes in response to Tone for the Intervene, Observe, and Exogenous cue groups. Error bars indicate ± 1 standard error of the mean.

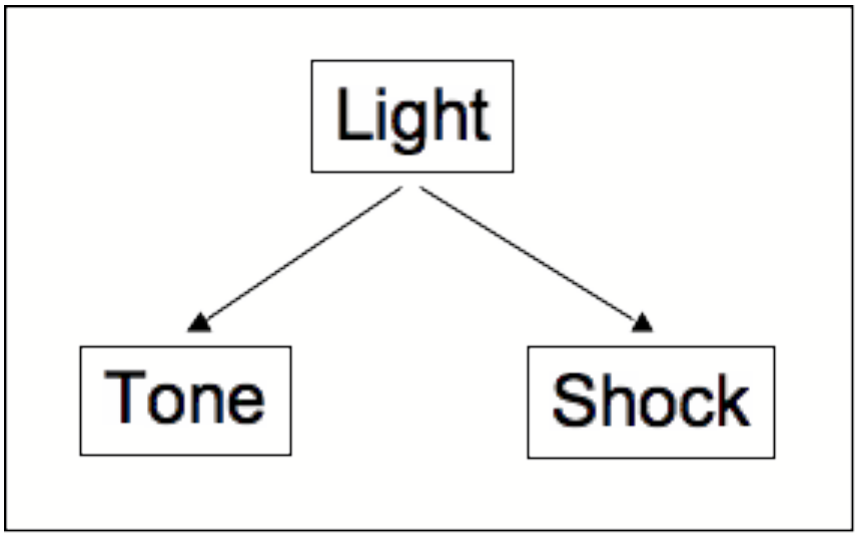

Figure 1.6. The causal model used in Wong & Blaisdell (Unpublished data), consisting of an aversive variant of the common cause model used in Blaisdell, Sawa, Leising, & Waldmann (2006). The arrows represent causal directions from cause to effect.

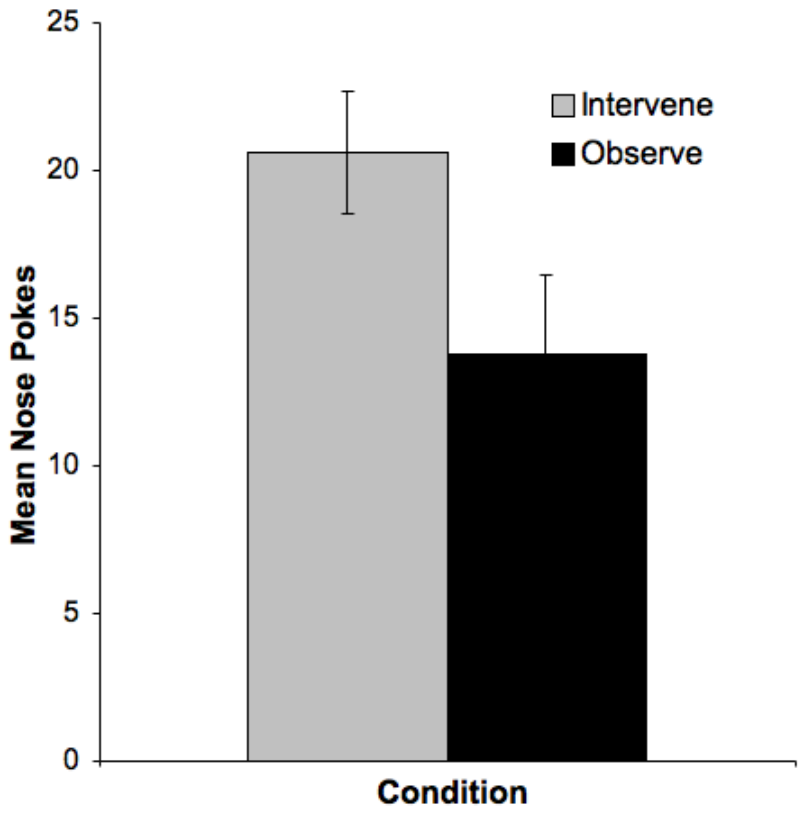

Figure 1.7. Results from Wong & Blaisdell (Unpublished): Mean nose pokes in response to the Tone for the Intervene and Observe groups. Error bars indicate ± 1 standard error of the mean.

CHAPTER 2

Establishing the Basic Effects: Goal-Directed/Habitual Instrumental Behavior and the Causal Intervention Effect

2.2: Summary

The experiments described in Aims 2 and 3 first require us to establish two sets of experimental parameters upon which these studies will be based: One to reliably produce habitual and goal-directed instrumental behavior (Aim 2), and another to reliably replicate the causal intervention effect as obtained in Blaisdell et al. (2006) and Leising et al. (2008; Aims 2 and 3). For the former, we replicated the procedures used by Yin, Knowlton, & Balleine (2004) and Yin, Ostlund, Knowlton, Balleine, & (2005) to obtain habitual (via a random interval schedule of reinforcement, seven total sessions) and goal-directed (via a random ratio schedule of reinforcement, seven total sessions) instrumental behavior, respectively. While we were able to successfully obtain goal-directed instrumental behavior, we were not met with similar success in the case of habitual instrumental behavior. Since it was previously reported that goal-directed behavior has a tendency to develop into habitual behavior with extended training of the instrumental response (e.g., Adams, 1982), we attempted an extended interval schedule with another group of rats, and were eventually met with success, but only after extending the total number of lever press shaping sessions to 25.

As for the causal intervention effect: We seem to have unfortunately lost the ability to reproduce this effect following some adjustment to the equipment that produces auditory stimuli in our laboratory. Suspecting that the higher amplitude of the auditory stimuli may have caused a startle effect which interfered with the ability by rats to effectively interpret and make correct inferences about causal interventions, we turned the amplitudes down to 15 dB(A) above background, and later 8 dB(A) above background to see if the effect could be rescued. Results suggest that we appear to have recovered the effect with the stimuli set at 8dB(A) above

background, corroborating our suspicion (and providing further evidence against the response competition account). Having established these parameters, they were used in Aims 2 and 3.

2.2: Introduction

Before the proposed experiments could be done, there was a need to ensure that we could first obtain the effects upon which our experimental paradigms are built. Goal-directed behavior is a form of instrumental behavior that underlies intentional actions. However, instrumental behavior can also result from non-goal-directed processes as well, such as habit learning. Previous studies have shown that goal-directed behavior is motivated by the knowledge of the contingency between the action and the outcome, whereas this knowledge is not a factor in habitual behavior. For example, Dickinson, Nicholas, & Adams (1983) demonstrated that lever pressing for food by rats was goal-directed if they were trained on a ratio schedule of reinforcement, whereas lever pressing was habitual if rats were trained on a interval schedule of reinforcement. In their study, two groups of rats were trained to lever-press for food either on a ratio or an interval schedule. Subsequently, they received conditioned taste aversion by receiving food pairings with lithium chloride. When tested with the levers again, rats that received the ratio schedule showed attenuated rates of lever pressing, on top of rejecting the food. However, rats that received the interval schedule did not show a similar attenuation, suggesting that these rats acted as if they were not really pressing the lever for food – a hallmark of habitual response. For Aim 2, we planned to first give rats lever press shaping sessions such that goal-directed instrumental behavior is instilled on one group of rats, while habitual instrumental behavior is instilled on the other. Since we have not done such experiments in our laboratory before, we needed to first establish a set of training parameters that would garner the desired types of instrumental behavior.

For Aims 2 and 3, we also needed to first ensure that we could still obtain the basic intervention effect obtained by Leising et al. (2008), wherein under the common-cause condition, rats that intervened on the lever to produce a cue did not expect food as much as those that merely observed it. In contrast, under a direct-cause condition, rats showed an equally high expectation of food, regardless of whether they intervened to produce or simply observed that cue. Unfortunately, we appear to have lost the ability to replicate the said effect after turning the amplitude of the auditory stimuli in our experimental chambers (which served as outcomes in the causal model that our subjects are trained on) up to 20 dB(A) above background some time after the Leising et al. (2008) experiments. Instead, our data has since resembled those as obtained in replication attempts by Dwyer et al. (2009), which were more suggestive of a response competition account rather than the notion that rats are able to reason effectively about causal interventions (i.e., the causal intervention effect). Thus, before we proceeded with Experiment 2, this causal intervention effect must be rescued in order for any resultant data to be meaningful.

2.3: Replicating the Causal Intervention Effect and Goal-Directed/Habitual Instrumental Behavior Effects

The first goal of this series of experiments was to rescue the basic causal intervention effect that was obtained in this laboratory (e.g., Blaisdell et al., 2006; Leising et al., 2008). As mentioned previously, in the time after the experiments for Leising et al. (2008) was conducted, we recalibrated the amplitudes of all auditory stimuli issued in our experimental chambers. The aim was to standardize all auditory stimuli to 20 dB(A) above background for all future experiments conducted in these chambers. Since that adjustment, we conducted several experiments that were based off of the basic causal intervention effect, but that effect has since appeared to elude us. Thus, before performing the experiments proposed in this dissertation, it

made sense to first replicate the Leising et al. (2008) experiment to ascertain if we could still obtain that basic effect.

The second goal was to make sure that we could establish a set of instrumental training parameters that could reliably produce goal-directed and habitual lever pressing behavior. Since we have not done experiments of this nature in our laboratory, a good place to begin would be to see if we could replicate the interval (from Yin et al., 2004) or ratio schedules (from Yin et al., 2005b) used to obtain habitual and goal-directed instrumental behavior, respectively.

To minimize the number of animals used, we decided to run these pre-experiments sequentially with the same subjects: We would first attempt to replicate the basic causal intervention effect before giving them the instrumental training with the levers to see if we could obtain habitual or goal-directed lever pressing.

2.3.1: Method

Subjects. Thirty-two experimentally-naïve female Long-Evans rats (*Rattus norvegicus*) acquired from a commercial breeder (Harlan, Indianapolis, IN) served as subjects. Subjects were pair-housed in transparent plastic tubs with a wood shaving substrate in a vivarium maintained on a 14/10 hr dark/light cycle where they had unlimited access to water and unlimited access to food during the food restriction and water restriction portions of the study, respectively (see below). Experiments were conducted during the dark portion of the cycle. For the causal-model training portion of the experiment, a food restriction schedule was imposed starting from five days before the first session to maintain rats at 85% of their initial free-feeding weights. During this time, rats were given free access to fresh water. Thereafter, starting from one day prior to the lever-press shaping portion of the experiment, rats were given ad libitum access to food, and a water restriction schedule was imposed until the end of the experiment. On the first day of water

restriction (approximately 24 hours before the first experimental session), rats were given access to water for 30 min. During the rest of the lever-press shaping portion of the experiment, rats were given 30-minute access to water daily after each session. All animals were handled for 45 s during the three days prior to the initiation of the study.

For the causal-model training portion of the experiment, all subjects received identical training, but during testing, half of the subjects were randomly assigned to the Common-Cause condition, while the remaining half were assigned to the Direct-Cause condition. Within each Causal Model condition, half the subjects were assigned to Intervene for their respective Common-Cause or Direct-Cause cues, while the remaining half merely observed the cues (i.e., Observe; see Table 2.1). For the instrumental portion of the experiment, half the subjects were randomly assigned to receive lever-pressing training under a random ratio schedule, while the other half received training under a random interval schedule. During testing, half of the subjects within each Training condition were assigned to receive prefeeding (i.e., Devalued), while the remaining received no prefeeding (i.e., Non-devalued, see Table 2).

Apparatus. The experiment was run with eight experimental chambers, each measuring 30 x 25 x 20 cm (L x W x H), which were housed in separate sound- and light- attenuating environmental isolation chests (ENV-008, Med Associates, Georgia, VT). The front and back walls and ceiling of the chamber were constructed of clear Plexiglas, the side walls were made of aluminum, and the floors were constructed of stainless steel rods measuring 0.5 cm in diameter, spaced 1.5 cm center-to-center. The enclosure was dimly illuminated by a 28-V bulb (ENV-215M, Med Associates) house light located 2 cm from the top and 4 cm from the right of the right-side chamber wall. On that same wall, a diffuse light (ENV-227M, Med Associates) was also located 2 cm from the top and 3.5 cm from the left.

Each chamber was equipped with a liquid-dipper (ENV-202M, Med Associates) that could be lowered into a trough of water or sucrose solution (20%) and raised, located at the bottom middle of the right-side chamber wall. When in the raised position, a small well (0.05 cc) at the end of the dipper arm that contained water or sucrose solution protruded up into the drinking niche. On the metal wall of the chamber directly left of the food niche, there was a 3.5-cm wide operant lever (ENV-112CM, Med Associates) that could be retracted from the chamber when not in use, located 8 cm from the food niche center-to-center, and 2 cm above the floor grid. A speaker (ENV-223AM, Med Associates) on the upper left corner of the left-side chamber wall (flush with the upper left corner of the wall) could deliver a tone (3 kHz) and a white noise stimulus, 20 dB(A) above background. CS L was a 10-s flashing light produced by flashing the diffuse light at a rate of 2 cycles (on-off) per second. CS X and Y were a 10-s presentation of a tone and noise, counterbalanced (See Table 2.1). Ventilation fans in each enclosure and a white-noise generator on a shelf outside of the enclosures provided a constant 62-dB(A) background noise.

Procedure.

I. Replication of Leising et al. (2008). The experimental design for this portion of the study is summarized in Table 2.1.

Magazine training. The levers were retracted during all phases except at test. On Day 1, sucrose was delivered every 20 ± 15 s (actual ITI values = 5, 10, 15, 20, 25, 30, and 35 s) in a 60-min session to train rats to approach and drink from the dipper (magazine training). Reinforcement consisted of 10-s access to the raised dipper cup. The house light was on during this session.

Phase 1 (sensory preconditioning). On Days 2-5, subjects in all groups received six daily trials on which Stimulus L was presented for 10 s followed by Stimulus X for 10 s. The onset of X coincided with the termination of L. Trials occurred with a mean interval of 5 ± 2 min (actual ITI values = 3, 4, 5, 6, and 7 min) during each 30-min session. During this phase, the food niche was covered by a metal plate that blocked access to into the niche.

Phase 2 (first-order conditioning). On Days 6-7, rats received 12 trials of L-Food pairings in each daily 60-min session. Food was delivered at the termination of Stimulus L. Interspersed within each session, rats also received 12 simultaneous pairing of Stimulus Y and Food. Trials occurred with a mean intertrial interval of 5 ± 2 min (actual ITI values = 3, 4, 5, 6, and 7 min).

Testing. Day 8. For this phase, the light that delivered Stimulus L was covered with a 2"(w) x 2"(l) x ¾" (d) stainless steel cover. The levers were extended into the chambers only for rats in the intervention test condition and X and Y were presented in the following manner: Half the rats in Group Common-Cause were assigned to the intervention test condition, in which X was presented each time the rat pressed the lever. The remaining rats in Group Common-Cause were allocated to the observation test condition, in which presentations of X were yoked to a Master rat in the intervention condition. Likewise, half the rats in Group Direct-Cause were allocated to the intervention condition and the remainder to the observation condition, and Y was presented in an analogous manner. Each session lasted 30 min. Expectation of food, operationalized as nose pokes into the food niche, was recorded during presentations of X and Y.

II. Replication of Yin et al. (2004) and Yin, et al. (2005). The experimental design for this portion of the study is summarized in Table 2.2.

Lever-Press Shaping. Days 9-16. Rats were trained to approach and press the operant lever to gain access to water. The first two sessions of lever press shaping were conducted on a continuous reinforcement schedule in which each lever press was followed by access to water for 10 s. Each session terminated after 30 min had elapsed.

Six additional daily 30-min training sessions were then conducted, with the duration of each water presentation reduced to 5 s. Rats in the Ratio condition were trained under a random ratio (RR) schedule of reinforcement with an average ratio of 5 (RR-5, two sessions), and progressed to RR-10 (two sessions), and finally RR-20 (two sessions). Rats in the Interval condition were trained under a random interval (RI) schedule of reinforcement with an average ratio of 15 s (RI-15s, two sessions), which progressed to RI-30 s (two sessions), and finally RI-60 s (two sessions).

Testing. Days 17. Rats in Groups Ratio-Prefeed and Interval-Prefeed (i.e., half of subjects in each of the Ratio and Interval conditions) were given free access to water in their home cages for 30 min prior to the test session. Thereafter, all rats were placed in the chambers with the levers extended, and given 5-minute test sessions in extinction. We measured the total number of lever presses made by all rats during the session, which we used to calculate an average rate of response (lever presses per minute).

2.3.2: Results and Discussion

I. Replication of Leising et al. (2008). Figure 2.1 displays the mean nose pokes made in the food niche by rats presented as a function of Condition (Intervene vs. Observe), and of Causal Model (Common-Cause vs. Direct-Cause). A 2x2 ANOVA with Condition and Causal Model as between-subjects factors revealed a main effect of Condition, $F(1, 28)= 5.831$, $p = .023$, such that generally, rats that Intervened to produce either the tone or noise made

significantly less nose pokes (M = 10.795, SEM = 2.648) than rats that merely Observed these cues (M = 19.838, SEM = 2.648). No main effect of Causal Model was found, $F(1, 28) = .631$, $p = .434$, as was the case for an interaction, $F(1, 28) = .146$, $p = .706$. This suggests that we unfortunately did not replicate the intervention effect as obtained in Leising et al. (2008). In general, rats that intervened on the lever, regardless of whether or not they were in the Common-Cause or the Direct-Cause Condition, made fewer nose pokes than when the respective cues were merely observed, corroborating the response competition argument put forth by Dwyer et al. (2009). We suspected that the adjustment of auditory stimuli amplitude was probably the culprit behind the loss of our desired effect. As such, our next step is to lower the amplitude of the auditory stimuli. Hopefully, this manipulation would be sufficient to rescue the basic causal intervention effect.

II. Replication of Yin et al. (2004) and Yin, et al. (2005). Figure 2.2 shows the rate of lever-pressing per minute (in extinction) made by rats that were either Ratio or Interval-trained, and as a function of devaluation. A 2x2 ANOVA with Training (Ratio vs. Interval) and Devaluation (Devalued vs. Non-devalued) as between-subjects factors revealed a main effect of Devaluation, $F(1, 28) = 20.047$, $p < .001$, with Non-devalued rats making significantly more lever mean presses per minute (M = 22.963, SEM = 2.880) than Devalued rats (M = 4.725, SEM = 2.880), regardless of Training. Otherwise, no main effect was found for Training, $F(1, 28) = 2.922$, $p = .098$, nor was a significant interaction revealed, $F(1, 28) = 3.223$, $p = .083$. We were thus unable to attain rats that lever pressed in a manner that was outcome-insensitive (i.e., habitually). That is, outcome devaluation (via prefeeding before testing) seemed to attenuate lever-pressing, whether the rats were ratio-trained or interval-trained. This suggests that lever-pressing produced goal-directed behavior in both cases. As such, our next step would be to adopt

an alternate procedure to produce habitual lever-pressing: An extended random ratio schedule of reinforcement.

2.4: Obtaining Habitual Instrumental Behavior

2.4.1: Attempting an Extended-Interval Schedule

As described above, our first attempt at obtaining habitual instrumental behavior by replicating the training procedures used by Yin et al. (2004) was not successful. Previous research has shown that with extended training of the instrumental response, goal-directed, outcome-guided behavior tends to develop into habitual, stimulus-response behavior (e.g., Adams, 1982). Thus, we decided to implement an extended random ratio schedule of reinforcement (with sucrose solution as the reinforcement instead of water) to see if the desired effect would be obtained. Since we were able to obtain rats that lever-pressed in a goal-directed fashion with the parameters adapted from Yin et al. (2005), no modifications to the procedures were necessary in that aspect.

2.4.1.1: Method

Subjects. Thirty-two subjects were used, as in 2.3.1: Half of them were randomly assigned to the Extended-Interval condition, while the remaining half were assigned to the Ratio condition. Starting from five days before the experiment, a food restriction schedule was imposed to maintain rats at 85% of their initial free-feeding weights. This schedule was maintained for the duration of the experiment.

Apparatus. As in 2.3.1, except that the diffuse light was not used, nor were any auditory stimuli.

Procedure. The procedure described here applies for rats in the Extended-Interval condition only. The procedure for rats in the Ratio condition were as described in 2.3.1, and were

run beginning on Day 8; this was done so that all subjects would complete their respective lever-press training phases at the same time, to be tested together on the same day. Regardless of condition, rats were pre-exposed to drinking sucrose from a bottle in their home cages two days prior to the commencement of their respective lever-press training phases. This was done so that the sucrose bottles would not be novel during the devaluation session prior to testing for all subjects involved. On both days, the sucrose bottles were inserted into the cages during feeding time and removed 30 min later.

Magazine training. Day 1. The levers were retracted during this phase only. On Day 1, sucrose was delivered every 20 ± 15 s (actual ITI values = 5, 10, 15, 20, 25, 30, and 35 s) in a 60-min session to train rats to approach and drink from the dipper. Reinforcement consisted of 10-s access to the raised dipper cup. The house light was on during every session.

Lever-Press Shaping. Days 2-16. Rats were trained to approach and press the operant lever to gain access to sucrose. The first session of lever press shaping was conducted on a continuous reinforcement schedule in which each lever press was followed by access to sucrose for 10 s. This session terminated after 30 min had elapsed.

A total of 14 additional daily 30-min training sessions were then conducted, with the duration of each sucrose presentation reduced to 5 s. Rats were trained under a random interval (RI) schedule of reinforcement with an average ratio of 15 s (RI-15s, 1 session), which progressed to RI-30s (1 session), and finally RI-60s (12 sessions). Throughout the Lever-Press Shaping phase, the subjects were run daily at the same time of the day, as well as by the same experimenter (this was true for rats in the Ratio condition as well).

Testing. Day 17. The test session was conducted on the same day for all subjects. Rats in Groups Ratio/Devalued and Extended-Interval/Devalued (i.e., half the subjects from both the

Extended-Interval and Ratio conditions) were given free access to rat chow and sucrose solution for 30 min prior to the test sessions. The rest of the subjects (all in the Non-devalued condition) remained in their home cages without food pellets or sucrose solution during this time. Thereafter, all rats were placed in the chambers with the levers extended, and given a 5-min extinction test on the levers. We measured the total number of lever presses made by all rats, with which we used to calculate a mean rate (lever presses per minute).

2.4.1.2: Results and Discussion. Figure 2.3 shows the mean lever presses per minute as a function of Training (Ratio vs. Extended-Interval) and Devaluation (Devalued vs. Non-devalued). A 2x2 ANOVA with Training (Ratio vs. Ext-Interval) and Devaluation (Devalued vs. Non-devalued) as between-subjects factors revealed main effect of Devaluation, $F(1, 27) = 36.416$, $p < .001$, a main effect of Training, $F(1, 27) = 7.274$, $p = .012$, as well as a significant interaction $F(1, 27) = 8.652$, $p = .007$. A planned comparison revealed that in the Devalued condition, mean lever press rates made by Ratio-trained rats ($M = 4.075$, $SEM = .785$) did not differ to that by Extended Interval-trained rats ($M = 5.000$, $SEM = .826$), $t(14) = .812$, $p = .431$, suggesting that we did not obtain habitual instrumental behavior with extended interval training. In fact, the data appear remarkably similar to that obtained in Figure 2.2. In the Non-devalued condition, Ratio-trained rats produced a higher rate of lever pressing ($M = 38.029$, $SEM = 7.286$) than Extended Interval-trained rats ($M = 16.700$, $SEM = 3.489$), $t(13) = -2.754$. $p = .016$, most likely because reinforcements are directly dependent on the number of lever presses made in ratio schedules, which is not the case for interval schedules. For rats that were Ratio-trained, rats that did not receive the devaluation treatment produced a significantly higher rate of lever pressing than those that did, $t(13) = -4.971$, $p < .001$. This was also the case for the Extended

Interval-trained rats, $t(14) = -3.264$, $p = .006$. Together, the data indicates that our extended interval training was still unsuccessful in producing habitual instrumental behavior.

2.4.2: Further Extending the Extended-Interval Schedule

Because we were still unable to obtain habitual instrumental behavior, we decided to give ten further RI-60 sessions to all subjects in the Extended-Interval condition to see if this extra-extended interval training would make a difference. For this portion of the study, as an alternate method of outcome devaluation, we used conditioned taste aversion instead of sensory specific satiety (prefeeding). Additionally, since we again had no problems obtaining goal-directed instrumental behavior for rats in the Ratio condition, we did not give those rats any further training or test sessions.

2.4.2.1: Method

Subjects. All 16 rats in the Extended-Interval condition from 2.4.1 were used.

Apparatus. As in 2.4.1.

Procedure.

Further Lever-Press Shaping. Days 18 – 27. We gave all rats 10 additional 30-min RI-60 s sessions. As before, all rats were run at the same time by the same experimenter every day.

Conditioned Taste Aversion. Days 28 – 30. Beginning the day after the further lever-press shaping sessions, we devalued the sucrose reward by way of conditioned taste aversion for half the subjects (i.e., the Devalued condition), following the procedures used in Yin et al. (2004): For each of three daily sessions, all rats were given the opportunity to drink sucrose from a bottle attached to their home cages for 30 min. Immediately thereafter, half of these rats received an intraperitoneal injection of lithium chloride (0.15 M LiCl, 20 mL/kg), while the remaining half received saline injections with the same concentration and dosage.

Testing. Day 31. We first gave all subjects the same 5-min extinction test on the levers, as in 2.3.1. To ascertain if a lack of difference in lever pressing rates between rats that received outcome devaluation treatment and those that did not (if obtained) was due to habitual lever pressing behavior as opposed to failure to obtain conditioned taste aversion, we followed this test up with a 5-minute post-test consumption check: Immediately after this lever press test session, we gave all rats access to a bottle of sucrose in their home cages for five minutes and measured the amount of time each rat spent during these five minutes drinking from it.

2.4.2.2: Results and Discussion. Figure 2.4 displays the mean lever presses per minute made by rats in the Devalued and Non-devalued conditions. An independent samples *t*-test revealed that the mean lever pressing rate of rats that received the devaluation treatment (M = 14.500, SEM = 1.125) did not differ to that by rats that did not (M = 15.850, SEM = 3.978), $t(14) = -.327$, $p = .749$. The results from the post-test consumption check is displayed in Figure 2.5, which indicate subjects that received the devaluation treatment spent less time drinking from the sucrose bottle than those that did not. Indeed, an independent samples *t*-test revealed that rats that received the devaluation treatment spent significantly less time drinking from the sucrose bottle (M = 38.750 s, SEM = 11.196 s) than rats that did not (M = 138.750 s, SEM = 11.599 s), $t(14) = -6.203$, $p < .001$.

Together, the data indicates that we successfully devalued the sucrose with taste aversion conditioning for the Devalued rats, but this conditioning did not affect their rates of lever pressing during the lever press test relative to the Non-devalued controls. This suggests that these Devalued rats were not pressing the levers for the sucrose reward; rather, they were pressing the levers in a habitual manner. Thus, it appears that we were thus finally able to obtain this elusive effect.

2.5: Rescuing the Causal Intervention Effect

2.5.1: Lowering the Amplitude of Auditory Stimuli to 15 dB(A) Above Background

As described in 2.2, since the amplitude of the auditory stimuli issued by our experimental chambers were increased to 20 dB(A) above background from an uncertain point, we have been unable to replicate the causal intervention effect that we obtained in Blaisdell et al. (2006) and Leising et al. (2008). We thus attempted to replicate the procedures used in Leising et al. (2008), but with the amplitudes lowered down to 15 dB(A) above background to see if we the effect can be rescued.

2.5.1.1: Method

Subjects. Sixty-four experimentally-naïve female Long-Evans rats (*Rattus norvegicus*) acquired from a commercial breeder (Harlan, Indianapolis, IN) served as subjects. As in the previous experiments, subjects were pair-housed in transparent plastic tubs with a wood shaving substrate in a vivarium maintained on a 14/10 hr dark/light cycle where they had unlimited access to fresh water. Experiments were conducted during the dark portion of the cycle. A food restriction schedule was imposed starting from five days before the first session to maintain rats at 85% of their initial free-feeding weights. All animals were handled for 45 s during the three days prior to the initiation of the study.

Apparatus. As in 2.3.1, except that all auditory stimuli (i.e., the tone and white noise) were decreased to 15 dB(A) above background, down from 20 dB(A).

Procedure.

Magazine training. The levers were retracted during all phases except at test. On Day 1, sucrose was delivered every 20 ± 15 s (actual ITI values = 5, 10, 15, 20, 25, 30, and 35 s) in a 60-min session to train rats to approach and drink from the dipper (magazine training).

Reinforcement consisted of 10-s access to the raised dipper cup. The house light was on during all sessions of the study.

Phase 1 (Sensory Preconditioning). On Days 2-5, subjects in all groups received six daily trials on which Stimulus L was presented for 10 s followed by Stimulus X for 10 s. The onset of X coincided with the termination of L. Trials occurred with a mean interval of 5 ± 2 min (actual ITI values = 3, 4, 5, 6, and 7 min) during each 30-min session.

Phase 2 (First-order Conditioning). On Days 6-7, rats received 12 trials of L-Food pairings in each daily 60-min session. Food was delivered at the termination of Stimulus L. Interspersed within each session, rats also received 12 simultaneous pairing of Stimulus Y and Food. Trials occurred with a mean intertrial interval of 2.5 ± 1 min (actual ITI values = 1.5, 2.5, & 3.5 min, randomly picked).

Testing. Day 8. During testing, the light that delivered Stimulus L was covered with a 2"(w) x 2"(l) x ¾" (d) stainless steel cover. The levers were extended into the chambers only for rats in the intervention test condition and X and Y were presented in the following manner: Half the rats in Group Common-Cause were assigned to the Intervene condition, in which X was presented each time the rat pressed the lever. The remaining rats in Group Common-Cause were allocated to the Observe condition, in which presentations of X were yoked to a master rat in the intervention condition. Likewise, half the rats in Group Direct-Cause were allocated to the intervention condition and the remainder to the observation condition, and Y was presented in an analogous manner. Each session lasted 30 min. Expectation of food, operationalized as nose pokes into the food niche, was recorded during presentations of X and Y.

2.5.1.2: Results and Discussion. During testing, one of the rats assigned to the Intervene/Common-Cause condition failed to make any lever presses, which meant that neither it

nor its yoked Observe/Common-Cause counterpart received any test trials. As a result, we were left with data from 30 subjects in the Common-Cause condition (15 Intervene, 15 Observe), with 32 subjects in the Direct-Cause condition (16 Intervene, 16 Observe).

The mean number of nose pokes in the food niche made by subjects presented as function of Condition (Intervene vs. Observe) and of Causal Model (Common-Cause vs. Direct-Cause) is displayed in Figure 2.6. A 2x2 ANOVA with both Condition and Causal Model as between-subjects factors revealed a main effect of Condition, $F(1, 58) = 15.154$, $p < .001$, such that generally, rats that Intervened to produce either the tone or noise made significantly less nose pokes ($M = 20.339$, $SEM = 2.204$) than rats that merely Observed these cues ($M = 32.471$, $SEM = 2.204$). A main effect of Causal Model was also found, $F(1, 58) = 6.302$, $p = .015$, such that generally, rats that were tested on Y (i.e., the Direct-Cause cue) made significantly more nose pokes ($M = 30.317$, $SEM = 2.168$) than those tested on X (i.e., the Common-Cause cue; $M = 22.493$, $SEM = 2.239$). No significant interaction was revealed, $F(1, 58) = .174$, $p = .678$.

Unfortunately, this attempt to rescue the causal intervention effect was not successful, and the main effect of Condition (with Observe rats producing higher responses than Intervene rats regardless of Causal Model) again supported the response competition account posited by Dwyer et al. (2009). However, the gap in responding between Intervene and Observe rats in the Direct-Cause condition appears to have narrowed to a degree (compared to the results in 2.2), which could suggest that the 5 dB(A) reduction in stimuli amplitude might have rescued the desired effect to a degree. Additionally, a qualitative assessment of this data vis-à-vis Figure 2.1 indicates that there was a general elevation of nose poke responses in all four experimental conditions (i.e., Common-Cause/Intervene, Common-Cause/Observe, Direct-Cause/Intervene, and Direct-Cause/Observe). Taken together, this led us to consider the role that the amplitude of

the auditory stimuli could have on our paradigm: We postulated that perhaps the decreased responding at the food niche in the Intervene conditions (for both Common- and Direct-Cause conditions) was not a result of response competition, as much as it might have been a result of a startle response. Specifically, during testing, Intervene rats have never experienced pressing a lever to produce an auditory tone before. Thus, if the stimuli were too intense, the subjects may have at first been startled by the onset of the stimuli, which would have resulted in a degree of freezing. The rats may then have "gotten over" the startle, and subsequently made the respective responses at the food niche, only there would not have been enough time left (post-startle) within the 10-s CS duration to be a reliable measure of food expectancy based on the conditions.

To test this hunch, we decided to replicate this procedure again, but turn the amplitude of the auditory stimuli further down to 8dB(A) above background.

2.5.2: Further Lowering the Amplitude of Auditory Stimuli to 8 dB(A) Above Background

We hypothesized in 2.5.1.2 that the loss of the causal intervention effect as obtained in Blaisdell et al. (2006) and Leising et al. (2008) could have been due to the high amplitude of the auditory cues involved. Namely, if these cues were too loud, rats that intervened to produce these cues (which they have never done before until testing) might have been startled, which in turn might have interfered with how they would have otherwise made their respective causal diagnoses (i.e., Common-Cause vs. Direct-Cause). Specifically, subjects would have required some time to get over this startle, and since the stimulus periods were only 10 s long, they would not have had enough time for the desired behavior to be expressed, post-startle. Lowering the amplitude from 20 dB(A) down to 15 dB(A) seems to have rescued the effect to a small degree.

Thus, we decided to further lower the amplitude down to 8dB(A) to see if we could finally rescue the desired effect.

2.5.2.1: Method

Subjects. Thirty-two experimentally-naïve female Long-Evans rats (*Rattus norvegicus*) acquired from a commercial breeder (Harlan, Indianapolis, IN) served as subjects. Subjects were housed and treated in the same manner as in 2.5.1.

Apparatus. As in 2.5.1, except that all auditory stimuli (i.e., the tone and white noise) were further decreased to 8 dB(A) above background, down from 15 dB(A).

Procedure. As in 2.5.1.

2.5.2.2: Results and Discussion. During testing, two of the rats assigned to the Intervene/Direct-Cause condition failed to make any lever presses, which meant that neither them nor their yoked Observe/Direct-Cause counterparts received any test trials. As a result, we were left with data from only 12 subjects in the Direct-Cause condition (6 Intervene, 6 Observe), with 16 subjects in the Common-Cause condition (8 Intervene, 8 Observe).

Figure 2.7 shows the mean number of nose pokes made in the food niche by subjects presented as function of Condition (Intervene vs. Observe) and of Causal Model (Common-Cause vs. Direct-Cause). Looking at the pattern of results, it appears that we might have successfully rescued the causal intervention effect. However, the loss of four subjects in the Direct-Cause condition appears to have taken its toll on statistical power. A 2x2 ANOVA with both Condition and Causal Model as between-subjects factors revealed a main effect of Causal Model, $F(1, 24) = 4.711$, $p = .040$, such that generally, rats that were tested on Y (i.e., the Direct-Cause cue) made significantly more nose pokes ($M = 28.010$, $SEM = 4.262$) than those tested on X (i.e., the Common-Cause cue; $M = 15.771$, $SEM = 3.691$). No main effect of Condition was

found, $F(1, 24) = 2.856$, $p = .104$, nor was there a significant interaction, $F(1, 24) = .504$, $p = .485$.

The experiment was underpowered, but the data do appear optimistic. Indeed, while the means were in the right direction (i.e., in favor of the causal intervention effect), the loss of a quarter of our subjects in the Direct-Cause condition appears to have prevented us from obtaining the desired interaction. Ordinarily, such results would warrant a replication to increase statistical power, thus unveiling the hypothesized effect. However, aiming to minimize the use of additional animals, the current results were encouraging enough for us to move on to the next aim of this dissertation.

2.6: General Discussion

In this chapter, we aimed to establish the appropriate parameters necessary to train rats to lever press habitually and in a goal-directed manner. Starting with the training procedures used by Yin et al. (2004) and Yin et al. (2005b), we obtained goal-directed, but not habitual instrumental behavior with the brief random ratio and random interval schedules of reinforcement (respectively) as they were described. We decided to give extended random interval training to see if habitual lever pressing could be obtained, and were met with success only after extending the lever press training sessions to a total of 25 sessions (1 continuous reinforcement session, 1 RI-15 s session, 1 RI-30 s session, and finally 22 RI-60 s sessions). It remains unclear how Yin and colleagues (2004) were able to obtain rats that lever-pressed in a habitual manner with only six training sessions with the random interval schedule (1 continuous reinforcement session, 2 RI-15 s sessions, 2 RI-30 s sessions, and 2 RI-60 s sessions).

We also described that we had lost the basic causal intervention effect (as obtained in Blaisdell et al. 2006 and Leising et al., 2008) after making amplitude increases to the auditory

stimuli issued by our operant equipment, and we aimed to recover the effect. Thus, we replicated the procedures used in Leising et al. (2008) after turning the amplitude down, first from 20 dB(A) to 15 dB(A), and subsequently to 8 dB(A) above background. We appeared to have rescued the effect to an extent with 8 dB(A), but due to low statistical power from losing several subjects, the effect did not reach statistical significance. Thus, it appears that there may be a basis to our hunch: With Stimuli X and Y too loud at 20 dB(A), subjects in the Intervene condition were likely startled. Even though they had been exposed to X and Y before at that same amplitude, it had never before been triggered by their own actions; it was an entirely new experience for these subjects. This startle probably caused a degree of freezing, and by the time the subjects had gotten over the startle, there would not have been enough time left within the stimulus period (i,e.,10 s) for them to fully express their behavior, compared to a case were startle did not happen. When the amplitudes were turned down to 15 dB(A) above background, the causal intervention effect appeared to have been rescued to a degree, as reflected by the higher levels nose poking exhibited by subjects in the Intervene/Direct-Cause condition (See Figure 2.6) compared to the first attempt at this effect (i.e., Figure 2.1). The pattern of results obtained when the stimuli were turned down to 8dB(A) above background were in line with the causal intervention effect as obtained in Blaisdell et al. (2006) and Leising et al., (2008), low statistical power notwithstanding.

The startle account makes sense especially when the logarithmic nature of the decibel scale is considered: When the amplitude was turned down from 20 dB(A) to 15 dB(A) above background, the decrease of 5 dB(A) made the stimuli 3.16 times lower in intensity; and when the amplitude was further turned down to 8 dB(A) above background, the stimuli were lowered in intensity by 15.85 times relative to the original 20 dB(A). The frequency dependence of

apparent loudness should also be addressed: A white noise stimulus would be perceived to be louder than a pure 3 kHz tone, even if both stimuli measure the same on the decibel scale. Thus, if the Tone was loud enough to cause startling, then the case for Noise would have been perceptively louder. Dwyer and colleagues (2009) did not indicate the amplitude of their auditory stimuli above background, only stating that they were 80 dB. As a comparison, the absolute amplitude of the auditory stimuli that we used when the stimuli was 8 dB(A) above background was about 74 dB(A). The difference in 6 dB(A) meant that the stimuli used by Dwyer and colleagues were 3.98 times more intense than those that we used, and it is possible that this difference was enough for the stimuli to evoke the startle response. Thus, it is possible that the failure to replicate the causal intervention effect may not be due to response competition after all (as was suggested by Dwyer and colleagues), but instead due to this aforementioned startle effect. The best way to put this hypothesis to the test would be to again replicate the experiment, with stimuli amplitude (high and low) added as an additional factor.

Having determined parameters to confidently obtain the relevant instrumental effects, as well as the causal intervention effect, they were applied to the remaining experiments in Chapters 3 and 4.

Figures and Tables

Table 2.1

Experimental design for the replication of Leising et al. (2008).

Group	Causal training		Test	Predicted magazine behavior (nose pokes)
Common-Cause/Intervene	L→X	L→Food/Y:Food	LP/T (Yoke)	Low
Common-Cause/Observe	L→X	L→Food/Y:Food	LP→T	High
Direct-Cause/Intervene	L→X	L→Food/Y:Food	LP/T (Yoke)	High
Direct-Cause/Observe	L→X	L→Food/Y:Food	LP→T	High

Note: LP = lever press, L = Flashing light, X & Y = Tone & Noise (counterbalanced), Food = sucrose presentation. Arrows indicate order of events during a trial, and colons indicate concurrent presentations of events. Events on either side of a slash were interspersed within the same session. Presentations of L, X, Y, and Food all lasted 10-s in duration.

Table 2.2

Experimental design for the replication of Yin et al. (2004) and Yin, et al. (2005b).

Group	Pre-training	Test	Predicted Lever-pressing behavior
Ratio	LP→Food	LP	High
Ratio-Prefeed	LP→Food	LP (Prefed)	Low
Interval	LP→Food	LP	High
Interval-Prefeed	LP→Food	LP (Prefed)	High

Note: LP = lever press, Food = sucrose presentation. Arrows indicate order of events during a trial.

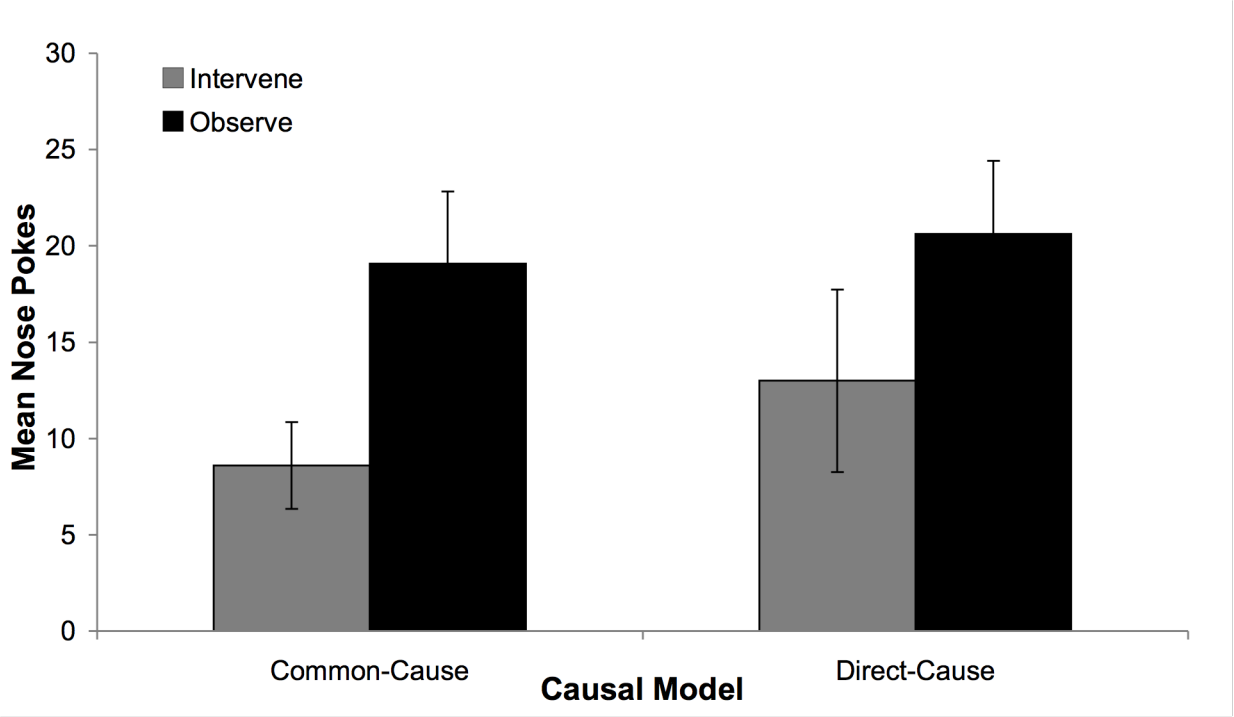

Figure 2.1. Mean nose pokes made in the sucrose niche during presentations of Stimulus X (Common-Cause) or Y (Direct-Cause) for the replication of Leising et al. (2008), presented as a function of Condition (Intervene vs. Observe) and Causal Model (Common-Cause vs. Direct-Cause). There was a main effect of Condition, where Intervene rats made fewer nose pokes than Observe rats in both Causal Models, reflecting in a failure to replicate the causal intervention effect. Error bars represent ± 1 standard error of the mean.

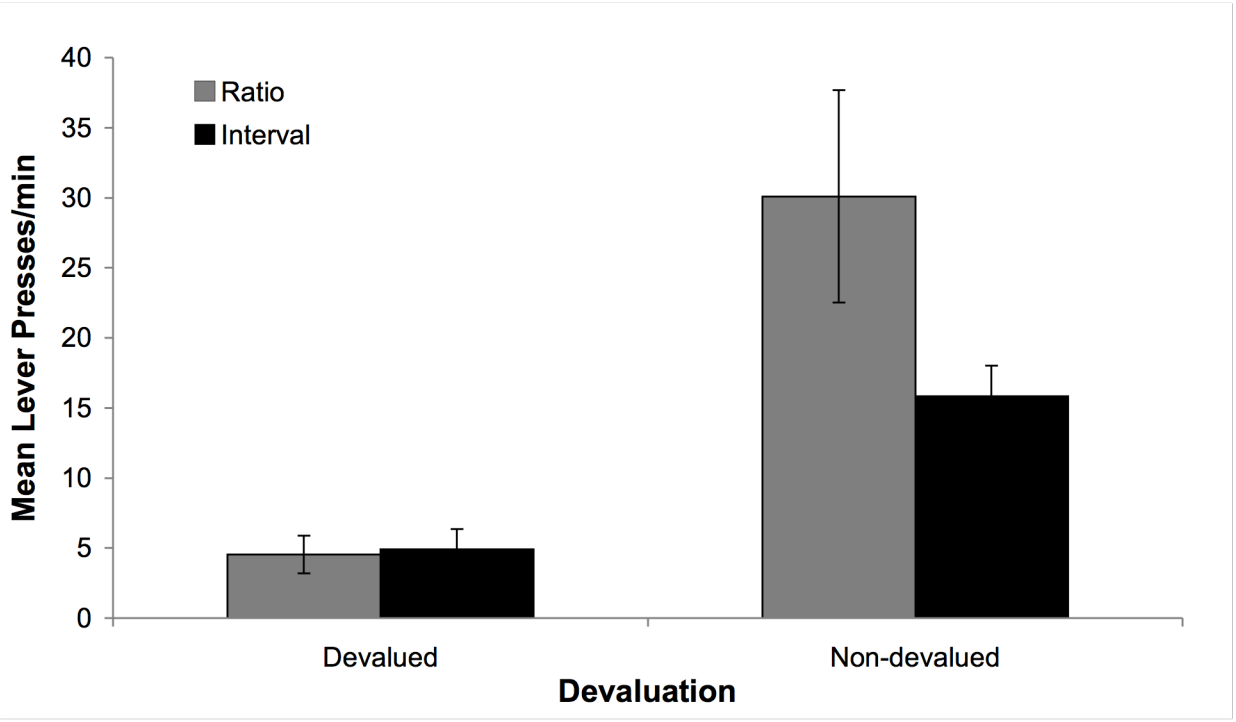

Figure 2.2. Results of the lever-press test between prefed (Devalued) and non-prefed (Non-devalued rats. A devaluation effect was observed for both Ratio- and Interval-trained subjects, suggesting that the Interval-trained rats lever pressed in a goal-directed manner, as per the subjects in the Ratio-trained condition. Error bars represent ± 1 standard error of the mean.

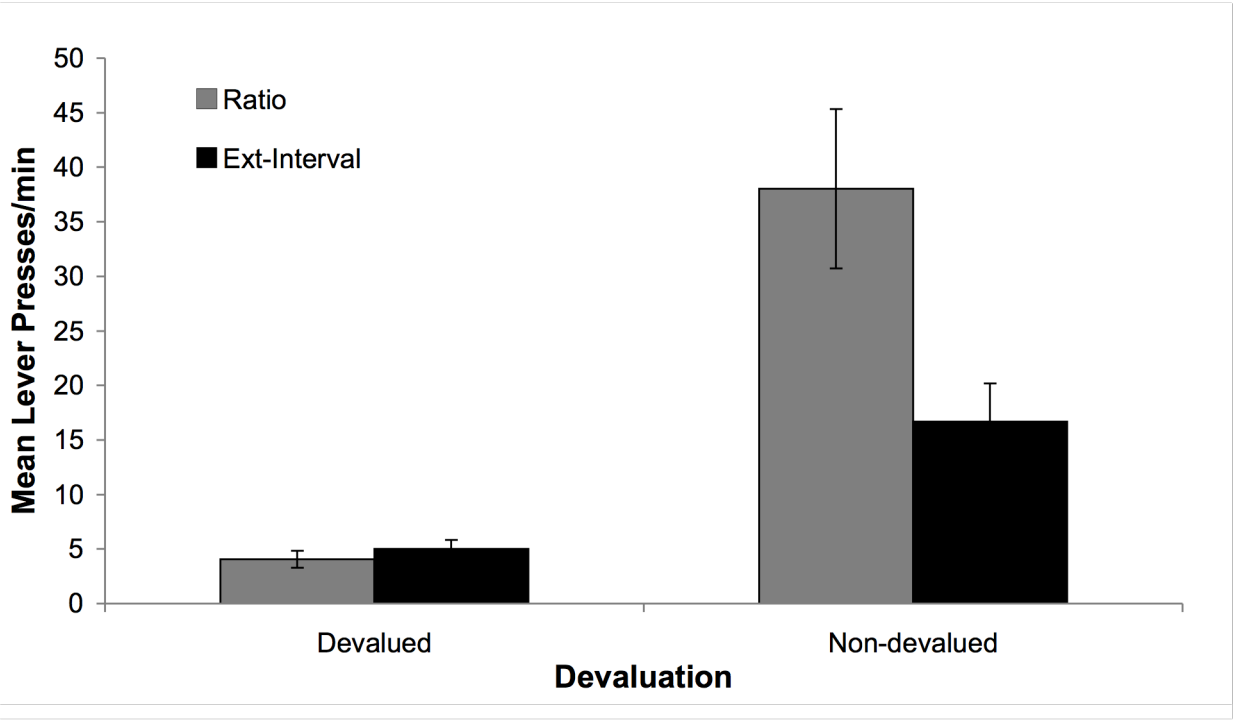

Figure 2.3. Results of the lever-press test between prefed (Devalued) and non-prefed (Non-devalued rats. A devaluation effect was observed for both Ratio- and Ext-Interval-trained subjects, suggesting that the Interval-trained rats lever pressed in a goal-directed manner, as per the subjects in the Ratio-trained condition. Error bars represent ± 1 standard error of the mean.

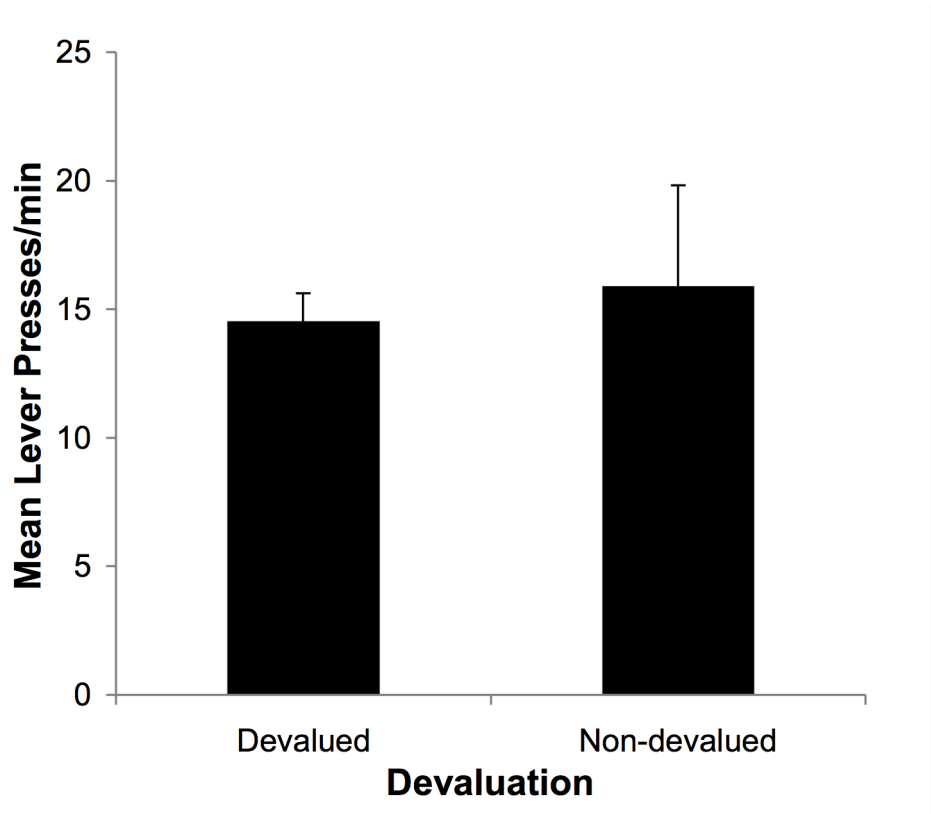

Figure 2.4. Results of the lever-press test between subjects that received taste aversion conditioning with sucrose (Devalued) and subjects that did not (Non-devalued). Rats in both Devaluation conditions produced similar lever-pressing rates, suggesting that the further extension of the extended interval training produced rats that lever-pressed habitually. Error bars represent ± 1 standard error of the mean.

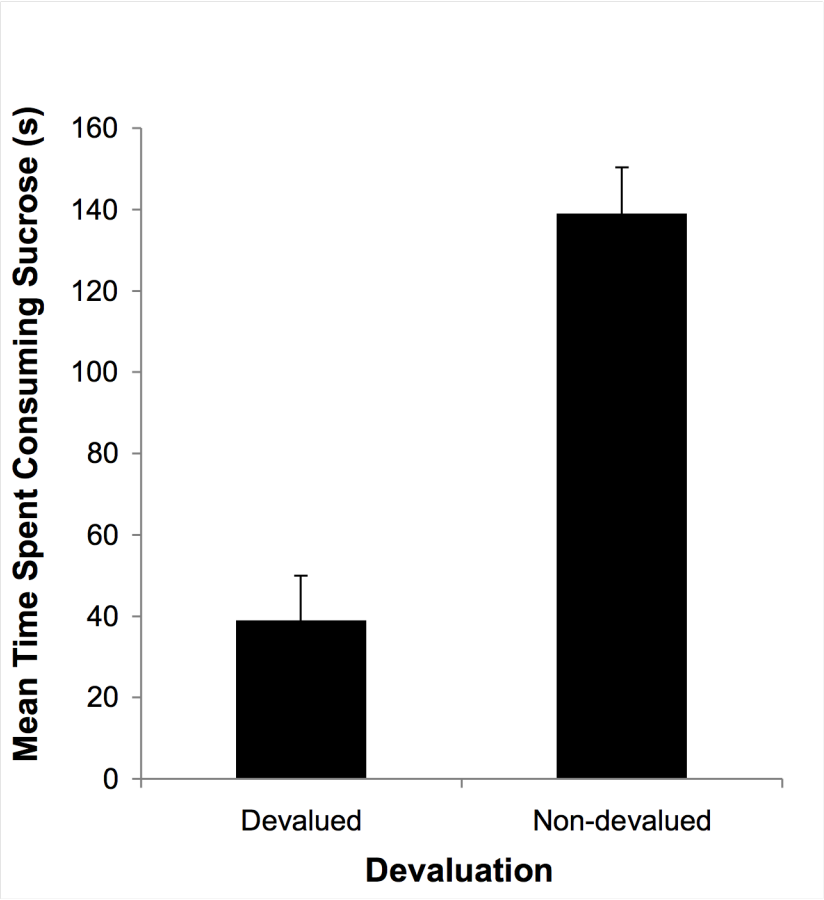

Figure 2.5. Results of the post-test consumption check. Subjects that received taste aversion conditioning with sucrose (i.e., Devalued) spent a significantly less amount of time drinking from the sucrose bottle compared to subjects that did not receive the conditioning (i.e., Non-devalued), suggesting that the taste aversion conditioning was successful. Error bars represent ± 1 standard error of the mean.

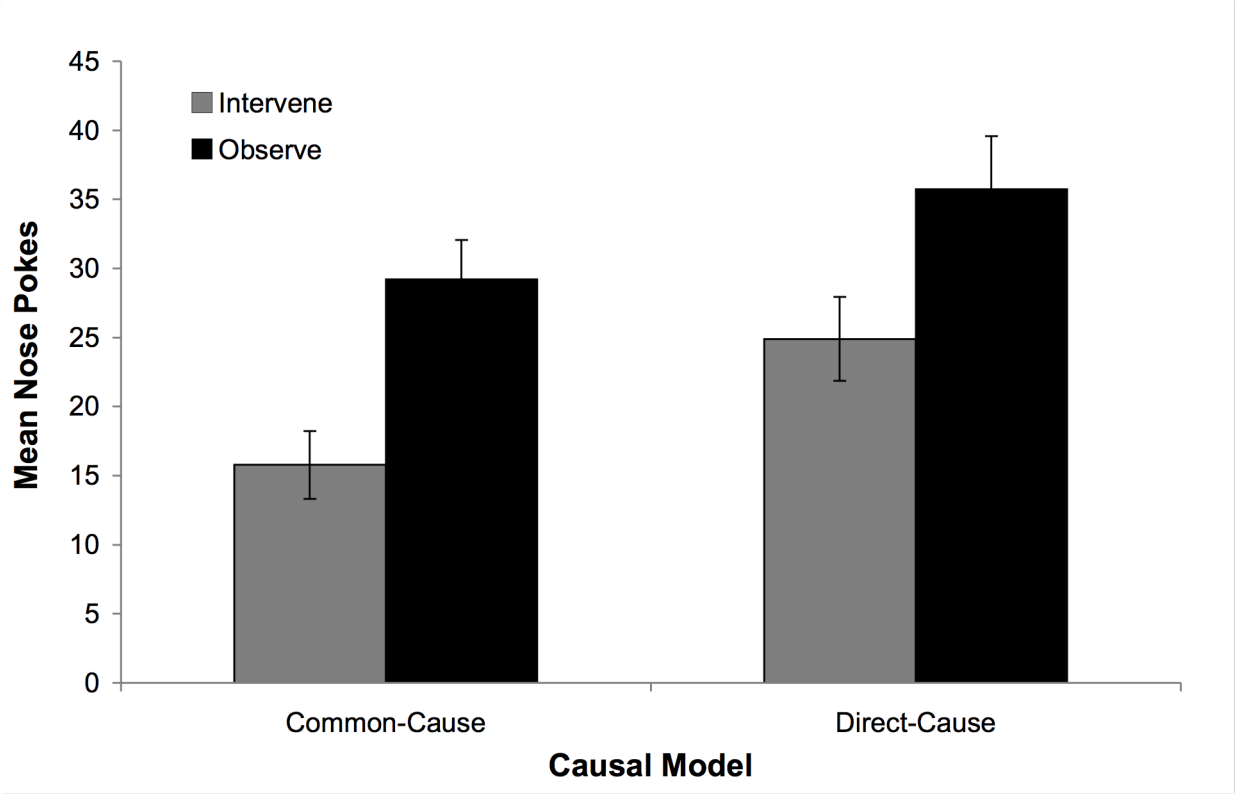

Figure 2.6. Mean nose pokes into the sucrose niche during presentations of Stimulus X (Common-Cause) or Y (Direct-Cause) from the replication of Leising et al. (2008) with all auditory stimuli turned down to 15 dB(A), presented as a function of Condition (Intervene vs. Observe) and Causal Model (Common-Cause vs. Direct-Cause). There was a main effect of Condition, wherein rats that Intervened to produce the Tone produced lower mean nose pokes than rats in the Observe condition. A main effect of Causal Model was also found, wherein rats tested on the Direct-Cause portion of the causal model produced higher amounts of nose pokes compared to rats tested on the Common-Cause portion. Though still without a significant Condition x Causal Model interaction, our results appear to be closer to a reproduction of the causal intervention effect, compared to the data reflected in Figure 2.1. Error bars represent ± 1 standard error of the mean.

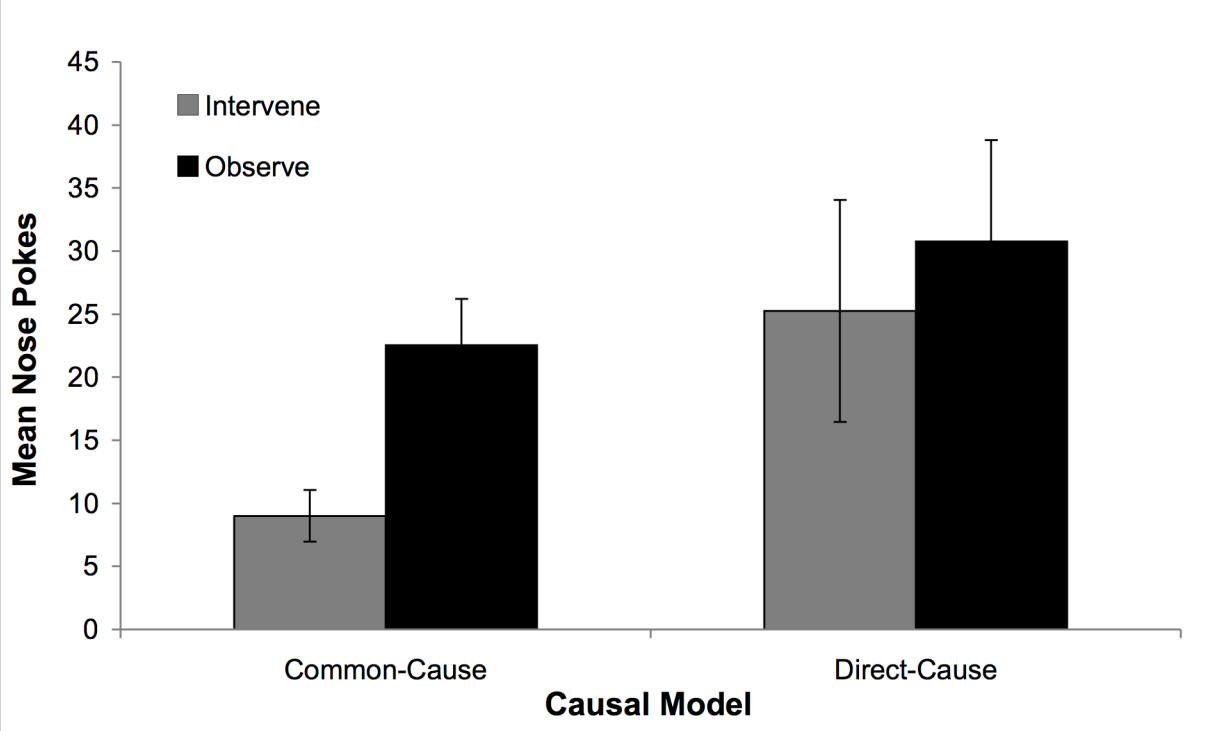

Figure 2.7. Mean nose pokes into the sucrose niche during presentations of Stimulus X (Common-Cause) or Y (Direct-Cause) from the replication of Leising et al. (2008) with all auditory stimuli turned further down to 8 dB(A), presented as a function of Condition (Intervene vs. Observe) and Causal Model (Common-Cause vs. Direct-Cause). Due to a loss of several subjects, only a main effect of Causal Model was revealed, such that rats that were tested on the Direct-Cause portion of the causal model produced more nose pokes, relative to rats that were tested on the Common-Cause portion. Although the desired Condition x Causal Model interaction was still not significant, the pattern of results suggest that we may have replicated the causal intervention effect if we had greater statistical power. Error bars represent ± 1 standard error of the mean.

CHAPTER 3

Are Causal Interventions Goal-directed in Nature?

3.1: Summary

This chapter describes the experiment associated with Aim 2, which investigates the nature of causal interventions. Particularly, are causal interventions goal-directed in nature? Goal-directed behavior is a type of instrumental behavior that is guided by expected outcomes. If it is indeed the case that rats that intervene to produce an outcome of a common cause do not also expect the other outcome to occur because they attribute the former outcome to their own actions, then it should follow that such causal interventions must also be sensitive to validation of expected outcomes, and thus driven by the goal-directed system. We put this hypothesis to the test by first training rats to lever press with either a brief random ratio or an extended random interval schedule of reinforcement for water to establish goal-directed or habitual instrumental behavior, respectively. Following this, we gave subjects training on common cause model as in Leising et al. (2008; i.e., Tone←Light→Food), with Food presentations (in the form of sucrose solution) in a separate niche. Rats were then tested with the opportunity to either intervene on the lever to produce the Tone, or merely observe it. If causal interventions are driven by the same neural substrates that drive goal-directed behavior, then we would expect ratio-trained rats that intervened to produce the tone to be sensitive to the outcome, thereby attributing the Tone to their own actions as opposed to the Light. As a consequence, they should not expect Food. Conversely, we would expect rats trained via an extended interval schedule that intervene on the Tone to be insensitive to the outcome associated with their lever presses, because their action should be driven by stimulus-response behavior (i.e., habitual; although it is crucial to note that it does not mean that these rats lack any expectation of outcome; it just means that their behavior is not under the control of this expectation). Thus, they should fail to attribute the Tone to their own actions, but rather to the Light, and therefore expect Food. All subjects that merely observed

the Tone should behave in the same manner, since presentations of Tone would occur independently of actions.

However, our results were inconclusive. We obtained only a significant main effect of niche, wherein unsurprisingly, subjects generally made more searches in the sucrose niche. Otherwise, there were no differences in response rates in the sucrose niche, regardless of Training (Ratio vs. Extended Interval) or Condition (Intervene vs. Observe).

3.2: Introduction

Suppose you just walked in a very dark bathroom. One of the first things you would probably do is to feel the walls by the entrance with your hands in search of a light switch. However, once you find one and attempt to flip it on, you might be surprised by the sudden whirring of a ventilation fan instead of seeing an illuminated bathroom. You would surmise that you must have flipped the wrong switch, and continue searching for the light switch. During this process however, you would probably attribute the turning on of the ventilation fan to your own action (i.e., pressing on the wrong switch), as opposed to some other cause. Now, imagine the same scenario, but instead of a ventilation fan turning on (which you might have had prior experience with), something completely foreign to you occurs – for example, an air horn suddenly goes off. Even though you probably have never experienced turning on an air horn with an electrical switch before, and even though you did not intend to turn on an air horn with the switch in the first place, you would still probably be more likely to attribute that air horn to your action on the switch than some other cause.

There is something seemingly goal-directed in nature about causal interventions, and considering that we manipulate causal structures in the world in order to bring out desired outcomes on a daily basis, this should not be a surprising notion. However, if an intervention

should bring about an unexpected outcome, it seems to result in an updating of our causal map – based on the example above, we will subsequently learn that the switch causes the ventilation fan (or the air horn) to turn on. Does this mean then, that on top of effective interventions, the systems that govern goal-directed action also promote the evaluation of novel causal links as well? If so, do brain systems that govern learning about goal-directed actions and consequences also subserve causal interventions?

As discussed in Chapter 1, goal-directed behavior is a form of instrumental behavior that is the basis of intentional actions. In contrast, habitual behavior is a form of instrumental behavior that is insensitive to action-outcome contingencies; the action is performed in the absence of volition insofar as behavioral choices are concerned. The dorsomedial striatum (DMS) and the doroslateral striatum (DLS) have been shown to govern these two types of instrumental behavior, respectively (see Chapter 4 for details). If it is indeed the case that causal interventions are underpinned by the potency of our actions, then it should follow that reasoning about causal interventions require us to be sensitive to the evaluation of action-outcome contingencies, as well as to be able to form representations pertaining to the outcomes of our actions. Thus, if an intervention is borne out of a habitual action, then the actor should be insensitive to any causal consequences of that action, and the actor should be less likely to attribute the outcome of that intervention to his or her own actions. Together, this posits that the DMS but not the DLS should be important for effective causal interventions.

To test the plausibility of this supposition, we launched a purely behavioral investigation into whether or not causal interventions depend on goal-directed behavior (i.e., driven by the DMS) with rats that have been pretrained to lever press in either a goal-directed (i.e., with a random ratio schedule) or a habitual manner (i.e., with an extended interval schedule), using

parameters that we established in 2.4.2. After instrumental lever pressing has been established, we gave all rats Pavlovian training on a common-cause model (i.e., Tone ← Light → Food; using the parameters established in 2.5.2) without the lever present, following the procedures of Leising et al. (2008). Half the rats within each training schedule were then randomly assigned to either the Intervene or the Observe condition. At test, levers were reinserted in the chamber, and the Tone was presented upon a lever press for rats in the Intervene conditions. For rats in the Observe conditions, the Tone was yoked to a corresponding rat in the Intervene conditions. If the effectiveness of a lever press depends on the goal-directed nature of lever pressing, then rats that received pre-training on a ratio schedule should show the intervention effect. That is, ratio-trained rats that produced the Tone via a lever press should attribute the tone to their own action (despite the lever being previously paired with water presentations) and discount the occurrence of the Light, and by extension, Food. As such, they should show low rates of searching in the food niche. Ratio-trained rats that merely observed the tone should attribute the Tone to the common cause – Light – and thus expect Food to also occur.

Conversely, if pre-training on an extended interval schedule resulted in instrumental behavior that was habitual, then lever pressing during the testing phase should also be habitual and insensitive to outcome. As a result, interval-trained subjects that produce the Tone via a lever press should fail to treat that Tone as a consequence of their own action; instead, they should treat the Tone as an outcome of the Light, and show high expectation of Food in the food niche. Interval-trained rats that merely observed the tone should behave similarly to the ratio-trained rats in the Observe condition.

3.3: Method

Subjects. Thirty-two experimentally-naïve female Long-Evans rats served as subjects. These rats were obtained from the same source and housed in the same manner as in all previous experiments described. Starting from one day prior to the beginning of the experiment, a water restriction schedule was imposed until the end of the lever-press shaping phase (see below). On the first day of water restriction (approximately 24 hours before the first experimental session), rats were given access to water for 30 min before the bottles were removed. During the magazine training and lever-press shaping portion of the experiment (Days 1-26), rats were given 30-minute access to water daily after each session. Once the causal-model training phase commenced (Days 27-33), subjects were taken off the water restriction schedule, and a food restriction schedule was enforced for the remainder of the experiment to maintain rats at 80% of their initial free-feeding weights.

Apparatus. Each of the eight experimental chambers, measuring 30 x 25 x 20 cm (L x W x H) was housed in separate sound- and light- attenuating environmental isolation chests (ENV-008, Med Associates, Georgia, VT). The front and back walls and ceiling of the chamber were constructed of clear Plexiglas, the side walls were made of aluminum, and the floors were constructed of stainless steel rods measuring 0.5 cm in diameter, spaced 1.5 cm center-to-center. The enclosure was dimly illuminated by a 28-V bulb (ENV-215M, Med Associates) house light located 2 cm from the top and 2 cm from the left of the left-side chamber wall. On the right-side chamber wall, a diffuse light (ENV-227M, Med Associates) was located 4 cm from the top and 4 cm from the left.

Each chamber was equipped with two liquid-dippers (ENV-202M, Med Associates) that could be lowered into a trough of water or sucrose solution (20%) and raised, one located at the

bottom left of the right-side chamber wall (for water presentations), and the other on the bottom right (for sucrose presentations). When in the raised position, a small well (0.05 cc) at the end of the dipper arm that contained water or sucrose solution protruded up into the drinking niche. In the middle of the metal wall of the chamber directly in between the two dippers, there was a 3.5-cm wide operant lever (ENV-112CM, Med Associates) that could be retracted from the chamber when not in use, located 8cm to the right from the water niche (and also 8cm to the left of the sucrose niche) center-to-center, and 2 cm above the floor grid. A speaker (ENV-223AM, Med Associates) on the exact center of the right-side chamber wall (flush to the ceiling of the chamber) could deliver a tone (T; 3 kHz) 8 dB(A) above background. CS L was a 10-s flashing light produced by flashing the diffuse light (with the house light turned off during the presentations) at a rate of 2 cycles (on-off) per second (See Table 3.1). CS T was a 10-s presentation of a tone. Ventilation fans in each enclosure and a white-noise generator on a shelf outside of the enclosures provided a constant 62-dB(A) background noise.

Procedure. The experimental design for this experiment is summarized in Table 3.1.

***Magazine training*.** Day 1. Water was delivered every 20 ± 15 s (actual ITI values = 5, 10, 15, 20, 25, 30, and 35 s) in a 60-min session to train rats to approach and drink from the water dipper. Reinforcement consisted of 10-s access to the raised dipper cup. During this phase (and up to Phase 1), the sucrose niche was covered by a completely solid stainless steel metal plate (each 12.5 cm high x 6.5 cm wide) that was held in place by two 3.5cm-diameter circular magnets (each 15 lbs. force), one placed on the top edge of the metal plate, and the other placed on the bottom edge of the metal plate. This served to prevent any possible latent inhibition that could interfere with the Light→Sucrose training in Phase 2.

Lever-Press Shaping. Days 2 – 26. During this phase, rats were trained to approach and press the operant lever to gain access to water. The first session of lever press shaping was conducted on a continuous reinforcement schedule in which each lever press was followed by access to water for 10 s. Each session terminated after 30 min had elapsed.

Additional daily 30-min training sessions were then conducted, with the duration of each water presentation reduced to 5 s. Rats in the Ext-Interval condition were trained under an extended random interval (RI) schedule of reinforcement with an average ratio of 15 s (RI-15s, 1 session), which progressed to RI-30s (1 session), and finally RI-60s (22 sessions). Rats in the Ratio condition were trained under a random ratio (RR) schedule of reinforcement with an average ratio of 5 (RR-5, 2 sessions), and progressed to RR-10 (2 sessions), and finally RR-20 (2 sessions). Rats in the Interval condition commenced lever-press shaping training before those in the Ratio condition; the training was scheduled in such a way that the last lever press shaping sessions in each group coincided with each other.

Phase 1 (sensory preconditioning). Days 27 – 30. The levers were retracted from this phase and in Phase 2. Rats were taken off the water restriction schedule and placed on a progressive food restriction schedule at this point (see above). Subjects in all groups received six daily trials on which Stimulus L was presented for 10 s followed by Stimulus T for 10 s. The onset of T coincided with the termination of L. Trials occurred with a mean interval of 5 ± 3 min (actual ITI values = 3, 4, 5, 6, and 7 min, randomly picked) during each 30-min session.

Phase 2 (first-order conditioning). On Days 31 – 32, the metal plate that had been covering the sucrose niche thus far was removed. Rats received 12 trials of L-sucrose pairings in each daily 30-min session. Sucrose was delivered at the termination of Stimulus A. Trials occurred with a mean intertrial interval of 3 ± 2 min (actual ITI values = 1, 3, and 5 min).

***Testing*.** Day 33. The levers were extended into the chambers only for rats in the Intervene condition, and T was presented in the following manner: Each lever press resulted in a presentation of Stimulus T. A corresponding rat in the Observe condition was yoked to each of these Intervene rats which received the same schedule of reinforcement, for which Stimulus T was also presented each time the rat in the Intervene condition made a lever press (i.e., each subject in the Intervene-Interval condition was yoked to one in the Observe-Interval condition, and each subject in the Intervene-Ratio condition was yoked to one in the Observe-Ratio condition). The diffuse light was covered with a 2"(w) x 2"(l) x ¾" (d) metal block during this phase. The amount of nose poke entries in both sucrose and water niches were recorded during each 10-s presentation of Stimulus T for all subjects.

Following this test session, rats were returned to their home cages and given ad libitum access to food pellets and water for 30 minutes. The rats were then put on water restriction with the removal of their water bottles (see 'Devaluation Test' below).

***Devaluation Test*.** Day 34. To assess if the lever-press shaping sessions during Days 2 – 26 were successful in producing goal-directed (rats in the Ratio condition) and habitual (rats in the Ext-Interval condition) instrumental behavior, we gave all subjects a devaluation test. Half of the rats in each training condition were given free access to water for 30 min (i.e., Devalued), whereas the remaining rats were not (i.e., Non-devalued). Immediately thereafter, all rats were placed in the chambers with the levers extended and given a 5-min extinction test on the levers (as was done in the experiments described in 2.3). We measured the total number of lever presses made by all rats, with which we used to calculate a mean rate (lever presses per minute).

3.4: Results

Figure 3.1 displays the mean number of nose pokes made in the sucrose and water niches, both as a function of Training (Ratio vs. Ext-Interval) as well as Condition (Intervene vs. Observe). A 2x2x2 ANOVA conducted with Niche (Sucrose vs. Water) as a within-subjects factor and both Training and Condition as between-subjects factors revealed only a main effect of Niche $F(1, 28) = 6.555$, $p = .016$. A post-hoc test indicated that subjects made significantly more nose pokes in the sucrose niche ($M = 12.304$, $SEM = 1.360$) than the water niche ($M = 7.435$, $SEM = 1.018$). Otherwise, analysis revealed no significant main effects of Training, $F(1, 28) = 1.830$, $p = .187$, nor Condition, $F(1, 28) = 1.150$, $p = .293$. Additionally, no significant interactions were found (Niche x Training x Condition, $F(1, 28) < .001$, $p = .983$; Niche x Training, $F(1, 28) = 2.428$, $p = .080$; Niche x Condition, $F(1, 28) = 1.521$, $p = .130$; Training x Condition, $F(1, 28) = 2.600$, $p = .118$).

The results of our devaluation test are shown in Figure 3.2, which displays the mean lever-pressing rate as a function of Training (Ratio vs. Ext-Interval) and Devaluation (Devalued vs. Non-devalued). Looking at the means, it appears that we obtained a clear devaluation effect for subjects in the Ratio condition, but not for subjects in the Ext-Interval condition. Indeed, a 2x2 ANOVA with Training and Devaluation as between-subjects factors reveals a significant main effect of Devaluation, $F(1, 28) = 8.449$, $p = .007$, as well as a significant Training x Devaluation interaction, $F(1, 28) = 7.242$, $p = .012$. No main effect of Training was found, $F(1, 28) = 2.437$, $p = .130$. Planned comparisons revealed that among Ratio-trained rats, subjects that did not receive the devaluation treatment produced significantly higher lever-pressing rates ($M = 16.275$, $SEM = 4.316$) than those that received the devaluation treatment ($M = 2.650$, $SEM = .458$), $t(14) = 3.140$, $p = .007$. In contrast, for the Ext-Interval-trained rats, lever-pressing rates

did not differ between Non-devalued (M = 5.925, SEM = 1.239) and Devalued (M = 5.400, SEM = 1.825) conditions, $t(14) = .238$, $p = .815$. This strongly suggests that even on the very last day of this experiment, ratio-trained rats were still responding to the lever in an outcome-driven (and thus goal-directed) manner, while rats that received the extended interval training were still responding to the lever in an outcome-insensitive (and thus habitual) manner.

3.5: Discussion

The data from this experiment indicated that regardless of Condition (i.e., Intervene or Observe) or Training (Ratio or Ext-Interval), rats generally made more responses in the sucrose niche than the water niche when the Tone was presented. Given that rats were no longer water restricted at the point of testing, this is not surprising. However, a limitation of this experiment was that all subjects could search in both sucrose and water niches within a single trial, and the current data do not allow us to discriminate between search locations within each trial, nor were we able to track a distribution of such responses temporally (i.e., did subjects search the sucrose niche first, and then move on to the water niche after discovering that sucrose was not delivered?). Additionally, we need to acknowledge the possibility that just because a rat has been trained to lever press in a habitual manner, it does not mean that it would later lack any knowledge at all about action-outcome contingency during testing, especially after aspects of context have changed in the time between the lever pressing phase and the causal model testing phases (e.g., the outcome is now delivered in another location, and this outcome is different than the previously trained outcome). Since no other main effects were found to be statistically significant, analyses can only be speculative.

Looking purely at the means, it can be seen that during Tone presentations, all rats in the Observe conditions (regardless of Training) tended to make more responses in the sucrose niche

than the water niche, suggesting that rats understood the causal model and made the correct inference. Rats trained on the extended interval schedule (i.e., Ext-Interval) that intervened on the lever to produce Tone also showed a similar level of response, which may support our hypothesis that interval-trained rats acted on the lever in a manner that was not guided by outcome, and attributed the Tone to the Light (and therefore also expected sucrose). Support for this notion can be drawn from the results from our devaluation test, which indicate that rats trained on the extended interval schedule lever pressed in a manner that was insensitive to outcome (i.e., habitual), whereas ratio-trained rats did so in a manner that was (i.e., goal-directed). Furthermore, it is worthwhile to note that the devaluation test was conducted eight days after the last lever press training sessions, and occurred after the rats had been undergone a degree of extinction on the lever during the casual intervention test. This illustrates the robustness of the instrumental effect that was obtained, and favors the ruling out of inadequate or failure in lever press training as reasons for the pattern of results.

The most curious aspect of the data lie in the fact that ratio-trained rats that intervened on the lever to produce the Tone also made high levels of nose pokes in the sucrose niche – as high as all subjects in the Observe condition, and the opposite of what was predicted. The main difference between this experiment and the ones done in Blaisdell et al. (2006) and Leising et al. (2008) is the lever-press training given to subjects prior to the causal model training and testing.

Perhaps this elevation in searching in the sucrose niche by ratio-trained Intervene rats is due to a Pavlovian-instrumental transfer-like process that was mediated by the causal model training (even though water and sucrose are different outcomes, Pavlovian-instrumental transfer has been demonstrated to apply to different but similar rewards – see Corbit, Janak, & Balleine, 2007; see Holmes, Marchand, & Coutureau, 2010 for a review of Pavlovian-instrumental

transfer): During lever-press training, rats learned that the lever led to a outcome with good appetitive value (i.e., water when they were thirsty). During the subsequent causal model training phase, although Tone and Sucrose – both effects of the Light – were never directly paired together, they were both co-effects within the common-cause model, and Sucrose was an outcome with good appetitive value that the Tone was indirectly associated with (insofar as the causal model is concerned). In fact, it is possible that the Tone acquired second-order biological significance during this training. When given the opportunity to act on the lever arose again during testing, the rats discovered that a lever press that previously resulted in water now resulted in the Tone, which retrieved the representation of the common-cause model, and hence Sucrose. Subjects probably still attributed the Tone to their own actions and not the Light (i.e., representations of the common-cause structure remains unchanged), but the relationship between the lever press and Sucrose was uncertain. Since the subjects had been trained that acting on the lever resulted in an outcome with good appetitive value, and if the lever press now caused the Tone to be presented, perhaps its co-effect Sucrose might also occurred (i.e., since Tone and Sucrose are co-effects, perhaps the lever might also now be a common cause, like Light). As such, these rats made searches in the sucrose niche. This was not observed in the original studies by Blaisdell et al. (2006) and Leising et al. (2008) because unlike this experiment, those rats had never been trained on the lever for an outcome prior to test.

We could frame this possible explanation with the causal model analogy used in Chapter 1 – namely, that atmospheric pressure is a common cause of changes in barometric reading and probability of rain (Figure 1.1, i.e., Barometer ← Atmospheric Pressure → Rainfall). If we learned this causal model, and we had an opportunity to press a button that would change the barometric reading, we would discount the probability of rainfall, since we would attribute the

change in barometric reading to our pushing the button, and we know that atmospheric pressure remains unchanged. However, suppose we have been given prior training that pushing the button causes something to happen that is similar to changing the probability of rainfall, for instance, the temperature (i.e., both weather effects; just as water and sucrose were similar appetitive outcomes for the rats). You push the button, expecting the temperature to change, but now discover that it has caused barometric reading to change. With the prior knowledge that your action on that button can in fact affect the environment, it is perhaps conceivable that you might also think that you had altered the probability of rainfall as well. In other words, the Barometer ← Atmospheric Pressure → Rainfall common-cause structure has not changed, but perhaps your action on the button is now also a common cause of change in both barometric change and rainfall probability. Thus, it could be the case that the ratio-trained rats that Intervened were still acting on the levers in a manner that was sensitive to outcome (as data from our devaluation test indicates), just that their behavior on the sucrose niche manifested in a way that was unexpected, and which warrants further investigation.

The aim of this experiment was to find out if subjects that responded to the lever in a manner that was outcome-sensitive (i.e., Ratio subjects) or outcome-insensitive (i.e., Ext-Interval subjects) to produce Tone made the correct inferences within the context of the causal model, as a behavioral means to assess if causal interventions depend on the DMS (which is involved in the learning of goal-directed actions). Unfortunately, because the data was inconclusive, this investigation would have to move towards a more directly neural manipulation.

Figures and Tables

Table 3.1

Experimental Design for the Experiment Described in Chapter 3.

Group	Pre-training	Causal Model Training		Test	Predicted sucrose magazine behavior (nose pokes)
Observe/Ratio	LP→water	L→T	L→Food	LP/T (Yoke)	High
Intervene/Ratio	LP→water	L→T	L→Food	LP→T	Low
Observe/Ext-Interval	LP→water	L→T	L→Food	LP/T (Yoke)	High
Intervene/Ext-Interval	LP→water	L→T	L→Food	LP→T	High
Note: LP = lever press, L = Flashing light, T = Tone, Food = sucrose presentation. Arrows indicate order of events during a trial. Events on either side of a slash were interspersed within the same session. L, T, and Food all lasted 10-s in duration.					

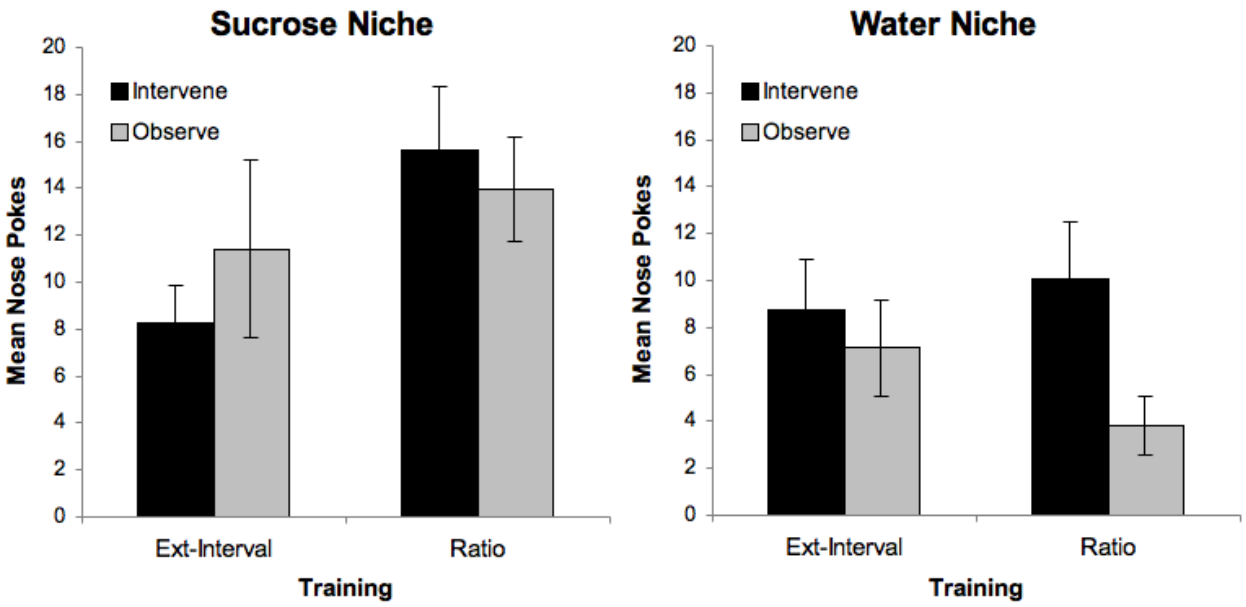

Figure 3.1. Mean nose pokes in both the sucrose and water niches during presentations of Stimulus T, presented as a function of Condition (Intervene vs. Observe) and Training (Ratio vs. Ext-Interval). Analysis revealed only a main effect of Niche, wherein rats generally produced more nose pokes in the sucrose niche than the water niche, regardless of Condition or Training. Error bars represent ± 1 standard error of the mean.

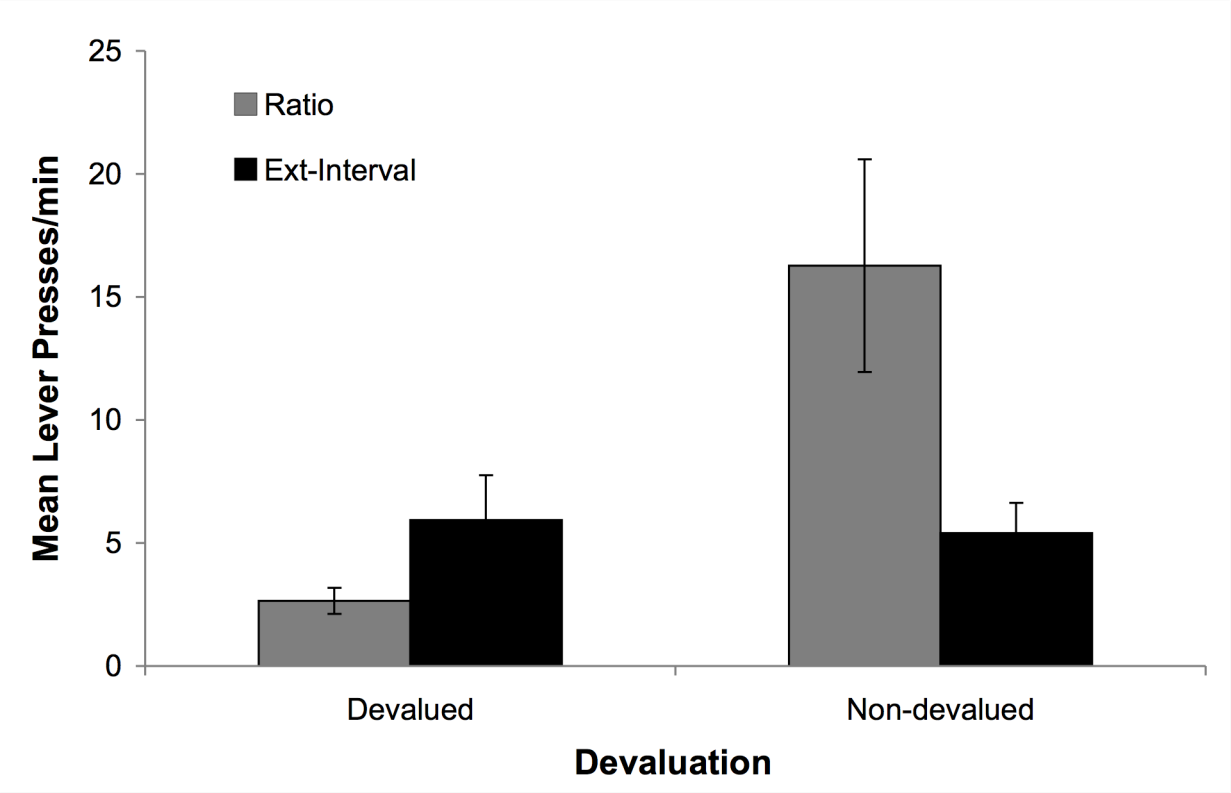

Figure 3.2. Mean lever-pressing rate during the devaluation test, presented as a function of Training (Ratio vs. Ext-Interval) and Devaluation (Devalued vs. Non-devalued). A main effect of Devaluation was obtained, as was a significant Devaluation x Training interaction. Specifically, rats that received lever press training with a ratio schedule showed a devaluation effect, whereas rats that received training with an extended interval schedule did not. This suggests that rats that received ratio training produced lever presses in a goal-directed manner, whereas rats that received extended interval training lever pressed in a habitual manner, thus evincing that our lever press training procedures were successful. Error bars represent ± 1 standard error of the mean.

CHAPTER 4

Investigations into the Neural Basis of Interventions: The Dorsomedial Striatum

4.1: Summary

In Chapter 3, we attempted to investigate if causal interventions are underpinned by goal-directed behavior, but results were inconclusive, owing at least in part unforeseen variables that are difficult to control for. Perhaps a more direct way to get at the question would be neural manipulation. The dorsomedial striatum (DMS) and dorsolateral striatum (DLS) have been shown to play an important role in goal-directed and habitual instrumental behavior, respectively. Particularly, lesions of the DMS have been shown to disrupt goal-directed but not habitual behavior (Yin et al., 2005b), and lesions of the DLS have been shown to disrupt habitual behavior, while preserving goal-directed behavior (Yin et al., 2004). Together, the data indicate that the DMS is a critical locus for the acquisition and expression of the instrumental action-outcome association (Yin et al., 2005b), while DLS activity is not correlated with outcome expectancy, and instead mediates responding driven by antecedent stimuli (Yin et al., 2004). A further distinction within the DMS was found – only post-training lesions of the anterior DMS (aDMS) disrupted goal-directed behavior, while both pre and post-training lesions of the posterior DMS (pDMS) disrupted goal-directed behavior (Yin et al., 2005b), suggesting that the pDMS is necessary for both the acquisition and expression of action-outcome associations in instrumental learning. Thus, we performed an experiment wherein rats received pre-training lesions of the pDMS or a sham lesion. Thereafter, we gave rats training on a common-cause model (i.e., Tone ← Light → Food). During testing, rats within each lesion condition were given an opportunity intervene (via a lever press) or observe for Tone, and their expectancies of Food were assessed. Given the role of the pDMS is the acquisition of action-outcome associations during instrumental learning, if causal interventions are indeed driven by knowledge of outcomes of actions on the lever, then pDMS but not sham-lesioned subjects should not attribute the Tone

to their action on the lever; rather, they should attribute it to the Light, and thus expect Food. Rats with sham lesions should show the causal intervention effect and show low expectancy of Food, while all rats that observed the Tone should show high expectancy of Food.

Due to insufficient statistical power, the results of this experiment were unfortunately inconclusive. However, speculation of the means alone indicate that both pDMS-lesioned and sham rats showed the causal intervention effect, thereby suggesting that action-outcome associations in causal interventions may not be driven by the same systems that govern action-outcome associations in goal-directed actions. Nevertheless, no real inferences can be made based on the current data.

4.2: Introduction

In Chapter 3, a behavioral experiment aimed at investigating if causal interventions were goal-directed in nature was described. Results from that study were unfortunately inconclusive. Another way to get at the question would be a lesion study, which would provide a much cleaner design, on top of getting at the question more directly.

On top of its involvement in the reinforcement of actions that potentially leads to reward (Tricomi, Delgado, & Fiez, 2004), there is current evidence that implicates the dorsal striatum in decision-making (action selection and initiation) via integration of inputs from cognitive, sensorimotor, motivational, and emotional systems. With this information, it regulates goal-directed and habitual actions, thereby generating cognitive control of actions (e.g., Balleine & Ostlund, 2007; Balleine, Delgado, & Hikosaka, 2007; Valentin, Dickinson, & O'Doherty, 2007). Thus, if the effectiveness of interventions depends on the goal-directed nature of the action, do brain regions that govern the goal-directed system but not the habitual system also underpin causal interventions? The dorsomedial stratum (DMS) and the dorsolateral striatum (DLS) have

recently been implicated in goal-directed and habitual behavior, respectively (e.g., Yin, et al. 2004 and Yin et al., 2005b, respectively). Specifically, studies involving lesions (Yin et al., 2005b) or blockade of NMDA receptors (Yin, Knowlton, & Balleine, 2005a) on the DMS have produced disruption of goal-directed behavior, while preserving habitual instrumental behavior. Conversely, inactivations of the DLS selectively preserved goal-directed behavior, but disrupted habit formation in instrumental learning (Yin et al., 2004). Together, the data implicates the DMS as a critical locus for the acquisition and expression of the instrumental action-outcome association (Yin et al., 2005b), while DLS appears to mediate responding driven by antecedent stimuli since its activity has not been found to correlate with outcome expectancy (Yin et al., 2004). Additionally, a further difference was found between lesions of the anterior DMS (aDMS) and posterior (pDMS) dorsomedial striatum: Goal-directed instrumental behavior was disrupted with post-training aDMS lesions, while this disruption was found in both pre- and post-training lesions of the pDMS (Yin et al., 2005b), which indicates that the pDMS is important for both the acquisition and expression of action-outcome associations in instrumental behavior. Thus, the pDMS would appear to be a target region of interest for a study involving a pre-training lesion.

This chapter presents an experiment that used pre-training lesions of the pDMS to investigate the role of goal-directed action in causal interventions. Rats first received either pDMS or sham lesions. Following recovery, they were given the same common cause training as per Leising et al. (2008; i.e., Tone←Light→Food). Rats then received either Intervene or Observe trials during testing. Given the role of pDMS in the acquisition of action-outcome associations during instrumental task learning (Yin et al., 2005b), and if the effectiveness of interventions depends on knowledge of outcomes from the action of lever pressing, then pDMS-lesioned rats but not sham lesioned rats should fail to attribute the Tone to their own actions, thus

producing high rates of nose-poking into the Food niche, despite their intervention on the Tone. Sham-lesioned rats that intervened on the Tone should attribute the Tone to their own actions, and make fewer nose pokes into the Food niche as such. All rats that observed the Tone should attribute the Tone to the Light, and thus show high rates of expectancy of Food (i.e., high levels of nose-poking). However, if both pDMS-lesioned and sham rats show the causal intervention effect (i.e., low expectation of food when the Tone was intervened upon, with high expectation of food if the Tone was merely observed), then it will suggest that the DMS itself, while being important for action-outcome associations in goal-directed instrumental behavior, does not also serve causal interventions.

4.3: Method

Subjects. Sixteen experimentally-naïve female Long-Evans rats served as subjects. They were housed in the same manner as 3.3. As before, experiments were conducted during the dark portion of the cycle and all animals were handled for 45 s daily. Half the rats were randomly assigned to receive pDMS lesions, while the remaining half received sham lesions. Within each lesion condition, half the subjects were pseudorandomly selected to be in the Intervene condition, while the remaining half were in the Observe condition.

Apparatus. The same experimental chambers as in Chapter 3 were used, except that the layout was configured differently: The enclosure was dimly illuminated by a 28-V bulb (ENV-215M, Med Associates) house light located 2 cm from the top and 2 cm from the left of the left-side chamber wall. On the right-side chamber wall, a diffuse light (ENV-227M, Med Associates) was located right in the center and 4 cm from the top. Each chamber was equipped with one liquid dipper (ENV-202M, Med Associates) that could be lowered into a trough of sucrose solution (20%) and raised, located in the middle and bottom of the right-side chamber wall. On

the metal wall of the chamber directly left of the dipper, there was a 3.5-cm wide operant lever (ENV-112CM, Med Associates) that could be retracted from the chamber when not in use, located 8cm to the left from the sucrose niche center-to-center, and 2 cm above the floor grid. A speaker (ENV-223AM, Med Associates) on the exact center of the right-side chamber wall (flush to the ceiling of the chamber and on top of the diffuse light) could deliver a tone (T; 3 kHz) 8 dB(A) above background. CS L was a 10-s flashing light produced by flashing the diffuse light (with the house light turned off during the presentations) at a rate of 2 cycles (on-off) per second. CS T was a 10-s presentation of a tone. Ventilation fans in each enclosure and a white-noise generator on a shelf outside of the enclosures provided a constant 62-dB(A) background noise.

Procedure. The experimental design for this experiment is summarized in Table 4.1.

Surgery and recovery. Rats were anesthetized by placing them in an isoflurane induction chamber (oxygen flow rate of 2 L per minute; isoflurane concentration at 5%). After 3-5 minutes when the rats were deeply anesthetized, they were removed from the chamber, and their hairs were shaved from their heads using barber clippers, with care being taken not to abrade the skin. The rats were then mounted in a stereotaxic frame, and a rodent anesthesia mask was placed over their noses to deliver isoflurane gas anesthesia throughout surgery (the oxygen flow rate was set at 0.5 – 2.0 L per minute, and the isoflurane concentration was set between 2.5 – 3.0% to maintain the rats at a constant level of anesthesia). The depth of anesthesia was monitored throughout the procedure by checking the rats' respiration rate, heart rate, footpad, color, and toe-pinch reflex response; the lowest concentration of isoflurane necessary to maintain anesthesia was always used. While in the stereotax, the rats' bodies rested on an isothermal heating pad that maintained body temperature at 37°C throughout the duration of the procedure. A cloth was placed between the rats and the heating pad, and a sterile drape was placed over the

rats' bodies so that only the surgical work area on the head was exposed. The rats were then given an injection of buprenorphine (0.03 mg/kg) as a preemptive analgesic, and bland ophthalmic ointment was placed in the rats' eyes to prevent dessication. The shaved area of the scalp was then thoroughly disinfected with three alternating scrubs of surgical soap (Betadine) followed by a wipe with 70% ethyl alcohol. Following this, the cranium was exposed by a single anterior-posterior incision of the skin with a scalpel. The skull surface was scraped clean with forceps, and wiped dry with sterile cotton swabs.

Using a dental drill, small burr holes (about 1mm in diameter) were drilled into the skull bilaterally, and 28 gauge cannulae were lowered into the brain at the following coordinates for both pDMS lesion (n = 8) and sham (n = 8) subjects: 0.4 mm posterior, 2.6 mm lateral to bregma, and 4.5 mm below skull surface. For pDMS lesion rats, the volume of NMDA (0.12 M; Sigma, St. Louis, MO) infused was 0.4 μL per side over 3 min. For sham lesion rats, saline was infused instead, at the same volume and rate. Two minutes after the infusion, the cannulae were removed, sterile wound clips were used to close the incisions, and antibiotic ointment (Neosporin) was applied generously over the wound.

The rats were then placed in an empty cage with no bedding while they emerged from anesthesia. Following that, they were returned to their home cages and given access to food and fresh water (ad libitum) with antibiotic added in the water (Sulfamethoxazole & Trimethoprim; at a concentration of 0.25 mg per mL) to prevent the development of infection. All rats were given seven days for recovery, and this antibiotic was given in the water during these days. Additionally, on the day after the surgery, rats were given another injection of buprenorphine (dosage as before).

Food restriction. Seven days after surgery, all rats were placed on a progressive food restriction schedule over five days to maintain rats at 85% of their initial free-feeding weights. Rats were always given ad libitum access to fresh water throughout the experiment.

Magazine training. Day 1. As in 3.3.

Phase 1 (sensory preconditioning). Days 2-5. As in 3.3.

Phase 2 (first-order conditioning). Days 6-7. As in 3.3.

Testing. Day 8. As in 3.3.

Perfusion and histology. At the end of the experiment, prior to perfusion, rats were given a lethal dose (100 mg/kg) of sodium pentobarbital via IP injection. When the rats were deeply anesthetized and exhibited no toe-pinch reflex, the chest cavity was opened by making a small scissor cut through the skin and muscle just below the xiphoid process. The scissors were then inserted into the opening, and then used to cut through the ribs along both sides of the breastplate. The breastplate was retracted with a hemostat, allowing access to the heart. A blunt 20 g needle (attached by plastic tubing to a peristaltic pump) was inserted into the left ventricle of the heart. A small scissor cut was then made in the right atrium to drain the blood. The pump was turned on to transcardially perfuse 10% formalin in phosphate buffered saline (PBS) at a flow rate of 1 – 5 mL/min. The rats were monitored for fixative tremors, which are an indication that the perfusion is successful. Perfusion was considered complete when the tremors have stopped, internal organs appeared pale in color, the rats' bodies were rigid, and the fluid exiting the right atrium was clear in color. Following perfusion, the rats were decapitated, and the fixed brains were removed using rangeurs to gently crack and peel away the skulls and meninges to expose the entire brain. The extracted brains were stored in 50 mL glass scintillation vials containing 30% sucrose formalin solution. After several days when the brain has sunk to the

bottom of the vial (indicating that the sucrose has permeated and cyroprotected the brain), it was sectioned with a cryostat at coordinates ranging from -1.4 and 1.4 mm posterior to bregma. Sliced tissue sections were then mounted on glass slides, stained with cresyl violet, and coverslipped. Sections were digitally photographed at 4x magnification.

4.4: Results

Due to an accident that occurred during surgery, one rat in the sham lesion group was excluded from the experiment, leaving a total of 7 subjects in the sham lesion group, and 8 subjects in the pDMS lesion group.

Figure 4.1 shows representative photomicrographs of the lesion and sham lesions. The most noticeable difference between the pDMS lesions and the sham lesions is a widening of the lateral ventricles in the pDMS-lesioned brains, indicating cell loss and shrinkage in the target region below the corpus callosum.

Otherwise, Figure 4.2 illustrates the average number of nose pokes made by rats during Tone presentations as a function of Lesion (pDMS vs. Sham) and Condition (Intervene vs. Observe). A 2x2 ANOVA with both factors as between-subjects factors revealed neither a main effect of Lesion, $F(1, 11) = 1.156$, $p = .305$, nor a main effect of Condition, $F(1, 11) = 3.404$, $p = .092$. There was also an absence of a significant interaction, $F(1, 11) = .985$, $p = .342$.

4.5: Discussion

The data suffers from a clear lack of power, with only four subjects in each of the conditions (and only three subjects in the Sham/Observe condition). This would be rectified by an increase in the number of subjects used. Looking simply at the means however, it appears that all subjects that observed the Tone produced a higher mean number of nose pokes into the sucrose niche than subjects that intervened to produce the Tone, regardless of lesion condition.

This is indicative of the causal intervention effect, which suggests that subjects learned the common-cause model, and were able to navigate through it logically. Purely speculating on the current data alone however, it appears that the DMS, while known to be involved in learning about actions and outcomes in goal-directed actions, does not appear to drive action-outcome associations in causal interventions. Nevertheless, with the current statistical power, this suggestion could be just as plausible as the inverse.

On top of the small sample size, the possibility that our data reflects a false negative may be gleaned from the fact that our lesion procedures were adapted directly from Yin and colleagues (2005b), who obtained the desired effects (i.e., inability of rats to acquire action-outcome associations in instrumental behavior). We followed their described procedures exactly, suggesting that our pDMS lesions produced should have been identical to those obtained by them.

However, it is still possible that our results are true negatives due to the lesion misplacement. Since the region of interest was a specific part of the much larger striatum, even if the photomicrographs showed an enlargement of the lateral ventricles from a shrinkage of the mediodorsal portion of the striatum (Figure 4.1), it cannot be certainly confirmed if this was the same specific area that was targeted by Yin et al. (2005b) due to technical reasons that cannot be avoided when replicating procedures across different laboratories. Certainty of the targeted region could have been improved with a behavioral manipulation check conducted after the test session. Specifically, all subjects could have been put on a lever-press task to see if the pDMS-lesioned rats showed abolishment of goal-directed behavior (and thus were outcome-insensitive, which was an assumption upon which our experimental paradigm lay). If it was the case that we

were subsequently able to obtain goal-directed instrumental behavior in subjects that were given lesions of the pDMS, the test data of those subjects should be discarded.

Another issue that requires consideration is the fact that as of the present, no structures other than the pDMS have been studied in relation to action-outcome associations as they pertain to causal interventions, and gaining more insight into the neural substrates mediating causal interventions based on one brain structure alone is tenuous at best. For instance, the pDMS receives rich projections from the infralimbic cortex, lesions of which have shown it to be critical for acquisition (but not expression or storage) of goal-directed instrumental learning (Balleine and Dickinson, 1998; Corbit and Balleine, 2003). Thus, it is possible that the pDMS receives information about action-outcome sensitivity from the prelimbic cortex, and uses it to guide goal-directed behavior. At the same time, perhaps the prelimbic cortex may also project information to currently unknown brain structures that ultimately drive causal interventions. If this were to be the case, then it would support the current trend of our data that does not implicate the pDMS in action-outcome associations in causal interventions. Such a possibility certainly demands further investigation.

Otherwise, in the absence of a higher sample size, the implications of the data remain uncertain.

Figures and Tables

Table 4.1

Experimental Design for the Study Described in Chapter 4.

Group	Lesion	Causal model training		Test	Predicted magazine behavior (nose pokes)
pDMS/Intervene	pDMS	L→T	L→Food	LP→T	High
pDMS/Observe	pDMS	L→T	L→Food	LP/T (Yoke)	High
Sham/Intervene	Sham	L→T	L→Food	LP→T	Low
Sham/Observe	Sham	L→T	L→Food	LP/T (Yoke)	High
Note: pDMS = Posterior DMS, LP = lever press, L = Flashing light, T = Tone, Food = sucrose presentation. Arrows indicate order of events during a trial. Events on either side of a slash were interspersed within the same session. L, T, and Food all lasted 10-s in duration.					

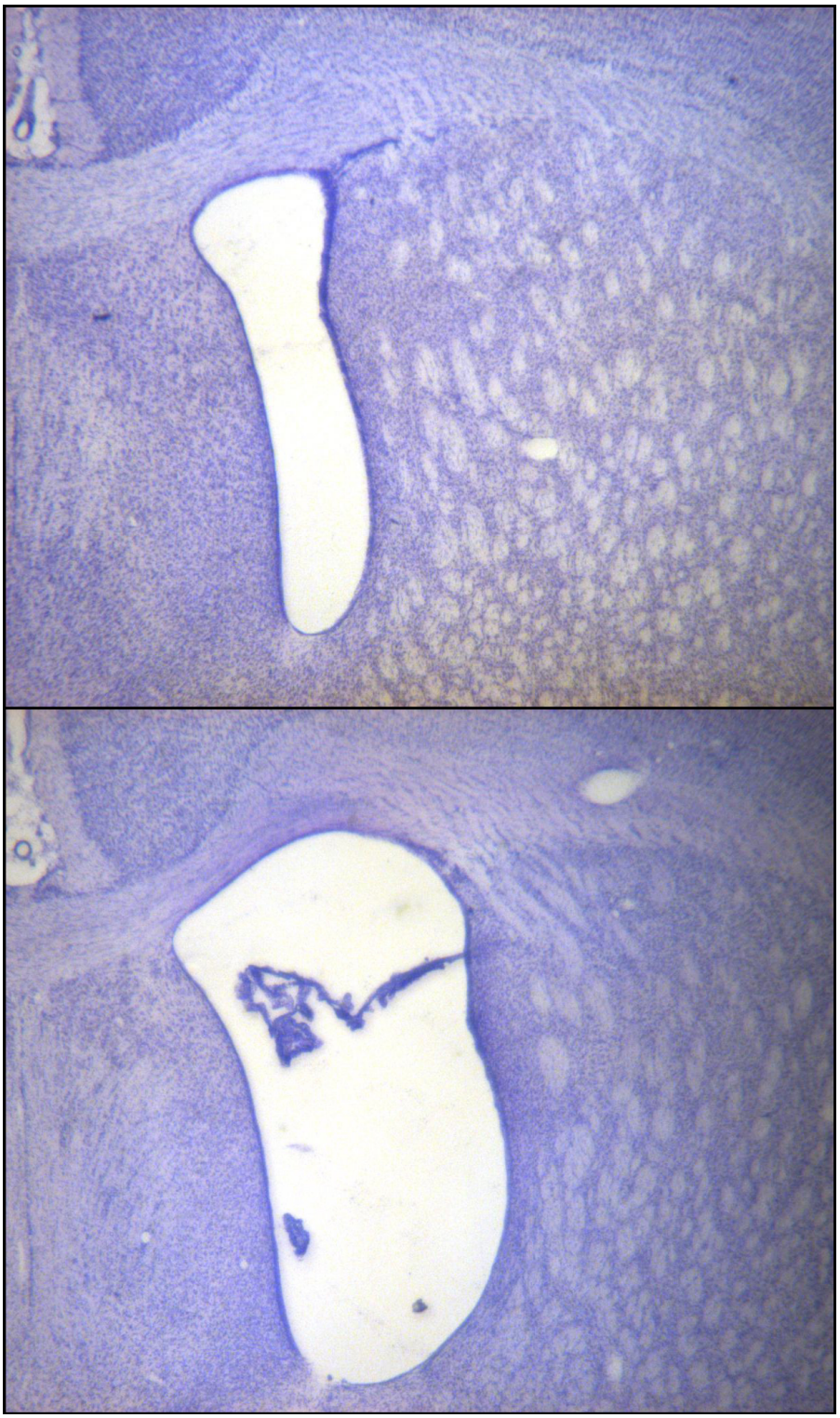

Figure 4.1. Photomicrograph of representative lesions of sham controls (top panel) and the posterior dorsomedial striatum (pDMS; bottom panel).

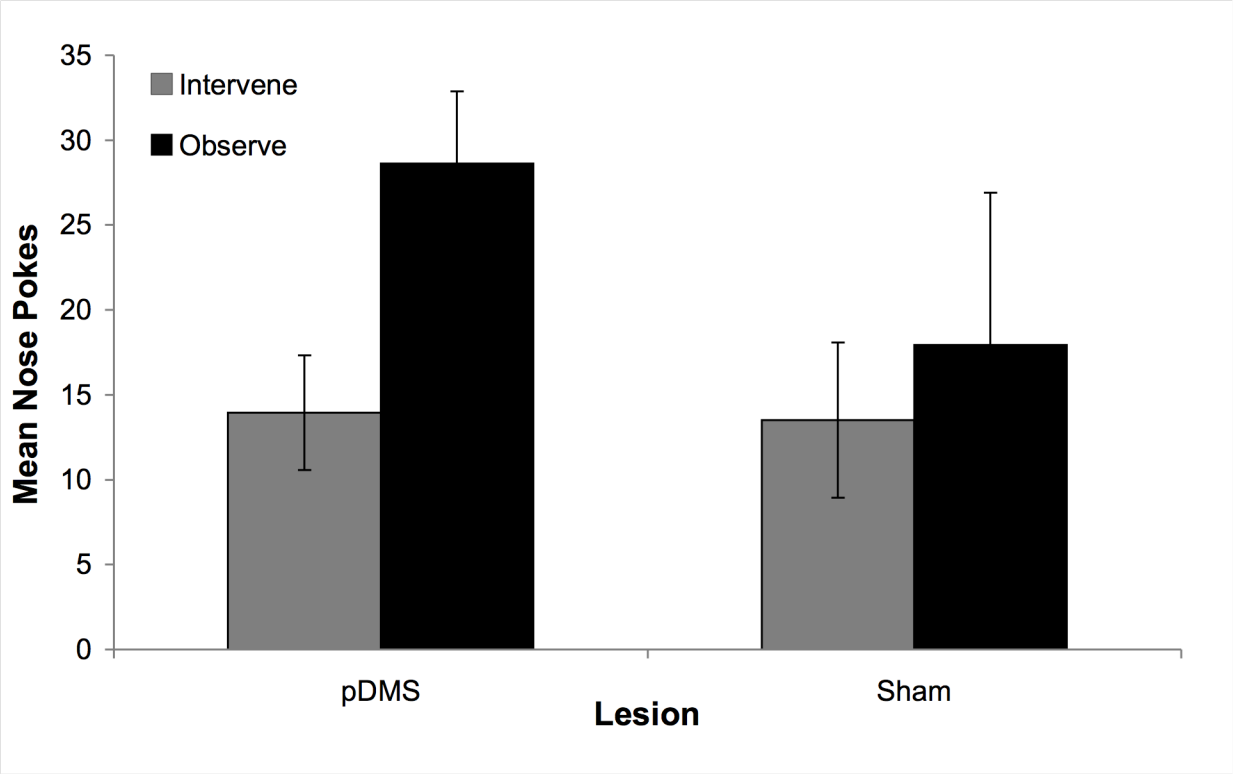

Figure 4.2. Mean nose pokes in the sucrose niche during trials of Stimulus T as a function of Lesion (pDMS vs. Sham) and Condition (Intervene vs. Observe). Neither significant main effects nor an interaction was found, probably due to small sample sizes (n = 4 in pDMS/Intervene, pDMS/Observe, and Sham/Intervene conditions; n = 3 in Sham/Observe condition). Error bars represent ± 1 standard error of the mean.

CHAPTER 5

General Discussion

5.1: Summary of Findings

In light of evidence that rats were able to effectively navigate through a causal model in order to make logical causal inferences, based on whether or not they intervened upon or merely observed one of the effects (i.e., Blaisdell et al., 2006; Leising et al., 2008), the experiments described in this dissertation described experimental investigations into the neural basis of causal interventions. Based on recent findings that implicated the dorsomedial striatum (DMS) in goal-directed actions, and since causal interventions require sensitivity to the outcome of actions in order to be effective, we postulated that the DMS might be important for causal interventions, and performed experiments to investigate the validity of this hypothesis.

Establishing the basic effects: Goal-directed/habitual instrumental behavior and the causal intervention effect. The first aim of this dissertation described in Chapter 2 was to establish appropriate experimental parameters before the crux of our studies could be performed. Namely, we needed to reestablish the causal intervention effect as found in Blaisdell et al. (2006) and Leising et al. (2008), and to formulate an optimal training regiment to produce rats that lever pressed in a goal-directed (i.e., outcome-guided) or a habitual (i.e., outcome-insensitive) manner. For the latter, we started off by replicating the procedures used by Yin et al. (2004) and Yin et al. (2005b). While we faced no issues in obtaining goal-directed instrumental behavior, we were able to establish habitual instrumental behavior only after increasing the total number of lever press training sessions (via a random interval schedule) to 24, as opposed to just 6 as described in Yin et al. (2005b).

As for reestablishing the causal intervention effect, we appeared to have lost the ability to obtain it ever since the auditory stimuli used in our experimental chambers were turned up to 20 dB(A) above background. Hypothesizing that the increased amplitude might have caused

startling when rats were given an opportunity to produce the stimuli via a lever press (which might have interfered with their ability to generate an appropriate causal diagnosis and hence response during the period of stimuli presentation), we eventually rescuing the effect after turning the amplitudes down to 8 dB(A) above background. These parameters were used in the remaining experiments described in this dissertation.

Are causal interventions goal-directed in nature? After the necessary experimental parameters were established, we performed an experiment to determine if causal interventions were goal-directed in nature (described in Chapter 3) by first pre-training water-restricted rats on either a random ratio or extended random interval schedule of reinforcement on a lever-pressing task so as to obtain either goal-directed (outcome-sensitive) or habitual (outcome-insensitive) instrumental behavior, respectively, with water as an outcome. Following this training, rats were taken off water restriction, put on food restriction, and given training on a common-cause model (Tone ← Light → Sucrose). During testing, they were given an opportunity to either intervene on the lever to produce Tone. We hypothesized that if causal interventions are goal-directed in nature, then ratio-trained rats that intervened on the Tone should show the causal intervention effect (i.e., discounting of Sucrose), while rats trained on the extended random interval schedule that intervened on the Tone should attribute the Tone to the Light instead of their actions on the lever, and thus should not have discounted Sucrose. Rats that merely observed the Tone should also shown a lack of discounting to the Sucrose, regardless of instrumental training (levers were not present in their chambers at test).

Our results indicated that generally, subjects made significantly more searches in the sucrose niche but not the water niche, which was not a surprising finding since they were food- but not water-restricted during testing. Otherwise however, regardless of instrumental training or

testing condition (i.e., Intervene or Observe), all subjects made a similar amount of searching in the sucrose niche during presentations of Tone. A subsequent devaluation test indicated ratio-trained rats that had been given an opportunity to drink water prior to the test showed the outcome devaluation effect, while ratio-trained rats that did not receive the water did not (i.e., goal-directed behavior). Conversely, all subjects trained on the extended interval schedule did not show the devaluation effect, regardless of whether or not they were given an opportunity to drink water prior to the devaluation test (i.e., habitual behavior). This indicated that our instrumental training was successful, and was not responsible for our unexpected pattern of results. As such, only speculations can be made about why ratio-trained rats that intervened on the Tone did not show signs of discounting of Sucrose.

Investigations into the neural basis of causal interventions: The dorsomedial striatum. We followed up the previous experiment with one that involved a direct neural manipulation, as described in Chapter 4. The targeted structure was the posterior dorsomedial striatum (pDMS), pre-training lesions of which has shown to abolish outcome-sensitive goal-directed instrumental behavior (Yin et al., 2005b). As such, subjects were either given pre-training lesions of the pDMS, or a sham lesion. Following recovery, rats were trained on a common-cause model (Tone ← Light → Sucrose) before subsequently given an intervene/observe test as in the previous experiment. We hypothesized that pDMS-lesioned rats that intervened to produce the Tone should not show discounting of Sucrose, just as all rats that merely observed the Tone (regardless of lesion type) should. Sham-lesioned rats that intervened to produce the Tone should show the causal intervention effect, thus discounting Sucrose.

Unfortunately, due most likely to a low number of subjects, the data revealed no main effects or interactions, and was inconclusive as such. Speculation on the direction of the means

alone seems to suggest that both pDMS- and sham-lesioned rats that intervened to produce the Tone showed the causal intervention effect, thus suggesting that while the pDMS might serve in the learning of action-outcome contingencies in goal-directed instrumental behavior, it does not subserve such contingencies in causal interventions. Without added statistical power however, the opposite could be just as plausible.

5.2: Future Avenues of Research and Theoretical Implications

Previous evidence has implicated the pDMS as a critical locus for both the acquisition and expression of the instrumental action-outcome association (Yin et al., 2005b). If it happens to be the case that the pDMS is not important for causal interventions (which, pending further research, still remains to be seen), then the next logical question would be: What about other brain structures?

There are regions other than the DMS that have been associated with goal-directed behavior, such as the prelimbic cortex, which has rich projections to the DMS. Specifically, lesions of the dorsal prelimbic cortex have been shown to abolish the acquisition of goal-directed instrumental behavior (Balleine and Dickinson, 1998; Corbit and Balleine, 2003). This differs from the DMS in the aspect that lesions of the DMS also abolishes the expression (on top of acquisition) of goal-directed instrumental learning, which suggests that the prelimbic cortex plays a more specific role in the monitoring of actions and their outcomes. It is a possibility that the prelimbic cortex sends information about actions and outcomes to the DMS, which uses it to control goal-directed instrumental learning and behavior; at the same time, it also sends such information to presently unknown brain structures that ultimately drive causal interventions. As such, the prelimbic cortex would be another area of interest.

Another relatively simple behavioral study that could be done to manipulate the action-outcome sensitivity on the part of rats that intervene on the Tone would be to alter the contiguity between the lever press and the Tone. That is, if the Tone were to be presented only after a brief delay following the lever press, the subjects would be less likely to attribute the Tone to their actions. In this case, we might expect less discounting of Food as such. If we performed an experiment that varied the magnitude of this delay, we might even observe a gradation in the degree of this discounting, thus demonstrating a continuous relationship between agency of action and confidence in making causal attributions.

Otherwise, perhaps a study involving the immediate early gene c-Fos would help shed light on which particular brain areas are involved in not only causal interventions, but causal inferences as well: When the Tone is presented (i.e., merely observed), which structures of the brain are involved in making the causal diagnosis from the Tone back to the representation of the Light, and subsequently to Food? Similarly, for subjects that intervene on the lever to produce the Tone, which structures (which presumably include the sensorimotor and corticostriatal areas) are involved in the decision to discount Food? Due to its role in inferential reasoning, we might expect to see the expression of c-Fos in the hippocampus (see Zeithamova, Schlichting, & Preston, 2012). One crucial aspect of inferential reasoning is the ability to form relationships between associations that were not learned together. For instance, subjects would first be trained with a set of premise relationships (e.g., If presented with A and B, pick A; if presented with B and C, pick B; thus forming separate sets of ordered hierarchy of relationships such as A > B > C > D and W > X > Y > Z). During probe trials, subjects might be presented with B, and given a choice of D or Y. Such a task requires the subject to infer transitively that since B > C and C > D, the choice should be D, even though B and D have never been paired together during training

trials. Such a task requires the inference of individual associations at the point of processing. This relates to our paradigm in the sense that during training of the full causal model (see Figure 1.3), the Light → Tone and Light → Food, and Noise → Food associations were each learned separately. Thus, the integrated causal model had to be inferred. Previous studies have shown that rats with hippocampal lesions (Bunsey & Eichenbaum, 1996) or disconnection of the hippocampus from its cortical or subcortical pathways (Dusek & Eichenbaum, 1997) are impaired in probe trials of associative inference tasks (under an odor-discrimination paradigm), while the acquisition of the individual associations were unimpaired during training, thus ruling out memory deficits as an explanation (see also DeVito, Kanter, & Eichenbaum, 2010). If the hippocampus plays an important role in inferential reasoning, then we might also expect it to play a role in causal inferences.

Another informative avenue of research would be to use electrophysiological methods to record activity of neurons in the ventral tegmental area (VTA) and the lateral habenula, so as to gauge the degree to which rats expect (and will thus search for) Food during presentations of the Tone (Common-cause) or the Noise (Direct-cause; Figure 1.3). Evidence has shown that neurons in the VTA as well as the adjacent substantia nigra pars compacta (SNc) produce a short-latency (70 – 100 ms), short-duration (< 200 ms) burst of dopaminergic (DA) activity, known as the phasic dopamine response (Schultz, 1998), about 70 ms after the occurrence of an unexpected event, such as the first time a Tone CS is presented or an unexpected US presentation. If the Tone is played repeatedly in the absence of a behaviorally rewarding stimulus, the DA neurons habituate quickly, and the phasic dopamine responses cease (Ljungberg, Apicella, & Schultz, 1992). However, if the CS and US is paired repeatedly, a phasic dopamine response will develop for the CS, but phasic dopamine response to the US will gradually diminish (Pan, Schmidt,

Wickens, & Hyland, 2005). If the CS is presented in the absence of the US, the phasic dopamine response will be observed for the CS, but a notable depression in the spontaneous activity of DA neurons will be observed about 70 – 100 ms after the time when the US presentation is expected (Schultz, 1998; Schultz, Dayan, & Montague, 1997). More recently, it has been reported that the lateral habenula behaves antagonistically with the DA neurons in the VTA: Whenever a phasic dopamine response is observed in the VTA (e.g., to a presentation of a CS), a depression in activity was observed in the lateral habenula; and whenever a depression is also observed in the VTA (e.g., to an omission of an expected US), an accompanying phasic burst of activity was observed in the lateral habenula. Through electrical stimulation, it was found that lateral habenula neurons had inhibitory projections to the SNc (i.e., activity in the lateral habenula caused an inhibition of DA neurons of the SNc; however, the depression of activity in the lateral habenula do not appear to be causally related to phasic dopamine responses in the SNc; Matsumoto & Hikisaka, 2007).

Thus, in the context of rats that have been trained on the causal model illustrated in Figure 1.3, electrophysiological recordings of these areas would provide direct and valuable clues as to the expectancies of Food, depending on how Tone was encountered. Namely, rats that merely observe the Tone or the Noise should expect Food, and since testing would be done in extinction, we would expect a dip in DA activity in the VTA and a phasic burst of activity in the lateral habenula some time during the presentation of the Tone. Rats that intervened to produce Noise should also show the same activity. However, rats that intervened to produce the Tone should not expect food, and we should expect to see no change in activity (relative to baseline) in either the VTA or the lateral habenula during the presentation of the Tone.

Ultimately, in anthropomorphic terms, the main difference between rats that intervene and those that observe the Tone at test can be expressed as "Did I cause that? Or was it something external?" The discovery of the ventral tegmental phasic DA responses has resulted in the reward prediction error hypothesis, which posits that these DA responses signal reward prediction errors, which are used by reinforcement learning mechanisms to drive future behavior (e.g., Montague, Dayan, & Sejnowski, 1996; Schultz, 1998; Schultz & Dickinson, 2000; Schultz, 2002; Montague, Hyman, & Cohen, 2004; Schultz, 2006). It is a retrospective point of view, in the sense that this learning is used to make adjustments of relative probabilities of choosing pre-existing actions in order to maximize future rewards. However, an alternate view has been put forth, which suggests that these signals are used instead to reinforce the repetition of whatever actions or movements that happened to immediately precede an unexpected event as a way of discovering novel actions that are rewarding (Redgrave & Gurney, 2006). A destination of these phasic DA bursts are the striatum, which also receive inputs from sensory systems via the thalamus (e.g., McHaffie, Stanford, Stein, Coizet, & Redgrave, 2005), information about the context (e.g., Apicella, Legallet, & Trouche, 1997; Samejima, Ueda, Doya, & Kimura, 2005), as well as copies of efferent motor commands (e.g., Reiner, Jiao, DelMar, Laverghetta, & Lei, 2003). According to this view, phasic DA responses in the VTA occur whenever an unexpected event occurs, and these action potentials arrive at the striatum at approximately the same time as the aforementioned sensory, context, and efferent motor inputs. Through Hebbian-like learning, this helps the animal identify the exact situation (both externally and internally) under which this event occurs; and if the outcome is rewarding, this action is more likely to be repeated under similar conditions (i.e., instrumental learning; note however that the authors made no predictions about whether or not this would be the case if the action was habitual). In contrast, when an

event that is external to the organism occurs, the same convergence of inputs happen upon the striatum, except for the motor information. In this case, learning about the event still occurs, but the organism will not erroneously learn that its actions were responsible for that event. Thus, if this theory were true, only the efferent motor input to the striatum distinguishes if the action is caused by the actor or otherwise. One implication of this theory is that the sense of action agency could theoretically be removed if the efferent motor input to the striatum were to be pharmacologically blocked, and the animal will no longer attribute the outcome to its instrumental actions. Conversely, and perhaps more interestingly, if an auditory stimulus were to be presented, and these efferent motor inputs to the striatum were stimulated at the same time, would the organism get a false sense that its immediately prior actions resulted in the stimulus, and attribute whatever action it was doing at that moment as a cause of that auditory stimulus?

Future investigation of this ventral midbrain DA system as it pertains to causal interventions would also provide valuable insight about abnormal causal attributions in individuals with abnormal DA systems. For instance, patients suffering from schizophrenia show an abnormal sense of agency, wherein they are more likely to feel responsible for actions that they did not actually make (e.g., Daprati, Franck, Georgieff, Proust, Pacherie, Dalery, & Jeannerod, 1997; this appears to be more rampant in patients with hallucinations). It is thus plausible that this symptom exists as a result of abnormally high DA input to the basal ganglia. Further understanding of this system could possibly lead to treatment of not just this symptom, but other disorders of the basal ganglia as well.

5.3: Conclusion

Needless to say, much work remains to be done in this contentious area of research. While no one has been bold (or perhaps foolish) enough to argue that rats are capable of

reasoning causally on a human level, much debate has been waged over whether or not causal reasoning remains as a cognitive ability that separates humans from non-human animals. From an evolutionary standpoint however, it would make the most sense for at least *some* forms of rudimentary faculties that contribute to what we consider "true" causal reasoning to have evolved at some point before the genus *Homo*, and it is a possibility that these very rudiments are what have been observed in the animal studies that are just now emerging. Hopefully, it will not be too long before more light is shed on how causal structures are learned, integrated, and used in casual judgments to produce meaningful and adaptive behavior in the world.

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