CONTROLLED AND AUTOMATIC PROCESSES IN PAVLOVIAN-INSTRUMENTAL TRANSFER

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Author's declaration

At no time during the registration for the degree of Doctor of Philosophy has the author been registered for any other University award. Work submitted for this research degree at Plymouth University has not formed part of any other degree either at Plymouth University or at another establishment.

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Abstract

Controlled and automatic processes in Pavlovian-instrumental transfer

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The current research aimed to further current knowledge on the psychological processes that underpin human outcome-selective Pavlovian-instrumental transfer (PIT) effects. PIT reflects the capacity of a Pavlovian stimulus to selectively potentiate an instrumental response that predicts a common rewarding outcome. PIT effects are often suggested to reflect a relatively automatic S-O-R mechanism, where the stimulus activates the sensory properties of the outcome, which then automatically triggers associated instrumental responses. The current research tested this S-O-R account of PIT against a propositional expected utility theory, which suggests that PIT effects reflect verbalizable inferences about the probability and value of each outcome. Chapter 1 reviews the relevant literature. Chapters 2-4 then report 11 experiments that aimed to set the S-O-R and propositional theories against one another. In Chapter 2, two experiments demonstrated that PIT is sensitive to a reversal instruction (Experiment 2), but is robust against a time pressure (Experiment 1) and concurrent load (Experiment 2) manipulation. Chapter 3 details the development of a novel outcome devaluation procedure, and reports four experiments that examined the effect of both outcome devaluation and verbal instructions on PIT. These experiments demonstrated that a typical PIT procedure produces PIT effects that are insensitive to a very strong devaluation manipulation. Furthermore, PIT effects were observed for a devalued outcome even when an S-O-R mechanism was unlikely to control behaviour. Chapter 4 reports five experiments that show that PIT is highly sensitive to outcome devaluation

when multiple outcomes and responses are cued on every transfer test trial. Chapter 5 therefore concludes that, on balance, the results provide converging support for the propositional expected utility theory of PIT.

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Chapter 1: Pavlovian-instrumental transfer

1.1 Introduction

Reward-related cues exert a profound influence on behaviour. Cues that predict food availability, for example, can motivate actions to obtain food rewards (Colwill & Rescorla, 1988; Estes, 1943; Hogarth, Dickinson, Wright, Kouvaraki, & Duka, 2007; Kruse, Overmier, Konz, & Rokke, 1983; Watson, Wiers, Hommel, & de Wit, 2014). Pavlovian-instrumental transfer (PIT) tasks are a very popular way to measure such *cue reactivity* in the laboratory. The research in this thesis aims to extend current knowledge of the psychological processes that underlie the cue reactivity that is seen in PIT tasks.

PIT reflects an interaction between Pavlovian and instrumental conditioning processes. The thesis therefore begins with an introduction to these two fundamental forms of associative learning. The various forms of PIT and the dominant psychological theories are then examined. A core aim of this thesis is to test whether PIT effects are best explained by an associative link mechanism, a controlled reasoning process, or both. To this end, Chapters 2-4 report eleven experiments that test the dominant associative link account of PIT (S-O-R theory) against a recently developed propositional account (using expected utility [EU] theory). These experiments use verbal instructions with time pressure and concurrent load manipulations (Chapter 2), and a novel outcome devaluation procedure (Chapters 3 and 4). The results are finally discussed in Chapter 5. To pre-empt the results, Chapter 5 concludes that, on balance, the data provide converging evidence to support the propositional EU account of PIT.

1.2 Pavlovian and instrumental conditioning

PIT effects reflect the ability of a Pavlovian stimulus to potentiate an instrumental response. To understand PIT effects, it is therefore necessary to provide an introduction to Pavlovian and instrumental conditioning. Figure 1.1, which was inspired by Hogarth, Balleine, Corbit, and Killcross (2013), depicts the learning structures that are commonly thought to mediate Pavlovian and instrumental conditioning.

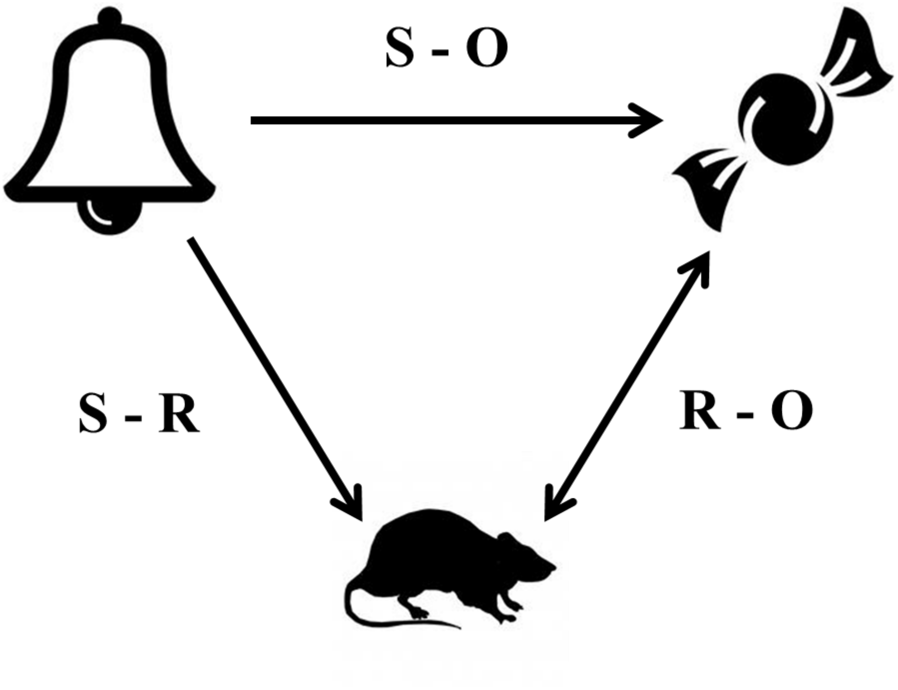

Figure 1.1. Theoretical learning structures mediating Pavlovian and instrumental conditioning. S-O refers to a Pavlovian stimulus-outcome association. R-O refers to a goal-directed, bidirectional response-outcome association. S-R refers to an automatic or habitual stimulus-response association.

Pavlovian conditioning reflects the learning of a relationship between a stimulus (S) and an outcome (O). Pavlovian relationships (or *contingencies*) are often denoted as *stimulus-outcome* (S-O) associations (see Figure 1.1). Pavlov (1927) provided the iconic demonstration of Pavlovian conditioning. When food delivery was repeatedly preceded by a ringing bell, Pavlov's dogs came to salivate to the sound of the bell. In associative learning terminology, the ringing bell became a *conditioned stimulus* (CS) that predicted food delivery – a biologically significant, *unconditioned stimulus* (US). The dogs then salivated – a *conditioned response* (CR) – upon hearing the bell.

Instrumental conditioning is another important form of associative learning (Grindley, 1932; Mackintosh & Dickinson, 1979; Miller & Konorski, 1969; Skinner, 1932; Thorndike, 1911). Consider a hungry rat that learns to press a lever to obtain a food pellet. Here, the food pellet is contingent on the rat pressing the lever, and so the response (R) has a causal role in producing the outcome (O). Contemporary dual-process theories propose that instrumental learning can be either habitual or goal-directed, with each controller dominating under different circumstances (Balleine & O'Doherty, 2010; de Wit & Dickinson, 2009; Dickinson, 1985, 2016; Hogarth & Chase, 2011). This dual-process account is reviewed briefly below because it has informed many of the key theories of PIT.

Dual-process accounts of instrumental learning propose that habitual responses are mediated by the stimulus-response (S-R) mechanism shown in Figure 1.1. According to this account, instrumental conditioning produces an association between the contextual stimuli that are present in the animal's environment and the instrumental response. When the instrumental response R is reinforced by a rewarding outcome O in the presence of a stimulus S, the S-R association is strengthened (Hull, 1943). Notably, this association does not incorporate a representation of the outcome O. Rather, the outcome is a catalyst that strengthens the *S-R* association (Balleine & O'Doherty, 2010). According to S-R theory, instrumental responses are *habitual* responses that are automatically triggered by contextual stimuli.

Goal-directed instrumental responses are thought to be mediated by the bidirectional R-O mechanism shown in Figure 1.1. Two criteria must be met for an instrumental response to be classified as goal-directed (de Wit & Dickinson, 2009;

Heyes & Dickinson, 1990). First, the behaviour must satisfy the *belief* criterion, which means that the response must reflect knowledge about the relationship between the response and the outcome. Second, the response must meet the *desire* criterion, which means that it must be sensitive to changes in outcome value. That is, the response should only be performed when the outcome is desired. There is continued debate about the precise structure of the mental representations that underlie goal-directed behaviour (de Wit & Dickinson, 2009; Dickinson, 1985, 1994; Rescorla, 1992b). For the sake of simplicity, however, it will suffice to say at this point that a goal-directed action is usually thought to reflect either a *response-outcome* (R-O) or an *outcome-response* (O-R) association. This debate is of direct relevance to the key theories of PIT and is therefore discussed more thoroughly below.

1.3 Pavlovian-instrumental transfer

This thesis concerns the psychological processes that mediate human PIT effects. Learning theorists have, in recent years, uncovered several effects that are all classed under the umbrella term of ‘PIT’ (Corbit & Balleine, 2005; Estes, 1943; Kruse et al., 1983; Lovibond, 1981; Walker, 1942). The current research concentrates on the *outcome-selective* form of PIT (described below), but a brief overview of the other types of PIT is also provided below for clarity.

1.3.1 Non-selective PIT

Estes (1943) provided one of the earliest demonstrations of PIT. He trained two groups of rats to first press a lever to obtain a food pellet reward. For an experimental group, the levers were then removed and a tone was repeatedly paired with a food pellet. During the subsequent transfer test, the levers were restored and lever-press responses in both groups were measured in extinction (without reinforcement). Crucially, the tone was presented twice during the transfer test for the experimental group, to assess the effect of the tone on lever-press responses.

A classic extinction curve was observed in both groups; instrumental lever-pressing was initially high and declined throughout the transfer test because of non-reinforcement. However, the tones also *increased* the rate of lever-press responding in the experimental group. Estes' (1943) results provided one of the earliest demonstrations of PIT, where a Pavlovian stimulus potentiated an instrumental response that was associated with a common rewarding outcome.

Subsequent experiments used *non-selective* designs (shown in Table 1.1) to extend Estes' (1943) work. Lovibond (1983), for example, trained rabbits to perform a head-raising instrumental response (R) to earn a sucrose rewarding outcome (O). In a separate session, one stimulus (S1) was paired with sucrose (S1 – O), while another stimulus was presented but not paired with sucrose (S2 – no O). During the transfer test, instrumental responding was assessed both at baseline and during the presence of each stimulus. Consistent with Estes' (1943) results, the sucrose-paired stimulus facilitated instrumental responding above baseline responding, but the unpaired stimulus did not. Lovibond's design built upon the original design of Estes, because it showed that *reward-predictive* stimuli preferentially increase instrumental responding. This ability of a Pavlovian stimulus to enhance instrumental responding for a single rewarding outcome is referred to as a *non-selective* PIT effect (Holmes, Marchand, & Coutureau, 2010). Lovibond's (1983) procedure has also been successfully translated for use in human experiments in recent years, where similar results have been observed (Bezzina, Lee, Lovibond, & Colagiuri, 2016; Colagiuri & Lovibond, 2015; Lovibond & Colagiuri, 2013; Lovibond, Satkunarajah, & Colagiuri, 2015; Talmi, Seymour, Dayan, & Dolan, 2008).

Table 1.1

Design of non-selective PIT experiments.

Instrumental training	**Pavlovian conditioning**	**Transfer test**
R-O	S1-O	S1: R?
	S2- no O	S2: R?

Note: R refers to an instrumental response, O refers to an appetitive outcome, and S1 and S2 refer to Pavlovian stimuli.

1.3.2 Outcome-selective PIT

The early PIT research demonstrated that Pavlovian reward-predictive stimuli can facilitate instrumental responses that predict rewarding outcomes. Subsequent research confirmed that Pavlovian cues preferentially enhance instrumental responses that predict the *same* outcome, as opposed to *any* rewarding outcome – an effect known as *outcome-selective* PIT (Colwill & Rescorla, 1988; Kruse et al., 1983). Outcome-selective PIT can be defined as the ability of a Pavlovian stimulus S to selectively potentiate an instrumental response R that is associated with a common rewarding outcome O. Table 1.2 shows a typical outcome-selective PIT design. First, rats or humans[1] learn to perform two instrumental responses (R1 and R2) to earn distinct rewarding outcomes (O1 and O2) to establish R1-O1 and R2-O2 associations. In a separate Pavlovian conditioning phase, two neutral stimuli (S1 and S2) are also paired with either outcome O1 or O2 (S1-O1, S2-O2). Hence, each outcome (e.g., O1) is associated with one Pavlovian stimulus (S1) and one instrumental response (R1). The Pavlovian stimuli and instrumental responses should not be *directly* associated with one another, however, because they were not presented together.

In the final transfer test, both instrumental responses are measured in extinction in the presence of each Pavlovian stimulus, relative to baseline ‘no stimulus’ periods.

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[1] PIT effects have also been observed in other species (see Holmes et al., 2010), but the vast majority of research has used either rats or human participants.

The classic result is that the Pavlovian stimuli selectively elevate the instrumental response that is associated with the same outcome. That is, stimulus S1 increases response R1 more than R2, and S2 increases R2 more than R1. This is referred to as an *outcome-selective* PIT effect, because each Pavlovian stimulus S preferentially potentiates the instrumental response R that is paired with a common outcome O.

Table 1.2

Outcome-selective PIT design and typical result.

Instrumental conditioning	**Pavlovian conditioning**	**Transfer test**
R1-O1	S1-O1	S1: R1 > R2
R2-O2	S2-O2	S2: R1 < R2

Note: R1 and R2 refer to instrumental responses. O1 and O2 refer to appetitive outcomes. S1 and S2 refer to Pavlovian stimuli. Instrumental responding in the presence of the Pavlovian stimuli during the transfer test is usually assessed relative to either a baseline ‘no-stimulus’ period or a neutral stimulus.

Outcome-selective PIT procedures were developed in rodents, but they are now also widely used in human experiments (e.g., Bray, Rangel, Shimojo, Balleine, & O’Doherty, 2008; Hogarth et al., 2007; Paredes-Olay, Abad, Gámez, & Rosas, 2002). There is, however, substantial variation in the procedures used to measure outcome-selective PIT effects in humans. For example, some researchers use the approach that is used in animal experiments, and train neutral stimuli to predict rewarding outcomes in a Pavlovian conditioning phase (e.g., Bray et al., 2008; Hogarth et al., 2007; Watson et al., 2014). In contrast, others use pictorial stimuli that have pre-established Pavlovian associations with rewarding outcomes (Hogarth, 2012; Hogarth & Chase, 2011; Hogarth, Maynard, & Munafò, 2015). The response measurement technique also varies. Some researchers allow unconstrained responding during the instrumental training and transfer test phases (e.g., Quail, Morris, & Balleine, 2016; Watson et al., 2014). Others use a forced-choice procedure in which response choice is assessed on every discrete trial (e.g., Bray et al., 2008; Hogarth & Chase, 2011; Martinovic et al., 2014). Finally, some researchers use real rewards during the training phases (Watson et al., 2014), while others use symbolic points or pictures (Alarcón & Bonardi, 2016; Hogarth, 2012; Hogarth & Chase, 2011; Quail et al., 2016). Each procedure has advantages and disadvantages, but they essentially give rise to a similar result: reward-associated stimuli tend to facilitate instrumental responses that are associated with a common rewarding outcome.

1.3.3 General PIT

Corbit and Balleine (2005) provided perhaps the clearest demonstration of a PIT effect that is separable from outcome-selective PIT; they called this *general* PIT[2]. During instrumental training, rats were trained to perform two instrumental responses to earn distinct rewarding outcomes (R1-O1, R2-O2). Two stimuli were subsequently paired with each outcome (S1-O1, S2-O2), and a third stimulus was paired with a novel third outcome (S3-O3). In the critical transfer test, R1 and R2 responses were assessed in the presence of each Pavlovian stimulus, relative to a baseline ‘no-stimulus’ period. Rats also received either basolateral amygdala (BLA), amygdala central nucleus (CN), or sham lesions at the start of the experiment to explore the neural substrate of PIT.

The sham-lesioned rats demonstrated a clear outcome-selective PIT effect during the transfer test; S1 and S2 selectively increased R1 and R2 responses respectively, relative to the baseline response rate. Furthermore, a *general* PIT effect was also observed, where Pavlovian stimulus S3 increased R1 and R2 responses indiscriminately compared to the baseline response rate. The data therefore demonstrate a clear behavioural distinction between outcome-selective and general PIT. Similar

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[2] Non-selective PIT designs measure instrumental responding for a single rewarding outcome. They therefore do not distinguish between outcome-selective and general PIT (Cartoni, Balleine, & Baldassarre, 2016).

results have also recently been obtained in the human literature (Nadler, Delgado, & Delamater, 2011; Prévost, Liljeholm, Tyszka, & O'Doherty, 2012; Quail et al., 2016; Watson et al., 2014). Moreover, Corbit and Balleine (2005) reported a double dissociation at the neural level; BLA lesions abolished outcome-selective but not general PIT, while CN lesions abolished general but not outcome-selective PIT. Furthermore, lesions or inactivation of the nucleus accumbens (NAC) shell abolish outcome-selective but not general PIT, while lesions or inactivation of the NAC core abolish general but not outcome-selective PIT (Corbit & Balleine, 2011). Together, these data provide compelling evidence for two distinct ways in which Pavlovian cues influence instrumental behaviour.

1.3.4 Non-appetitive forms of PIT

The overview presented above might give the impression that PIT has only been studied in the appetitive domain (that is, by using biologically relevant outcomes such as food and drink). Whilst most PIT research has indeed used appetitive outcomes, non-appetitive PIT effects have also been obtained. In humans, for instance, PIT effects have been observed for money, a rewarding but non-biologically relevant outcome (Allman, DeLeon, Cataldo, Holland, & Johnson, 2010; Eder & Dignath, 2016a). *Aversive* (sometimes called *avoidance*) PIT effects have also been obtained. Aversive PIT effects reflect the capacity of a Pavlovian stimulus that predicts a negative outcome to increase instrumental responses that have been trained to *cancel* or *avoid* that negative outcome. Thus, aversive PIT effects are fundamentally different from appetitive PIT effects, which demonstrate a tendency for reward-predictive stimuli to increase instrumental responses to *obtain* the same outcome. There is a growing body of literature exploring the psychological and neural basis of aversive PIT effects, with regard to non-selective, outcome-selective and general PIT (Campese, McCue, Lázaro-Muñoz, LeDoux, & Cain, 2013; Lewis, Niznikiewicz, Delamater, & Delgado, 2013; Trick, Hogarth, & Duka, 2011). However, the current research focuses largely on appetitive PIT effects, so a comprehensive review of this literature is not provided here.

1.4 PIT as a model of cue reactivity

The literature discussed so far demonstrates that Pavlovian stimuli can influence instrumental responding in several distinct and important ways. The current research focuses primarily on the outcome-selective form of PIT, and it is therefore abbreviated to ‘PIT’ hereafter unless otherwise stated. PIT effects are widely researched for three primary reasons. First, PIT effects are theoretically interesting because they demonstrate an interaction between Pavlovian and instrumental processes. Second, PIT effects are extremely robust effects that persist even following experimental manipulations that aim to undermine their integrity (e.g., Delamater, 1996; Rescorla, 1994b). These manipulations are discussed more thoroughly below. Finally, PIT effects are of considerable applied interest. PIT effects have been implicated in a wide range of dysfunctional behaviours, including relapse to drug addiction (Everitt, Dickinson, & Robbins, 2001; Hogarth et al., 2013), alcohol dependency (Garbusow et al., 2015) and overeating behaviours (Watson et al., 2014; Watson, Wiers, Hommel, Ridderinkhof, & de Wit, 2016). In summary, PIT effects are studied because they are of both basic and applied interest. The section below now details some of the dominant psychological theories of PIT.

1.5 Theories of PIT

Psychological theories of PIT generally posit one of two fundamentally different mechanisms: an associative link mechanism, or a higher-order propositional process. The research in this thesis seeks evidence for each of these processes. The distinction between associative links and propositions also accords with a long-standing debate in the human associative learning literature more generally (see Mitchell, De Houwer, & Lovibond, 2009 for a review). It is beyond the scope of this thesis to provide a comprehensive review of this debate, but a summary is provided below.

1.5.1 The propositional versus dual-process debate

Human associative learning is often suggested to reflect the formation of associative links between mental representations (e.g., de Wit & Dickinson, 2009; Dickinson, 2012; McLaren et al., 2014; Sternberg & McClelland, 2012). Repeatedly pairing a stimulus S with an outcome O during Pavlovian conditioning, for example, is suggested to produce an associative link between their mental representations. Importantly, associative links allow mental representations to transmit excitation or inhibition to other mental representations (Dickinson, 2012). Once an associative link has formed, activation can then automatically pass from one representation (e.g. the stimulus) to another (the outcome). The term "automatic" is used here in the sense that the activation is said to be fast, unintentional and non-strategic (Moors & De Houwer, 2006). The ability of the stimulus S to activate the outcome O is not therefore suggested to be affected by deliberate decision processes. This link-based mechanism has been proposed to account for many associative learning phenomena.

The "link-based" account of learning can be contrasted to the *propositional* account, which assumes that higher order cognitive processes are necessary for human associative learning (De Houwer, 2009, 2014; De Houwer, Vandorpe, & Beckers, 2005; Mitchell et al., 2009; Mitchell, Livesey, & Lovibond, 2007). Associative learning, therefore, is suggested to reflect an effortful, cognitively-demanding process that produces beliefs about the world in the form of propositions. It should be recognised that few proponents of the link-based process deny that humans can also learn through propositional reasoning. Rather, they advocate a *dual-process* model of associative learning, suggesting that both propositions and associative links produce various associative learning phenomena (Heyes, 2012; McLaren et al., 2014). The key distinction here is that the propositional approach omits any reference to the link-formation mechanism. According to the propositional account, human associative learning can only occur through the formation of conscious propositions that arise from inferential reasoning processes.

The current research examines whether human PIT effects are best explained by an associative link mechanism, a higher-order propositional process, or a combination of both (a dual-process model). The dominant account of PIT, S-O-R theory, advocates an associative link mechanism (Alarcón & Bonardi, 2016; Balleine & Ostlund, 2007; de Wit & Dickinson, 2009, 2015; Watson et al., 2014). It therefore assumes that human PIT effects are, at least sometimes, mediated by automatic associative links. Alternative theories have also been proposed, including a hierarchical S:R-O model (e.g., Colwill & Rescorla, 1990b) and a propositional account of PIT (Hogarth et al., 2014; Seabrooke, Hogarth, & Mitchell, 2016). The sections below provide an overview of the evidence for each of these theories.

1.5.2 S-O-R theory

S-O-R theory is a widely supported model of PIT (Alarcón & Bonardi, 2016; Balleine & O’Doherty, 2010; Balleine & Ostlund, 2007; de Wit & Dickinson, 2015; Hogarth, 2012; Hogarth & Chase, 2011; Holland, 2004; Watson et al., 2014, 2016). It is an associative link-based account that derives from two-process models of instrumental learning (Rescorla & Solomon, 1967; Trapold & Overmier, 1972). S-O-R theory proposes that, in a typical PIT task, Pavlovian conditioning fosters associative links between the mental representations of each stimulus S and outcome O (S-O). Hence, presenting a Pavlovian stimulus during the transfer test should automatically activate the associated outcome representation. Instrumental training is similarly assumed to produce associative links between the mental representations of each instrumental response R and outcome O, but there is continued debate about the formation and structure of this association. Some researchers suggest that a bidirectional response-outcome (R-O/O-R) association forms because of the direct contingency between the instrumental response and the outcome (Asratyan, 1974; Pavlov, 1932). Others propose that Pavlovian associations form between the outcomes and contextual stimuli that are present during instrumental training. The contextual stimuli then activate the outcome representation while the instrumental response is performed. Consequently, the contiguous activation of the outcome and response representation is suggested to produce a backwards outcome-response (O-R) association (Trapold & Overmier, 1972). For the sake of simplicity these accounts will be collectively referred to as *outcome-response (O-R) theory*, because the learning processes are assumed to result in the same associative structure (de Wit & Dickinson, 2009; de Wit, Niry, Wariyar, Aitken, & Dickinson, 2007). In each case, O-R associative links are suggested to control instrumental behaviour.

S-O-R theory proposes that presenting a Pavlovian stimulus S during the PIT transfer test activates the associated outcome O via the S-O associative link. The outcome representation then activates and triggers the associated instrumental response R through the O-R associative link. Hence, PIT effects are suggested to operate via a stimulus-outcome-response (*S-O-R*) associative chain. S-O-R theory successfully predicts the selectivity of the PIT effect, because the stimulus activates only the outcome with which it was paired during Pavlovian conditioning. The outcome representation then activates only the instrumental response that it was paired with during the instrumental training phase (e.g., S1-O1-R1).

Notably, popular ideomotor theories of action control make very similar predictions to (S)-O-R theory with regard to the emergence of voluntary action control (e.g., Dutzi & Hommel, 2009; Elsner & Hommel, 2001, 2004; Hommel, 2013). Elsner and Hommel (2001), for example, suggested that actions are initially carried out randomly, and their sensory effects are registered. The mental representations underlying the motor responses then become associated with the sensory effect representations through a process of Hebbian learning (Hommel, 2015). In associative learning terminology, associative links form between the mental representations of responses and outcomes, providing the events are presented in a contingent and contiguous manner (Elsner & Hommel, 2004). Importantly, these response-outcome associations are assumed to operate bidirectionally, which allows activation of outcome representations to automatically activate associated instrumental responses (Elsner & Hommel, 2001; Kunde, 2004). The R-O associations can then be exploited to engage in goal-directed behaviour – thinking about goals (outcomes) should facilitate responses that have produced them in the past. Ideomotor theory therefore makes a similar assumption to S-O-R theory; goal-directed behaviours are suggested to reflect associative links between the mental representations of outcomes and associated instrumental responses. Although these theories evolved separately, there has been a concerted effort to unify them in recent years (de Wit & Dickinson, 2009, 2015; Eder, Rothermund, De Houwer, & Hommel, 2014; Watson, van Steenbergen, de Wit, Wiers, & Hommel, 2015; Wolfensteller & Ruge, 2012). The S-O-R model of PIT is a testament to this integration. The section below examines the key lines of support for S-O-R theory.

1.5.2.1 S-O-R theory as a model of instrumental behaviour

Trapold and Overmier (1972) provided some of the earliest support for S-O-R theory. The experiment, which was conducted in rats, is outlined in Table 1.3 (adapted from Balleine and Ostlund, 2007). First, two neutral stimuli were trained to predict different rewarding outcomes (S1-O1, S2-O2). These stimuli then served as *discriminative* stimuli that signalled that one response would be reinforced with either O1 or O2, and the other response would not be reinforced. Half of the rats received a discriminative contingency that was congruent with the previously established Pavlovian relations. For example, if S1 and S2 were paired with O1 and O2 respectively during Pavlovian conditioning, then S1 might now signal that R1 would produce O1, and R2 would not be reinforced (S2 would signal the opposite – R2 would produce O2 and R1 would produce nothing). The other rats received an incongruent discrimination; the stimuli predicted that one response would produce the *opposite* outcome to what it had previously signalled. Crucially, Trapold and Overmier (1972) found that the congruent discrimination was acquired more rapidly than the incongruent discrimination. This is regarded as evidence for S-O-R theory, because it suggests that the stimuli activated the outcomes that they were associated with during Pavlovian conditioning, which hindered acquisition of the new discriminative contingencies in the incongruent condition.

Table 1.3

Design of Trapold and Overmier (1972).

Group	**Training**	**Test**
Congruent	S1-O1, S2-O2	S1: R1-O1, R2- ; S2: R1-, R2-O2
Incongruent	S1-O1, S2-O2	S1: R1-O2, R2- ; S2: R1-, R2-O1

Note: S1 and S2 represent stimuli, O1 and O2 represent outcomes, and R1 and R2 represent instrumental responses.

More recently, two-stage priming tasks have provided support for ideomotor theory, and by extension, S-O-R theory (e.g., Elsner & Hommel, 2001; Flach, Osman, Dickinson, & Heyes, 2006; Watson et al., 2015). In a typical two-stage priming task, participants first learn to perform two different responses, which are each followed by either a high or a low tone (R1-O1, R2-O2). These tones are then presented as imperative stimuli, and participants are required to select either R1 or R2 as quickly and as accurately as possible. Half of the participants are allocated to an action-consistent group, where the mapping of response to outcome is congruent in the training and test phases; presentation of O1 on test signals that participants should execute response R1, and O2 signals R2. The mappings are reversed for a second, action-inconsistent group (O1-R2 and O2-R1). The classic result is that the action-inconsistent group respond more slowly than the action-consistent group. Thus, in the action-inconsistent group, the automatic activation of R1 by O1 (due to the O1-R1 binding) is suggested to interfere with the execution of the instructed R2 in response to O1. This lends credence to S-O-R theory, in which the anticipation of outcomes is suggested to automatically trigger associated instrumental responses.

1.5.2.2 S-O-R theory as a model of PIT

The results discussed above provide support for S-O-R theory as a general model of instrumental behaviour. There is, however, reason to believe that it may not provide a full account of PIT specifically. The primary evidence for this comes from the *outcome devaluation* procedure, which is the diagnostic test for determining whether an instrumental response is goal-directed or habitual (de Wit & Dickinson, 2009; Dickinson, 1985). Outcome devaluation procedures typically consist of three phases. First, an instrumental response R is trained to predict a rewarding outcome O. The value of the outcome is then reduced. In rodents, outcome devaluation is typically achieved by repeatedly pairing the outcome with a toxin (e.g., Adams, 1982; Adams & Dickinson, 1981), or by allowing *ad libitum* consumption to induce satiety (e.g., Colwill & Rescorla, 1985b). Human devaluation procedures also use satiation (Tricomi, Balleine, & O'Doherty, 2009; Valentin, Dickinson, & O'Doherty, 2007) and aversion techniques (typically by making the outcome taste unpleasant; e.g., Eder and Dignath, 2016b; Rose, Brown, Field, & Hogarth, 2013). Participants are also sometimes given health warnings (Hogarth & Chase, 2011) or instructions to devalue the outcomes (Allman et al., 2010; de Wit et al., 2007; de Wit, Ridderinkhof, Fletcher, & Dickinson, 2013; Eder & Dignath, 2016a). Reduced responding for the devalued outcome is regarded as evidence for goal-directed control, because the response satisfies both the belief and desire criteria for goal-directed action. Continued responding for the devalued outcome, by contrast, is considered evidence for automatic or habitual control (de Wit & Dickinson, 2009; Heyes & Dickinson, 1990).

One of the most interesting and counterintuitive aspects of PIT is that it is often insensitive to outcome devaluation manipulations (Colwill & Rescorla, 1990a; Corbit, Janak, & Balleine, 2007; Hogarth, 2012; Hogarth & Chase, 2011; Holland, 2004;

Rescorla, 1994b; Watson et al., 2014). Hogarth (2012), for example, trained smokers to perform one instrumental response to earn tobacco points and another instrumental response to earn chocolate points (R1-O1, R2-O2). Either the tobacco or chocolate (O1 or O2) was then devalued by having participants ingest nicotine replacement therapy nasal spray, or consume chocolate until it was no longer desirable. Response choice (R1 versus R2) was then tested in the presence of pictorial stimuli that were associated with the tobacco and chocolate outcomes, and a neutral control stimulus. Overall response choice was biased towards the still-valued outcome during the transfer test, indicating goal-directed control. Paradoxically, a PIT effect was still observed; the tobacco and chocolate cues elevated responding for the outcome with which they had previously been paired, and this elevation was similar regardless of whether the outcome had been devalued. Thus, the PIT effect was said to be *insensitive to outcome devaluation*.

Rodent PIT experiments have consistently observed insensitivity to devaluation (Colwill & Rescorla, 1990a; Corbit et al., 2007; Holland, 2004; Rescorla, 1994b). Hogarth's (2012) data are also supported by other human experiments showing that PIT effects are robust against health warnings (Hogarth & Chase, 2011) and food satiation (Watson et al., 2014). It should be noted that there are also several recent reports of *sensitivity* to devaluation in the human literature (Allman et al., 2010; Eder & Dignath, 2016a, 2016b). These latter studies reported that taste aversion (Eder & Dignath, 2016b) and instructed devaluation (Allman et al., 2010; Eder & Dignath, 2016a) procedures reduced PIT. Thus, the human literature is mixed. It seems likely that procedural differences underlie these inconsistent results. These differences are discussed more thoroughly in Chapter 3, where the effect of outcome devaluation on PIT is examined further.

Aside from the recent demonstrations of sensitivity to devaluation in the human PIT literature, it is fair to say that PIT is *often* argued to be insensitive to devaluation (e.g., Balleine & O'Doherty, 2010; de Wit & Dickinson, 2009; Hogarth et al., 2013). As noted above, insensitivity to devaluation is the canonical assay for habitual control (de Wit & Dickinson, 2009; Dickinson, 1985). Note that PIT effects do not appear to reflect a habitual response that is mediated by a direct stimulus-response (S-R) association (Watson et al., 2014; although see Cohen-Hatton, Haddon, George, & Honey, 2013 for an alternative interpretation). PIT experiments explicitly avoid presenting and reinforcing the Pavlovian and instrumental responses together, precisely to prevent a direct stimulus-response association forming. Instead, S-O-R theory proposes that PIT effects are mediated by the outcome representation, but they are *automatic* because they are not modulated by changes in outcome value (and hence do not meet the desire criterion for goal-directed action). More specifically, the stimulus is suggested to activate the *identity* (through its sensory properties) of the associated outcome, but not its value (Hogarth & Chase, 2011; Holland, 2004; Martinovic et al., 2014; Rescorla, 1994b). In this way, S-O-R theory successfully reconciles the selectivity of the PIT effect with its apparent insensitivity to devaluation. The sections below outline the other key advantages of S-O-R theory.

1.5.2.3 Compulsive cue reactivity

The amended S-O-R model, in which the stimulus activates only the identity of the outcome representation, has become the dominant model of PIT (Alarcón & Bonardi, 2016; Balleine & O'Doherty, 2010; Balleine & Ostlund, 2007; de Wit & Dickinson, 2015; Hogarth, 2012; Hogarth et al., 2013; Hogarth & Chase, 2011; Holland, 2004; Watson et al., 2014, 2016). A key advantage of S-O-R theory is that, by virtue of its ability to explain PIT's insensitivity to devaluation, it provides a straightforward mechanism by which behaviour might become dysfunctional. S-O-R theory is a link-based model, and therefore assumes that PIT effects can (at least sometimes) be automatic or outside of intentional control. This automaticity closely emulates the compulsive and pervasive nature of pathological reward-seeking behaviours, including drug addiction (Hogarth et al., 2013) and compulsive overeating (Colagiuri & Lovibond, 2015; Watson et al., 2014). Thus, S-O-R theory is favoured because it provides a straightforward and intuitive explanation of the destructive nature of pathological cue reactivity.

1.5.2.4 Irrationality

The insensitivity of PIT to outcome devaluation provides good evidence for the S-O-R account of PIT. An even stronger argument has also been put forward: PIT effects are said to be exclusively predicted by S-O-R theory (de Wit & Dickinson, 2015). The argument here is that the Pavlovian stimuli already predict the outcomes - the outcomes are not (usually) contingent on instrumental responses during Pavlovian conditioning. In the transfer test, the Pavlovian stimuli should (based on their predictive history) therefore continue to produce the outcomes, even without an instrumental response. PIT effects, where Pavlovian stimuli seemingly arbitrarily potentiate responding for the common outcome, are therefore deemed *irrational*. The implication is that PIT effects must then be mediated by an automatic associative link mechanism rather than a controlled propositional process (de Wit & Dickinson, 2015).

The “irrationality” argument has featured repeatedly in the dual-process versus propositional debate of human associative learning (e.g., Mitchell et al., 2009; Shanks, 2007). In truth, irrational effects are unlikely to provide conclusive evidence for a link-formation mechanism (De Houwer, 2014). Many associative learning phenomena seem irrational, but this does not preclude the role of (albeit sometimes suboptimal) inferential reasoning processes. Irrational effects can, at least in principle, be reconciled with the propositional approach by recognising that controlled reasoning processes are not always perfect (Mitchell et al., 2009).

S-O-R theory is, nevertheless, a very successful theory of PIT. Other theories have also been proposed, most notably including a hierarchical (S: R-O) theory that has provided a long-standing challenge to S-O-R theory (Balleine & Ostlund, 2007; Cartoni, Moretta, Puglisi-Allegra, Cabib, & Baldassarre, 2015; Colwill & Rescorla, 1990b; de Wit & Dickinson, 2009; Hogarth et al., 2014; Rescorla, 1991). The current research primarily tests S-O-R theory against a recently developed propositional theory of PIT that is discussed below. However, there is compelling evidence for a hierarchical mechanism in PIT, and much of this evidence inspired the propositional theory that is tested in this thesis. Indeed, although the propositional approach makes some additional assumptions, the hierarchical and propositional accounts are largely complementary approaches. The hierarchical S: R-O account is therefore outlined briefly below.

1.5.3 Hierarchical S: R-O theory

Similar to S-O-R theory, hierarchical S: R-O theory proposes that Pavlovian conditioning produces stimulus-outcome (S-O) associations. Importantly, instrumental training is suggested to establish *forward* response-outcome (R-O) associations. In the transfer test, the stimulus is then argued to “set the occasion” for the associated instrumental relationship. More informally, the stimulus S is suggested to increase the perceived probability that the associated response R will be reinforced. The section below highlights some key evidence for hierarchical S: R-O theory.

1.5.3.1 Biconditional effects

Rescorla's (1990) biconditional design (summarised in Table 1.4) provided some of the earliest support for hierarchical S: R-O theory. Two discriminative stimuli (Sd1 and Sd2) were initially trained to signal opposite response-outcome (R-O) relations. Sd1 signalled that R1 and R2 responses would produce outcomes O1 and O2, respectively. Sd2, by contrast, signalled that R1 and R2 would produce O2 and O1, respectively. A further two auditory stimuli (S1 and S2) were also trained to predict these outcomes. S1 signalled that both responses would produce O1, and S2 signalled that both responses would produce O2 (see Table 1.4). At the end of training, each instrumental response signalled both outcomes equally, but in different contexts. Sd1 and Sd2 were then presented in compound with either stimulus S1 or S2, and response choice was tested in extinction. S-O-R theory does not predict any response bias under these circumstances, because the outcomes are equally associated with each response. The stimulus compounds should therefore produce response conflict, and no bias should be observed. As can be seen in Table 1.4, however, a clear bias was observed. In compound with Sd1, S1 and S2 increased R1 and R2 respectively. The pattern was reversed when S1 and S2 were presented with Sd2. Thus, the elements of each stimulus compound combined to selectively signal the instrumental response that was most likely to be reinforced. Such a result depends on hierarchical knowledge of the discriminative stimuli in signalling particular response-outcome (R-O) relations.

Table 1.4

Design and results of Rescorla (1990, Experiment 4).

Training	**Summation test**
Sd1: R1-O1, R2-O2	Sd1 + S1: R1 > R2
Sd2: R1-O2, R2-O1	Sd1 + S2: R1 < R2
S1: R1-O1, R2-O1	Sd2 + S1: R1 < R2
S2: R1-O2, R2-O2	Sd2 + S2: R1 > R2

Note: Sd1 and Sd2 refer to visual discriminative stimuli. S1 and S2 refer to auditory Pavlovian stimuli. R1 and R2 refer to instrumental responses and O1 and O2 refer to appetitive outcomes.

Rescorla's (1990) data are clearly more consistent with hierarchical S: R-O theory than S-O-R theory, because the effect cannot be readily explained by appealing to binary S-O and R-O/O-R associations. It is of course possible that both hierarchical S: R-O and binary S-O-R associations control instrumental behaviour under different circumstances, and that Rescorla’s procedure was not optimised to detect the latter process (see Rescorla 1994a for some evidence of this). It should also be noted that Rescorla (1990) did not use a true PIT procedure, because the discriminative and Pavlovian stimuli were paired with the instrumental responses during training. The result has, however, recently been replicated in a human PIT procedure (Hardy, Mitchell, Seabrooke, & Hogarth, in revision). Experiment 6 (Chapter 3) extends Hardy et al.’s results, so their experimental design and results are discussed more thoroughly in that chapter.

1.5.3.2 Reinforcement probability estimates

More recent evidence for the hierarchical account in humans comes from Cartoni et al. (2015), who established Pavlovian (S1-O1, S2-O2) and instrumental (R1-O1, R2-O2) contingencies in a typical PIT procedure. Instrumental responses had a 33% probability of reinforcement in one group, and a 100% probability of reinforcement in the other group. In the transfer test, instrumental responding was then assessed in the presence of Pavlovian stimuli S1 and S2, relative to a baseline period. The hierarchical account predicts a stronger PIT effect in the 33% probability group, because there is more opportunity for the Pavlovian stimulus to resolve uncertainty about outcome probability in this condition. A typical PIT effect was observed in each group, where the Pavlovian stimuli selectively increased the response that was paired with a common outcome. Crucially, the 33% reinforcement group showed a significantly larger PIT effect than the 100% reinforcement group. That is, the PIT effect was more robust when the instrumental contingencies were more uncertain during the training phase. Clearly, this finding accords very well with hierarchical S: R-O theory.

It should be noted that Cartoni et al. (2015) observed a PIT effect even in the 100% contingency condition. This suggests that there may also be other factors involved, besides the probability account put forward by the authors. Nevertheless, it is a striking demonstration of how PIT effects can be influenced by manipulating the schedule of reinforcement. Notably, S-O-R theory predicts the *opposite* result. More probable instrumental training would produce stronger O-R links, which should allow the Pavlovian stimuli to prime the instrumental responses more strongly. The experiment is therefore useful because it sets the S-O-R and hierarchical S: R-O models against one another. As Cartoni et al. (2015) noted, the results provide strong support for the hierarchical account of PIT.

1.5.3.3 Extinction

Further evidence for the hierarchical account comes from experiments demonstrating that PIT is immune to Pavlovian extinction treatments, but is profoundly influenced by *discriminative* extinction training (Delamater, 1996; Gámez & Rosas, 2005; Hogarth et al., 2014; Rescorla, 1992a; Rosas, Paredes-Olay, García-Gutiérrez, Espinosa, & Abad, 2010; although see Bezzina et al., 2016 and Lovibond et al., 2015 for recent exceptions in the case of non-selective PIT). Hogarth et al. (2014), for example, used the design outlined in Table 1.5. Smokers were first trained to perform two instrumental responses to earn cigarette and chocolate points (R1-O1, R2-O2). Two stimuli were then trained to produce tobacco points (S1-O1, S2-O1), and another two stimuli were trained to produce chocolate points (S3-O2, S4-O2). One Pavlovian stimulus (S2 and S4) signalling each outcome was then extinguished (i.e. no longer reinforced). In the subsequent transfer test, instrumental response choice was tested in the presence of each stimulus.

Table 1.5

Design and results of Hogarth et al. (2014, Experiment 1).

Instrumental conditioning	**Pavlovian conditioning**	**Pavlovian extinction training**	**Transfer test**
R1 – O1	S1 – O1	S1 – O1	S1: R1 > R2
R2 – O2	S2 – O1	S2 – no O1	S2: R1 > R2
	S3 – O2	S3 – O2	S3: R1 < R2
	S4 – O2	S4 – no O2	S4: R1 < R2

Note: R1 and R2 refer to instrumental responses, S1-S4 refer to Pavlovian stimuli, and O1 and O2 refer to appetitive outcomes.

A typical PIT effect was observed, where the non-extinguished Pavlovian stimuli selectively enhanced the instrumental response that was paired with the same outcome. Crucially, the extinguished stimuli also produced a PIT effect that was of a similar magnitude to the non-extinguished cues. It is well known that extinction treatments do not erase the original learning, but the strength of the association is still usually reduced (Bouton, 2004). S-O-R theory predicts that PIT effects depend on the Pavlovian stimulus activating the associated outcome representation. The PIT effect should, therefore, be reduced when the S-O association is weak. Hence, the insensitivity of the PIT effect to Pavlovian extinction treatments is problematic for S-O-R theory.

Direct support for hierarchical S: R-O theory comes from studies showing that PIT is sensitive to *discriminative* extinction training. Hogarth et al. (2014) again provided a good example of this. Table 1.6 shows their design. Participants first learnt to perform one response to earn beer points and another response to earn chocolate points (R1-O1, R2-O2). For an extinction group, the R1-O1 contingency was then extinguished in the presence of one discriminative stimulus S1, but not in the presence of a second stimulus S2 (see Table 1.6). A control group experienced these discriminative contingencies but did not undergo discriminative extinction training. Both groups then learnt to perform two new instrumental responses to earn each outcome (R3-O1, R4-O2). Finally, choice of these new instrumental responses (R3 vs R4) was assessed in the presence of stimulus S1 and S2. The question was whether the discriminative extinction training would influence the ability of S1 to increase R3 to obtain O1.

A PIT effect was observed for O1 in the non-extinction group. Stimulus S1 selectively increased choice of R3 – both paired with O1 – during the transfer test. Crucially, a comparable effect was not observed in the extinction group, which suggests that the discriminative extinction training successfully abolished the PIT effect. This result lends credence to the hierarchical prediction that the Pavlovian stimuli serve as *discriminative* stimuli in the PIT transfer test. That is, they signal which instrumental response is more likely to be reinforced. When training is provided that undermines this signalling function, the PIT effect is abolished. Clearly, this result provides strong support for the hierarchical S: R-O account of PIT.

Table 1.6

Design of Hogarth et al.(2014, Experiment 2).

Instrumental conditioning	**Discriminative extinction training**	**Instrumental conditioning**	**Test**
R1 – O1	**Extinction group**	R3 – O1	S1: R3/R4
R2 – O2	S1: R1-no O1, R2-O2	R4 – O2	S2: R3/R4
	S2: R1-O1, R2-O2		
	Non-Extinction group		
	S1: R1-O1, R2-O2		
	S2: R1-O1, R2-O2		

Note: R1-R4 refers to instrumental responses, S1 and S2 refer to discriminative stimuli, and O1 and O2 refer to appetitive outcomes.

The results discussed above provide compelling evidence for the hierarchical account of PIT. As noted above, this evidence (alongside other recent experiments described below) inspired the propositional EU theory that the current research tests. Indeed, both the hierarchical and propositional theories propose that the Pavlovian stimulus S signals which response R is more likely to be reinforced during the PIT transfer test. The key distinction between the hierarchical and propositional theories is that the propositional account makes the explicit claim that the hierarchical mechanism is encoded *propositionally* (Hogarth et al., 2014; Hogarth, Maynard, et al., 2015; Hogarth & Troisi, 2015; Seabrooke et al., 2016). That is, PIT effects are assumed to require effortful inferential reasoning processes and verbalizable knowledge of the Pavlovian and instrumental contingencies. The section below outlines the propositional EU account more thoroughly, and then details two key lines of support.

1.5.4 Propositional expected utility theory

The propositional EU account proposes that PIT effects are driven by an EU function that reflects judgements about both the outcome's value (Ov) and probability

(Op) (Kennerley, Dahmubed, Lara, & Wallis, 2009; Mongin, 1997; Schultz, 2006). When multiple responses are available, such as in PIT tasks, the response with the highest utility estimate is chosen. In a typical PIT transfer test, the Pavlovian stimulus is suggested to increase the perceived probability of the associated outcome Op, providing the associated instrumental response is performed (Cartoni et al., 2015; Cartoni, Puglisi-Allegra, & Baldassarre, 2013; Hogarth, 2012; Hogarth et al., 2014; Seabrooke et al., 2016). That is, participants may infer that the cue tells the participant which outcome is most likely to be earnt on that trial. This cue-evoked increase in Op is assumed to underlie the basic PIT effect: the stimulus S selectively signals the response R that shares a common outcome O by increasing the perceived probability of that outcome's Op.

So how does this propositional 'decision-making' account explain PIT's insensitivity to devaluation? When a stimulus signals a devalued outcome, it signals a high-probability (high Op) outcome that is of low value (low Ov). The other outcome retains a high outcome value Ov, but has a low probability Op. These conditions might foster a high utility estimate for the devalued outcome because the probability estimate Op is so high in the presence of the cue. Indeed, participants might even infer that the cue signals that the alternative, valued outcome is completely unavailable (zero probability Op). In other words, participants may respond for the cued, devalued outcome because it is perceived to be much more available than the alternative outcome, and this difference in availability may outweigh the difference in value. Although this account remains to be formally tested, there are two key lines of evidence (discussed below) that support the propositional EU model of PIT.

1.5.4.1 Contingency awareness

Initial support for the propositional model came from the observation that participants only demonstrate PIT effects when they can verbalise the Pavlovian and instrumental contingencies (Hogarth et al., 2007). Similar findings have also been reported using non-selective PIT designs (Bezzina et al., 2016; Lovibond et al., 2015; Talmi et al., 2008). Indeed, researchers now routinely exclude participants who do not demonstrate explicit contingency awareness, precisely because they do not typically demonstrate PIT effects (e.g., Hogarth, 2012; Hogarth et al., 2015; Lewis et al., 2013; Nadler et al., 2011). This dependency on explicit awareness of the relevant contingencies is consistent with conclusions from the Pavlovian conditioning literature more generally (Lovibond & Shanks, 2002; Mitchell et al., 2009; Shanks & St John, 1994). Of course, the finding that PIT effects depend on explicit contingency knowledge does not mean that PIT effects are necessarily under voluntary control (Bezzina et al., 2016). Awareness may correlate with PIT effects without playing a *causal* role in its generation. Nevertheless, the finding that participants only usually show PIT effects when they are able to report explicit contingency awareness fits with the propositional prediction that PIT effects depend on explicit contingency knowledge.

1.5.4.2 Verbal instructions

Further support for the propositional model comes from the finding that PIT is sensitive to post-training instructional manipulations. Hogarth et al. (2014), for example, trained participants to perform one response to earn beer points and another response to earn chocolate points. Response choice was then tested in the presence of pictorial beer or chocolate stimuli, or a neutral stimulus. During the transfer test, half of the participants were instructed that the pictures *did not indicate which response was more likely to be rewarded*. The non-instructed group showed a PIT effect, where the beer and chocolate stimuli selectively increased responding for their respective outcomes, relative to the neutral stimulus. Crucially, the PIT effect was attenuated in the instructed group, and was completely abolished in participants who did not believe that the stimuli signalled which response would be rewarded. In a similar vein, Seabrooke et al. (2016) recently reported a complete reversal of response choice in participants who had been instructed that the pictures indicated which response would *not* be rewarded. In each case, the size of the cueing effect correlated with expectancy ratings – that is, participants' self-reported expectation that the stimuli signalled which response was more likely to be rewarded during the transfer test. Together, these data support the suggestion that PIT might not reflect an automatic process, but rather a high-level, propositional process.

1.6 The current research

The research discussed above demonstrates a paradox in the PIT literature. On the one hand, PIT is often insensitive to outcome devaluation, which implies that it is relatively automatic. On the other hand, PIT is sensitive to verbal instructions, which suggests that it is mediated by a high-level propositional process. The propositional EU account of PIT provides a possible resolution to this paradox. However, there are several important aspects of this theory that are currently untested. A core aim of the current research, therefore, is to test the propositional EU account of PIT. The experiments in Chapter 2 first aim to replicate the reversed cueing effect observed by Seabrooke et al. (2016) in a PIT task. They also use either a speeded reaction time task or a concurrent load task to seek evidence for an underlying S-O-R mechanism when propositional processes are unlikely to control behaviour. Chapter 3 details the development of a novel, very strong devaluation procedure to test whether this renders PIT sensitive to outcome devaluation. Subsequent experiments also use this devaluation procedure to test whether PIT is insensitive to devaluation even when an S-O-R mechanism cannot readily control behaviour. In Chapter 4, both outcome value and perceived outcome probability are systematically manipulated to test the propositional EU theory of PIT. Finally, the results are discussed in Chapter 5, with the aim of providing a cohesive model of PIT that best accounts for the observed data.

Chapter 2: Time pressure and concurrent load

2.1 Introduction

Chapter 1 outlined the key theories of PIT. The current work aims to test the dominant dual-process theory against the propositional EU theory of PIT. The focus is therefore narrowed to these theories henceforth. To briefly reiterate, the dual-process account proposes that Pavlovian and instrumental training fosters stimulus-outcome (S-O) and outcome-response (O-R) associative links. When a Pavlovian stimulus S is presented during the PIT transfer test, it is suggested to activate the mental representation of the associated outcome O. The outcome representation then triggers the associated instrumental response R via an S-O-R associative chain (Alarcón & Bonardi, 2016; Balleine & Ostlund, 2007; de Wit & Dickinson, 2015; Hogarth, 2012; Hogarth & Chase, 2011; Holland, 2004; Watson et al., 2014, 2016). Furthermore, the S-O-R mechanism is assumed to operate automatically and without flexible motivational control. The automaticity of the S-O-R mechanism allows the dual-process account to explain why PIT effects are often insensitive to outcome devaluation manipulations (Hogarth, 2012; Hogarth et al., 2013; Hogarth & Chase, 2011; Watson et al., 2014).

The dual-process account of PIT can be contrasted to the propositional account, which proposes that PIT effects reflect controlled processes that are based on explicit awareness of the Pavlovian and instrumental contingencies. When a Pavlovian stimulus is presented during the transfer test, participants are assumed to use effortful reasoning processes to infer that the Pavlovian stimulus S signals which instrumental response R is more likely to be reinforced. Hence, the Pavlovian stimulus is suggested to increase the perceived probability (Op) of the associated outcome, providing the associated instrumental response is performed.

It is clear that both the dual-process and propositional accounts have received empirical support. The insensitivity of PIT to devaluation provides evidence of automaticity, and is therefore a key line of support for the dual-process account of PIT (Colwill & Rescorla, 1990a; Corbit et al., 2007; Hogarth, 2012; Hogarth & Chase, 2011; Holland, 2004; Rescorla, 1994b; Watson et al., 2014). The finding that PIT is sensitive to instructional manipulations provides the primary support for the propositional account (Hogarth et al., 2014; Seabrooke et al., 2016). This instructional sensitivity favours the propositional approach because it is consistent with the suggestion that PIT effects are mediated by explicit inferences about the signalling role of the Pavlovian stimuli presented during the transfer test.

The instructional sensitivity discussed above suggests that propositional processes can play an important role in PIT. However, it does not confirm that an automatic S-O-R mechanism does not produce PIT effects in *other* circumstances. It is certainly possible that both propositional and S-O-R link processes contribute to PIT effects, but that Hogarth et al. (2014) and Seabrooke et al.'s (2016) instructional experiments were not optimised to detect automaticity. This dual-process account, which proposes that PIT effects can be mediated by both link-based S-O-R and higher order propositional processes, is in line with popular dual-process accounts of associative learning more generally (e.g., McLaren et al., 2014; Sternberg & McClelland, 2012).

It is worth emphasising here that the propositional account outlined by Mitchell et al. (2009) makes a clear distinction between learning and performance effects (section 3.2, page 187). Mitchell et al.'s (2009) propositional approach assumes that human associative learning depends on higher order cognition. However, their theory is open to the idea that performance effects may be automatic. PIT is a *performance* (rather than learning) effect, because response choice during the transfer test reflects contingency knowledge acquired during the preceding training phases. The demonstration of an “automatic” PIT effect would not, therefore, challenge the propositional account of associative *learning* put forward by Mitchell et al. (2009). It would, however, provide good evidence against the propositional account of PIT that was described in Chapter 1.

The current experiments sought evidence of an automatic PIT effect when propositional processes were unlikely to control behaviour. S-O-R theory assumes that PIT effects reflect the automatic operation of associative links. Propositional inferences, by contrast, are assumed to require both time and controlled reasoning processes that have a finite capacity (Mitchell et al., 2009). The dual-process account suggests that PIT effects can be mediated by both an S-O-R link mechanism and propositional processes. Importantly, the automatic S-O-R mechanism may only be detectable when the propositional system is otherwise engaged (McLaren et al., 2014). One way to test for an underlying automatic process, then, is to implement a procedure that renders it difficult for participants to use propositional processes. This approach has been widely used to test dual-process accounts of other associative learning phenomena (De Houwer & Beckers, 2003; de Wit et al., 2013; Karazinov & Boakes, 2007; Le Pelley, Oakeshott, & McLaren, 2005; Wills, Graham, Koh, McLaren, & Rolland, 2011). The aim here is to maximise the opportunity to detect automaticity by reducing participants’ ability to use controlled processes.

2.2 Experiment 1

Experiment 1 built on Seabrooke et al.'s (2016) reversal instruction experiment, where participants learnt to perform one instrumental response (R1) to earn beer points (O1) and another response (R2) to earn chocolate points (O2). Outcomes were presented as the statement “You win”, alongside a picture of beer or chocolate, depending on the outcome. In the subsequent transfer test, response choice (R1 versus R2) was assessed in the presence of the beer and chocolate pictures that were used during training, or a neutral stimulus. Crucially, half of the participants were instructed that the pictures indicated which response would *not* be rewarded during the transfer test. Seabrooke et al.'s procedure is referred to as an *outcome-response (O-R)* task, because the outcome pictures that were used during training also served as Pavlovian stimuli during the transfer test. Thus, it was possible for the instrumental responses and outcome pictures to become associated with one another during the training phase.

Seabrooke et al.’s (2016) design was a hybrid procedure that reflected the critical aspects of typical PIT tasks and ideomotor paradigms. It allowed links to be made between the two fields, and thus went some way to testing the ideomotor (S-O-R) account of PIT. A typical cueing effect was observed in the Non-Reversal group. The beer picture increased the response that had produced beer during the training phase, and the chocolate picture similarly increased the chocolate response (relative to the neutral stimulus). The opposite effect was observed in the Reversal group; the beer and chocolate pictures increased the chocolate and beer responses, respectively. This sensitivity to the reversal instruction was interpreted as evidence for the role of propositional processes in cue-elicited response choice.

Experiment 1 first aimed to demonstrate Seabrooke et al.'s (2016) reversal effect in a PIT experiment (where the instrumental responses and pictorial stimuli were not presented together prior to the transfer test). A 2 × 2 × 3 design was used, with instruction (Non-Reversal, Reversal) and speed group (Slow, Fast) as between-subjects variables, and stimulus (beer, neutral and chocolate) as the within-subjects factor. Table 2.1 outlines the design. Participants first learned to perform one instrumental response (R1) to obtain beer points (O1), and another instrumental response (R2) to obtain chocolate points (O2). Instrumental response choice (R1 versus R2) was then tested in the presence of a stimulus that depicted each outcome (pictures of beer or chocolate), or a neutral stimulus. Familiar, pre-trained Pavlovian cues were used to first be consistent with the previous instructional experiments (Hogarth et al., 2014; Seabrooke et al., 2016), and secondly because these pictorial stimuli have very well-established associations with their outcomes. They might, therefore, be more likely to produce evidence of automaticity than stimuli that are only weakly associated with the outcomes.

Table 2.1

Instrumental training and transfer test phases of Experiment 1.

Instrumental training	Transfer test
R1-O1	S0: R1/R2?
R2-O2	S1: R1/R2?
	S2: R1/R2?

Note: R1 and R2 refer to instrumental responses (left and right arrow key presses). O1 and O2 refer to outcomes (beer and chocolate points). S0 refers to a neutral stimulus, and S1 and S2 refer to pictures of O1 and O2, respectively.

A typical PIT effect, where the beer and chocolate pictures increase choice of the responses that were trained to produce those outcomes, was expected in the Non-Reversal group. Consistent with Seabrooke et al. (2016), half of the participants were allocated to a Reversal instruction group at the start of the experiment, and were instructed just prior to the transfer test that the pictures signalled which response would *not* be rewarded. A reversed PIT effect, in which the beer and chocolate stimuli increased the responses that had *not* produced those outcomes during instrumental training, was anticipated. Such instructional sensitivity would confirm that higher order propositional processes play an important role not only in the O-R design used by Seabrooke et al. (2016), but also in PIT procedures where the stimuli and responses are not paired prior to the transfer test. The demonstration of a standard (non-reversed) PIT effect in the Reversal instruction group, by contrast, would suggest that the PIT effect is automatic because of its insensitivity to the reversal instruction.

Participants were also allocated to a Slow or Fast condition at the start of the experiment. The Slow group had unlimited time to respond during the transfer test. The Fast group, by contrast, were required to respond within time limits that were customised for each participant by pre-testing their reaction time in a practice speed task. The speed manipulation aimed to reduce participants' ability to employ effortful and time-consuming reasoning processes during the transfer test, and therefore provide a better opportunity to detect an automatic S-O-R mechanism.

The propositional account predicts that PIT effects are entirely dependent on the operation of higher order cognitive processes. Both the non-reversed and reversed PIT effect should therefore be abolished in the Fast group (assuming the speed manipulation completely eliminates participants' ability to utilise propositional processes). That is, the propositional approach predicts that the beer and chocolate stimuli will not influence response choice in either the Non-Reversal Fast or the Reversal Fast condition.

The dual-process account makes different predictions. Associative links are assumed to operate automatically, and may therefore be revealed when participants do not have sufficient time to reason (e.g., Karazinov & Boakes, 2007; Mitchell et al., 2009; Shanks, 2007; Sternberg & McClelland, 2012). Hence, a non-reversal PIT effect should be observed in the Non-Reversal Fast group, because the automatic S-O-R mechanism should not be influenced by time pressure. A reversed PIT effect may also be observed in the Reversal Slow group, because the dual-process account allows propositional processes to control behaviour when the task is not demanding. Crucially, the dual-process account predicts an automatic *non-reversal* PIT effect in the Reversal Fast group, because the propositional process should be unable to control behaviour. Assuming the speed manipulation completely eliminates participants' ability to reason, the dual-process account predicts that evidence of automaticity should be revealed. Hence, a standard (non-reversed) PIT effect should be observed in the Reversal Fast group. Such a result would provide especially strong evidence for the dual-process account of PIT.

Expectancy ratings for the cued outcome were also reported after the transfer test. A correlation between expectancy beliefs and the strength of the PIT effect would not necessarily mean that expectancies play a *causal* role in generating PIT effects. However, such a relationship would be consistent with the propositional prediction that PIT effects are mediated by a belief that the Pavlovian stimulus signals which response is more likely to be rewarded during the transfer test.

Reaction times were also recorded throughout the transfer test to test whether an ideomotor effect, where the Non-Reversal Slow group respond more quickly than the Reversal Slow group, would be observed (e.g., Elsner & Hommel, 2001; Flach et al., 2006; Watson et al., 2015). Ideomotor theory is often coupled with S-O-R theory in the PIT literature (e.g., de Wit & Dickinson, 2009, 2015; Hogarth et al., 2013; Watson et al., 2015). However, the propositional model also predicts a similar effect. The propositional model predicts that the reversed PIT effect should require more time to execute than the non-reversal PIT effect because participants must integrate knowledge about the trained instrumental contingencies with the information provided by the instruction. The Non-Reversal group, by contrast, need only apply their knowledge of the trained instrumental contingencies. Although the theories predict the same result, reaction times were nevertheless recorded to test the prediction.

Finally, participants completed an operation span (OSPAN) task (e.g., Turner & Engle, 1989) to measure working memory capacity. The specific task was developed by Wills, Milton, Longmore, Hester, and Robinson (2013). The propositional model predicts that PIT effects depend on cognitive variables including working memory. A relationship might therefore be expected between OSPAN scores and the size of the PIT effect.

2.2.1 Method

Participants. Ninety-two participants (61 females, aged between 18 and 30; mean, *M* = 20.20, standard error of the mean, *SEM* = 0.25 years), completed the experiment for course credit. Participants provided written informed consent at the start of the experiment. The study was approved by the Plymouth University Ethics Committee.

Apparatus and materials. The experiment was programmed in E-Prime 2.0 (Psychology Software Tools, Inc.; pstnet.com) and was presented on a 22-inch computer monitor. Participants made all responses using a standard keyboard. A 330ml bottle of Beck's beer and a 45 gram Cadbury's Dairy Milk chocolate bar served as reward props. A picture of beer and chocolate presented on the computer screen served as Pavlovian stimuli. The neutral stimulus was a simple grey stimulus of equal size to the Pavlovian stimuli. Participants wore headphones throughout the experiment.

Procedure

PIT task. Participants were randomly allocated to an Instruction (Non-Reversal or Reversal) and Speed (Slow or Fast) condition at the start of the experiment. After providing informed consent, they were shown the bottle of beer and the chocolate bar and were told that they could win points towards those rewards throughout the experiment. These props were removed when the computer task began.

Speed task. All groups initially completed a speed task in which they responded to apple and banana stimuli as quickly as possible. The aim was to establish each participant's individual reaction time so that it could be used as the trial duration in the transfer test for the Fast group. Participants first received the following instructions: "In this task, you can earn the beer and chocolate in front of you by pressing the left or right arrow keys. We will first have a practice round where you can try to win banana and apple points. Press the [LEFT/RIGHT] ARROW key when you see a banana. Press the [RIGHT/LEFT] ARROW key when you see an apple. The aim of this phase is to respond as quickly and as accurately as possible. Press any key to begin." The stimulus-response instructions were counterbalanced between-subjects. Each trial began with a picture of an apple or a banana above a choice symbol ("← or →"), which was presented for 1000 milliseconds (ms) or until a response was made (whichever came first). Correct responses were followed by the statement "Correct!" in green font. Incorrect responses produced a high-pitched tone and the statement "Incorrect" in red font. The instructed contingencies were also presented, in the same manner as on the original instruction page. Omission trials in which participants failed to respond within 1000ms of the stimulus onset were followed by a high-pitched tone and the statement "Too slow!" (presented in red font). The text remained on-screen until participants pressed a key to continue.

An instruction to “Please respond as quickly as possible!” was presented continuously at the bottom of the screen throughout the speed task. There were ten trials in total, with five of each fruit stimulus. Trial order was random, and the trials were separated by 750-1250ms intervals. After all ten trials, any incorrect and omission trials were repeated in a random order until participants selected the correct response. The median reaction time for the ten correct trials was then calculated.

Instrumental training. Instrumental training commenced with the following on-screen instructions: “You can now earn beer and chocolate by pressing the left or right arrow keys. You will only earn these rewards on some trials. Press any key to begin.” There were 24 trials. Each response (left and right arrow key presses) was selectively paired with either beer or chocolate points, and this was counterbalanced between-subjects. The contingencies were also counterbalanced with respect to the fruit contingencies that were established in the practice speed task. Each trial began with the choice symbol (“←or→”), which remained until participants pressed the left or right arrow key. Responses were followed by the statement, “You earn one [beer/chocolate] point”, depending on the instrumental contingency and the response chosen. One outcome was scheduled to be available on each trial (chosen randomly), and so each key had a 50% chance of yielding a reward. If participants responded for an outcome that was not available, the text “You win nothing” was presented. Outcomes were presented for 1500ms and the trials were separated by 750-1250ms intervals.

Instrumental knowledge test. Explicit contingency knowledge was then assessed with an instrumental knowledge test. On-screen instructions read, “We would now like to test whether you know which key earned which reward. Press any key to begin.” Two questions were presented in a random order: “Which key earned [beer/chocolate], the left or right arrow key? Please choose carefully.” Participants were required to press the left or right arrow key, and response time was not limited. The questions were separated by a 750-1250ms interval.

Transfer test. The transfer test was preceded by the following instructions: “In this part of the task, you can earn beer and chocolate by pressing the left or right arrow key in the same way as before. You will only be told how many of each reward you have earned at the end of the experiment. Also, sometimes a picture of beer or chocolate will be presented before you choose the left or right arrow key. Press any key to begin.” Participants in the Fast condition were also instructed, “You MUST respond as quickly as possible to win rewards.”, while the Reversal group were told, “Pictures indicate which arrow key will NOT be rewarded!” The latter instruction also appeared at the bottom of the screen throughout the transfer test.

Each trial began with the presentation of a beer, chocolate or neutral stimulus. The choice symbol (“←or→”) was presented below the stimulus, and participants were required to choose the left or right arrow key. The Slow group had unlimited time to respond. The Fast group were required to respond within their median reaction time on the *practice speed task* (see above). Failure to respond within this duration was regarded as an omission trial, and produced a high-pitched tone and the warning, “Too slow! Please respond as quickly as possible. Press any key to continue.” No other feedback was given, and so the transfer test was conducted in nominal extinction (i.e., participants were not told whether or which outcomes had been earned). The term *nominal extinction* is used because although feedback was not presented, participants were nevertheless told that rewards were accumulating throughout the transfer test (e.g., Garbusow et al., 2015; Hogarth & Chase, 2011; Hogarth et al., 2015; Watson et al., 2014, 2016). Nominal extinction procedures are commonly used to retain high levels of motivation whilst reducing the likelihood of stimulus-response (S-R) associations forming during the transfer test (Hogarth & Chase, 2011).

If an omission was recorded, the stimulus duration for the next trial was increased by 50ms. When a response *was* registered in time, the time limit for the next trial was reduced by 50ms. The latter adjustment was only implemented when it would not reduce the time limit to below the original median reaction time of the practice speed task (the time limit otherwise remained constant for the next trial). The lower time limit was controlled in this way to ensure that the stimulus remained consciously visible.

There were eight cycles of six trials (48 trials in total). In each cycle, the three pictures (beer, chocolate or neutral) were presented twice in a random order. In the Fast condition, omission trials were randomly repeated at the end of each cycle until a response was performed in time. Omission trials were discarded from the main analysis, and so 48 completed trials were obtained in each condition for the analysis. The trials were separated by 750-1250ms intervals.

Expectancy ratings. After completing the transfer test, participants read the following instructions: “We would now like to examine your thoughts about the beer and chocolate pictures. Please think carefully about your answers. Press any key to begin.” Two questions were presented, one in the presence of the beer stimulus and the other in the presence of the chocolate stimulus: “When this picture was presented, to what extent did you think that the [beer/chocolate] key was more likely to be rewarded? Press a key from 1 to 7.” The outcome (beer or chocolate) was always consonant with the stimulus that was presented. Ratings of one and seven represented “Not at all” and “Very much”, respectively. The questions were presented in a random order and were separated by a 350-750ms interval.

OSPAN task. Participants finally completed an OSPAN task (Wills et al., 2013). The OSPAN task measures working memory span by requiring participants to remember 2-6 words whilst completing maths problems. A simple maths problem (e.g., [2 × 1] + 3 = 5) was presented at the start of each trial, and participants selected the M key if the answer was correct, or the Z key if it was incorrect. A word (e.g. BED) was presented alongside each equation, and participants were instructed to simultaneously memorise the words in the order that they appeared. Each equation/word combination was presented for eight seconds or until the participant pressed the M or Z key (whichever came first). If a response was not registered within eight seconds, the message "TOO SLOW!" appeared for 1500ms. No other feedback was provided.

The trials were divided into 15 blocks. Each block contained 2, 3, 4, 5 or 6 trials, and these blocks were presented three times each (60 trials in total). The order of the blocks was random. At the end of each block, participants were required to type the words that had appeared during that block in the correct order. Word recall was not time limited, but participants were not allowed to alter previously submitted answers. Feedback was not provided. Participants competed three initial practice blocks that consisted of two trials each, before moving on to the main OSPAN task.

At the end of the experiment, participants received both a written and verbal debrief.

2.2.2 Results

Exclusions. Three participants were excluded for failing to correctly report either one or both of the instrumental contingencies during the instrumental knowledge test.

Speed manipulation. In the practice speed task, correct responses (collapsed across all speed and instruction groups) were performed with a mean reaction time of 443.35ms (*SEM* = 8.07). The Fast group performed correct trials with a mean reaction time of 444.36ms (*SEM* = 18.97) in the practice speed task. This served as the mean starting duration that participants in the Fast group were required to respond within on the first transfer test trial (note that the actual starting duration reflected participants' personal mean reaction time during the speed task, and hence was unique to each participant). The starting duration also served as the lower time limit throughout the transfer test. The Slow group performed correct trials with a mean reaction time of 442.33ms (*SEM* = 11.91), but had an unlimited time to respond during the transfer test. Reaction times in the practice speed task did not significantly differ between groups, *Fs* < 1.

Transfer test

Omissions. The number of omissions did not significantly differ in the Non-Reversal Fast (*M* = 8.71, *SEM* = 1.02) and Reversal Fast (*M* = 10.63, *SEM* = 1.22) groups, *t* (43) = 1.18, *p* > .05, 95% confidence interval (CI) = [-1.35, 5.17].

Response choice. Figure 2.1 shows the percent choice of the beer key (versus the chocolate key) during the transfer test in each instruction and speed condition. The graph indicates that there was a standard PIT effect in both the Non-Reversal Slow and Fast condition, where the beer and chocolate stimuli increased choice of the beer and chocolate response, respectively, compared to the neutral stimulus. The stimuli did not appear to have any discernible effect in the Reversal instruction group (in either speed condition).

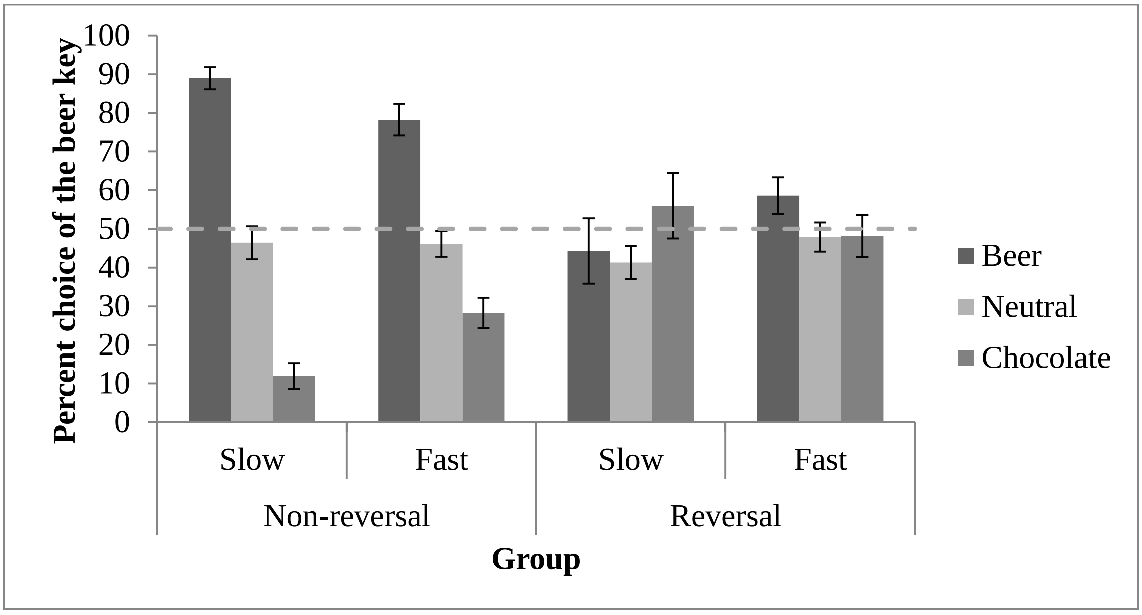

Figure 2.1. Instrumental response choice during the transfer test of Experiment 1. Response choice was tested in the presence of a beer, chocolate or neutral stimulus. The 50% mid-point represents no bias in response choice. Scores higher and lower than 50% represent a bias towards the beer and chocolate key, respectively. The error bars represent the standard error of the mean (SEM).

A 3 (Stimulus: beer, neutral and chocolate) × 2 (Instruction: Non-Reversal and Reversal) × 2 (Speed: Slow and Fast) mixed analysis of variance (ANOVA) was used to confirm these impressions. Greenhouse-Geisser corrected values are reported where necessary to correct for violations of sphericity. There was a main effect of stimulus, F (1.41, 119.96) = 29.02, $p < .001$, $\eta_p^2 = .26$, but not of speed, F (1, 85) = 3.29, $p = .07$, $\eta_p^2 = .04$, or instruction, $F < 1$. There was a significant interaction between stimulus and instruction, F (1.41, 119.96) = 28.66, $p < .001$, $\eta_p^2 = .25$. No significant interactions were observed between the stimulus and speed, or instruction and speed variables, Fs <

1. Finally, there was a three-way interaction between the stimulus, instruction and speed variables, *F* (1.41, 119.96) = 4.30, *p* = .03, η_p^2 = .05.

The significant three-way interaction was further analysed by exploring the effect of stimulus and speed in each instruction group. In the Non-Reversal group, there was an effect of stimulus, *F* (2, 80) = 127.60, *p* < .001, η_p^2 = .76, but not of speed, *F* < 1. There was a significant stimulus × speed interaction, indicating that the effect of stimulus was reduced in the Fast condition, *F* (2, 80) = 5.84, *p* = .004, η_p^2 = .13[3]. The significant interaction prompted separate, Bonferroni-corrected pairwise comparisons that explored the effect of stimulus in each speed condition of the Non-Reversal group. In the Slow condition, the beer stimulus increased choice of the beer response compared to the neutral stimulus, *t* (20) = 7.80, *p* <.001, 95% CI = [28.93, 56.19], and the chocolate stimulus, *t* (20) = 12.60, *p* < .001, 95% CI = [61.80, 92.37]. Conversely, the chocolate stimulus increased choice of the chocolate response compared to the neutral stimulus, *t* (20) = 6.44, *p* < .001, 95% CI = [21.13, 47.92].

The Fast group demonstrated a similar pattern. The beer stimulus increased choice of the beer response compared to the neutral stimulus, *t* (20) = 5.89, *p* < .001, 95% CI = [18.51, 45.77], and the chocolate stimulus, *t* (20) = 8.17, *p* < .001, 95% CI = [34.71, 65.29]. The chocolate stimulus, by contrast, increased choice of the chocolate response compared to the neutral stimulus, *t* (20) = 3.33, *p* < .01, 95% CI = [4.46, 31.25]. Thus, a PIT effect was observed in both the Non-Reversal Slow and Fast group.

Comparable analyses in the Reversal instruction group revealed a non-significant effect of speed, *F* (1, 45) = 3.23, *p* = .08, η_p^2 = .07. Inspection of the means revealed a trend towards the Fast group (*M* = 51.56, *SEM* = 1.72) performing more beer

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[3] It should be noted that the Stimulus × Speed interaction in the Non-Reversal group did not reach significance when using the pooled error term, *F* (1.41, 119.96) = 1.79, *p* > .05. This interaction is therefore interpreted with caution.

responses than the Slow group (*M* = 47.19, *SEM* = 1.72). There was no effect of stimulus, $F < 1$, nor was there a stimulus × speed interaction, F (1.26, 56.86) = 1.22, $p > .05$, $\eta_p^2 = .03$. Thus, neither the Reversal Slow nor Fast instruction group demonstrated a reversed PIT effect.

The failure to observe a significant reversed PIT effect in the Reversal instruction group is equivocal. Non-significant results arising from null hypothesis significance testing are ambiguous because they may either provide genuine evidence for the null hypothesis, or they may simply reflect an insensitivity of the data to distinguish the experimental hypothesis from the null hypothesis (Dienes, 2008). Bayes Factors provide a useful way to distinguish these possibilities. Values of more than three are regarded as evidence for the alternative hypothesis. Bayes Factors of less than one third, by contrast, reflect evidence for the null hypothesis. Values in between one third and three indicate that the data are insensitive to distinguish the theories (Dienes, 2011).

A Bayes Factor was calculated to further explore the null effect of stimulus in the Reversal Slow condition. The reversed PIT effect was expected to be of a comparable magnitude to that observed by Seabrooke et al. (2016). To calculate the size of the reversed PIT effect, the mean difference in the percent choice of the beer key between the chocolate and beer stimulus was calculated ($S_{\text{chocolate}} - S_{\text{beer}}$). Using this formula, Seabrooke et al. (2016) observed a mean difference of 51.88. The equivalent mean difference score in the Reversal Slow group in the current experiment was 11.68 (*SEM* = 16.68). A half-normal distribution with the standard deviation set as the plausible mean difference score (51.88) produced a Bayes Factor of 0.57. The Bayes Factor was in between the critical values of one third and three, and was therefore inconclusive. That is, there was insufficient evidence for either a reversed PIT effect or the null result.

Reaction times. The reaction time data are shown in Figure 2.2. The graph suggests that reaction times were longer in the Reversal group than the Non-Reversal group, but only in the Slow condition. A 2 (Stimulus: beer and chocolate) × 2 (Instruction: Non-Reversal and Reversal) × 2 (Speed: Slow and Fast) mixed ANOVA confirmed these impressions. The neutral stimulus was not included in the analysis because the hypothesis concerned the effect of the reversal instruction on reaction times to the beer and chocolate stimuli. Most interestingly, the main effect of instruction confirmed that reaction times were longer in the Reversal group than in the Non-Reversal group, F (1, 85) = 11.68, p = .001, η_p^2 = .12. Unsurprisingly, there was also a main effect of speed, with shorter reaction times observed in the Fast group than the Slow group, F (1, 85) = 104.65, p < .001, η_p^2 = .55. There was no effect of stimulus, F < 1. There was a significant interaction between the instruction and speed variables, F (1, 85) = 13.36, p < .001, η_p^2 = .14, suggesting that the effect of the reversal instruction was modulated by the speed condition. No other two-way interactions reached significance, *Fs* < 1. The three-way interaction between stimulus, instruction and speed was not significant either, F < 1.

Bonferroni-corrected pairwise comparisons revealed that the Non-Reversal Slow group responded significantly more quickly than the Reversal Slow group, t (42) = 4.98, p < .001, 95% CI = [190.46, 443.89]. This effect was not significant in the Fast condition, t < 1.

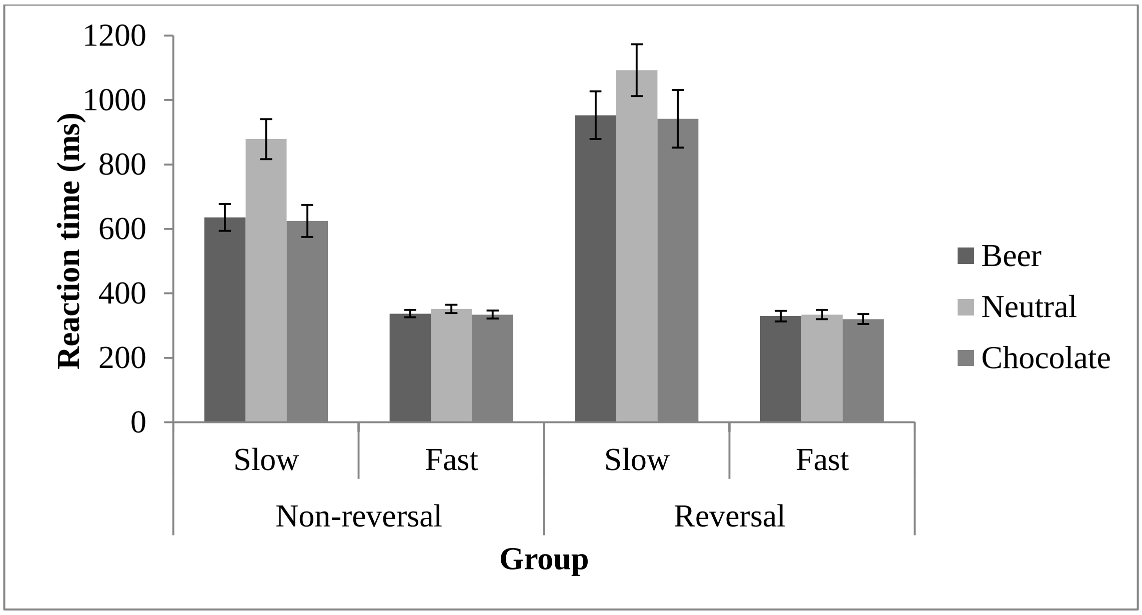

Figure 2.2. Mean reaction times during the transfer test of Experiment 1. Error bars represent SEM.

Additional correlations were calculated to explore the relationship between reaction times and the magnitude of the PIT effect in each group. The size of the beer PIT effect was calculated by subtracting the percent choice of the beer key on the neutral stimulus trials from the beer stimulus trials ($S_{beer} – S_{neutral}$). This calculation was reversed for the chocolate PIT effect ($S_{neutral} – S_{chocolate}$). The beer and chocolate PIT effects were then averaged to calculate an overall PIT effect score. Thus, larger PIT scores represent stronger non-reversal PIT effects. There was a strong positive correlation between reaction times and the size of the overall PIT effect in the Non-Reversal Fast condition, $r = .75$, $p < .001$. This correlation was not significant in any other group, *ps* > .23.

Expectancy ratings

Mean ratings. Figure 2.3 shows the mean expectancy ratings for each outcome. The graph suggests that the Reversal group gave lower expectancy ratings than the Non-Reversal group, and that overall expectancy ratings were higher for the chocolate than the beer. A mixed ANOVA revealed a main effect of instruction, with the Non-Reversal group giving higher overall ratings than the Reversal group, F (1, 85) = 9.42, $p < .01$, $\eta_p^2 = .10$. Participants also expected the chocolate more than the beer, which was confirmed by a main effect of outcome, F (1, 85) = 9.78, $p = .002$, $\eta_p^2 = .10$. There was no main effect of speed, $F < 1$, but there was a significant interaction between the speed and outcome variables, F (1, 85) = 5.29, $p < .03$, $\eta_p^2 = .06$. The remaining two- and three-way interactions did not reach significance, Fs < 1. Bonferroni-corrected pairwise comparisons revealed that expectancy ratings were higher for the chocolate than the beer in the Slow group, t (43) = 3.81, $p < .001$, 95% CI = [0.38, 1.22], but not in the Fast group, $t < 1$.

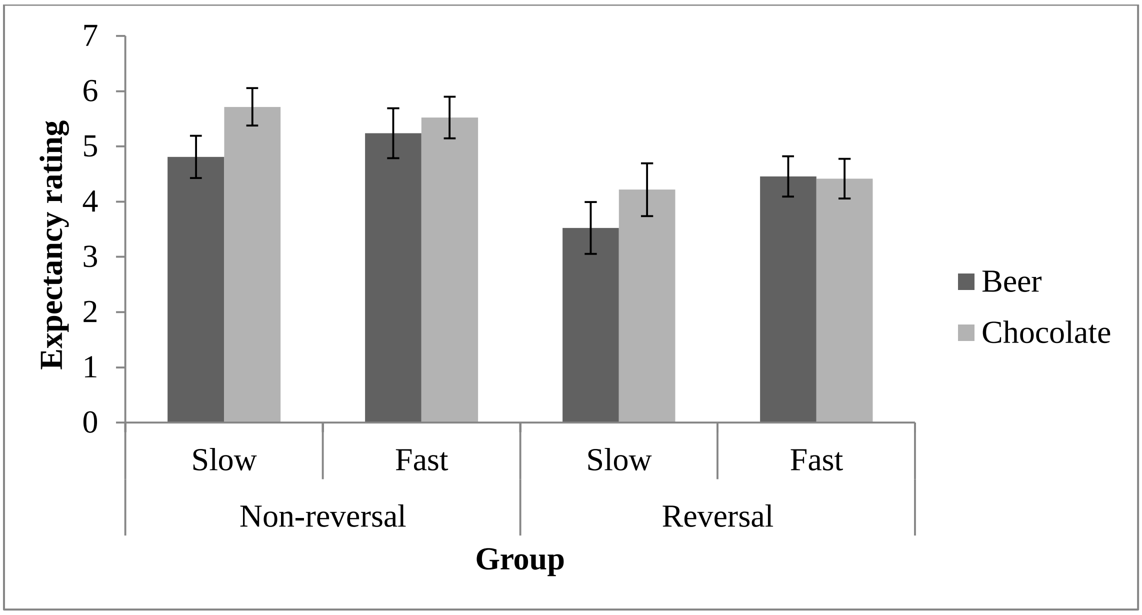

Figure 2.3. Mean expectancy ratings reported in Experiment 1. Participants were shown the beer and chocolate stimuli in turn and rated the extent to which they thought that the consonant key was more likely to be rewarded (1 = Not at all, 7 = Very much). Error bars represent SEM.

Correlations. Figure 2.4 shows the relationship between the size of the overall PIT effect and the strength of participants' self-reported expectancy ratings. Collapsed across the instruction and speed groups, expectancy ratings (averaged across the beer and chocolate outcomes) positively correlated with the size of the overall transfer effect, $r = .53$, $p < .001$. This correlation was present in both the Non-Reversal, $r = .56$, $p < .001$, and Reversal instruction group, $r = .43$, $p = .002$.

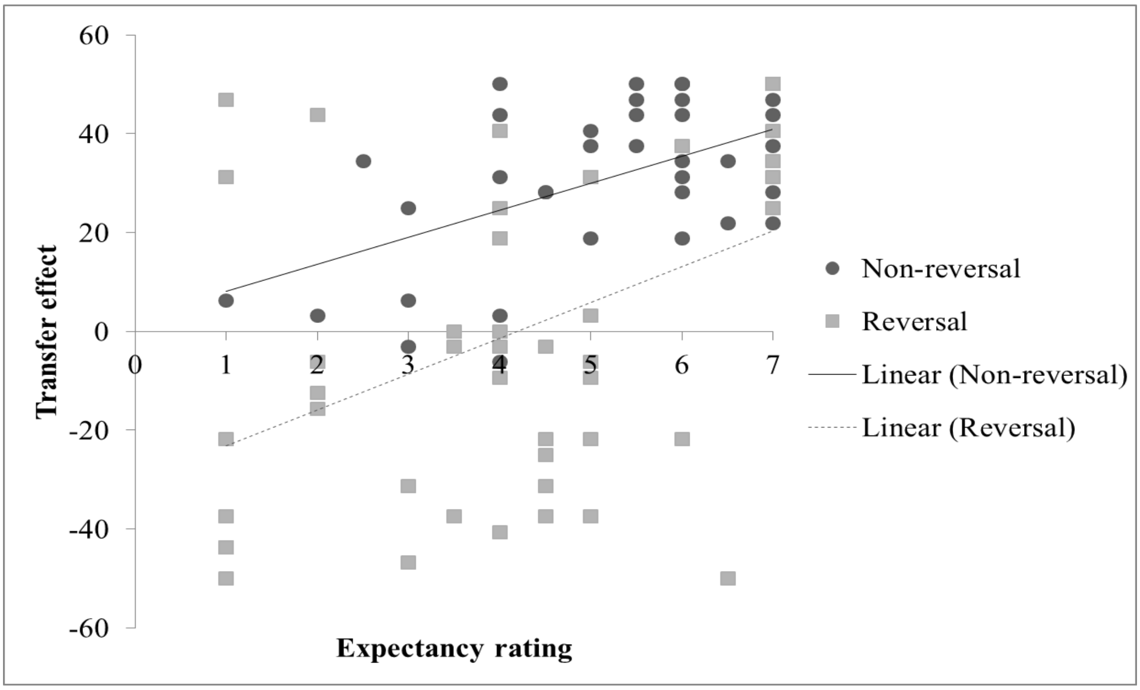

Figure 2.4. The relationship between self-reported expectancy ratings and transfer effect scores in Experiment 1.

OSPAN task. Consistent with Wills et al. (2013), working memory capacity was defined as the largest block size in which participants correctly recalled all of the words in all three repetitions of that block size. For example, if participants correctly reported all of the words in all three blocks in which two words were presented, but not when three words were presented per block, they received a working memory score of two. Thus, the maximum score was six. The minimum score of zero was given when participants failed to correctly report all of the words in all three repetitions of any block size.

According to this criterion, participants had a mean OSPAN score of 2.46 (*SEM* = 0.19). OSPAN scores did not significantly correlate with the overall transfer effect, *r*

= -.16, $p > .05$. These correlations were not significant in the Non-Reversal, $r = -.23$, $p > .05$, or Reversal instruction group, $r = -.13$, $p > .05$.

2.2.3 Discussion

Experiment 1 first aimed to establish a PIT effect and then to test whether it would be sensitive to a post-training reversal instruction. The effect of time pressure was also examined. A typical PIT effect was observed in the Non-Reversal group; the beer and chocolate stimuli selectively increased the response that was associated with the cued outcome. This outcome-selective PIT effect is consistent with much of the literature (e.g., Bray et al., 2008; Hogarth et al., 2007; Watson et al., 2014, 2016).

Several other interesting results were also observed in Experiment 1. Most notably, a non-reversal PIT effect was observed in the Non-Reversal Fast group, and a reversed PIT effect was not observed in the Reversal instruction group (irrespective of the speed manipulation). The expectancy ratings, reaction time data, and the OSPAN task also produced some interesting results. All of these results are of theoretical interest, so they will now be discussed in turn.

The first important result came from the significant non-reversal PIT effect observed in the Non-Reversal Fast group. This result suggests that PIT effects are not reliant on participants having a great deal of time to think. It is of course possible that the speed manipulation did not impose sufficient time pressure. The strong positive correlation between reaction times and the strength of the PIT effect in the Non-Reversal Fast condition provides some support for this. This correlation suggests that participants who responded more quickly during the transfer test were less likely to show a PIT effect. It therefore accords with the suggestion that the Non-Reversal Fast group demonstrated a PIT effect because they were not responding quickly enough to eliminate the propositional processes required to generate a PIT effect. However, response times were kept within tight boundaries for precisely this reason. A key assumption of the propositional approach is that behaviour is the product of a controlled process that requires time and effort (Mitchell et al., 2009). The observation of a PIT effect under time pressure is therefore problematic for the propositional account of PIT, because it suggests that PIT effects can be generated even when there is minimal time to think.

The second noteworthy aspect of the results comes from the failure to demonstrate a reversed PIT effect in the Reversal Slow group. The Bayes Factor indicated that the data were insensitive to distinguish the null hypothesis from the experimental hypothesis (which predicted a reversed PIT effect), which makes interpreting the result difficult. It also makes it difficult to assess the effect of the speed manipulation on the reversed PIT effect. At first glance, the data appear to be most consistent with a dual-process account in which an automatic S-O-R process and a higher order propositional process interact to *jointly* determine response choice. The propositional process may have fostered a reversed PIT effect (because it was sensitive to the instruction), while the S-O-R link process produced a typical non-reversal PIT effect (because it operated automatically). On test, the two processes may have combined to produce a net effect that was somewhere between a standard PIT effect and a reversed PIT effect.

The failure to demonstrate a reversed PIT effect in the Reversal Slow group is clearly problematic for the propositional approach. It is also inconsistent with Seabrooke et al.'s (2016) findings, who reported a complete reversal of response choice in a procedure that was very similar to the one employed here. One possibility is that the participants either did not understand or believe the reversal instruction in the current experiment. If participants were not entirely receptive to the instruction, then the propositional approach would not predict a reversed PIT effect. The expectancy ratings, which measured the extent to which participants believed that the pictorial stimuli signalled that the cued response was more likely to be rewarded, provide some insight into this issue. Sixty-five percent of the Reversal Slow group reported a mean expectancy rating of 3.5 or higher (the mid-point on the expectancy scale). This suggests that many of the participants believed that the cued response was more likely to be rewarded during the transfer test, despite the reversal instruction. Hence, the reversal instruction appeared to be ineffective in altering participants' propositional beliefs about the signalling role of the stimulus. Participants who *did* report beliefs that were consistent with the instruction were less likely to show a PIT effect during the transfer test. In sum, the data may be reconciled with the propositional approach by arguing that the instruction did not successfully alter participants' propositional beliefs about the signalling role of the stimulus.

The account described above may reconcile the failure to observe a reversed PIT effect with the propositional account. However, the questions remains as to why participants would understand and believe the instruction in the O-R experiment reported by Seabrooke et al. (2016), but not in the current procedure. One possibility is that the PIT effect is more 'automatic' than the O-R effect, and is hence less sensitive to instructional manipulations. It is worth noting that associative link-based approaches would typically predict the *opposite* result. From a link perspective, the O-R effect reflects a direct link between the instrumental response and the outcome picture. PIT effects, by contrast, reflect indirect S-O-R associations. The association between the stimulus S and the response R should therefore be weaker (and hence, less likely to produce automaticity) in the PIT procedure than in the O-R task. The fact that the *opposite* result was observed (performance was sensitive to the instruction in the O-R task but not in the PIT task) undermines this analysis.

Another possibility is that propositional processes mediated response choice in both experiments, but that successful reversal was more difficult (i.e. required more controlled resources) in the PIT task than in the O-R design. This possibility seems somewhat unlikely considering that the failure to reverse was observed even under non-speeded conditions, when the task was not the particularly challenging. Indeed, the fact that a PIT effect was observed in the Non-Reversal Fast condition attests to the fact that the task was not especially demanding, because participants generated a clear PIT effect even when they were required to respond very quickly.

An arguably better explanation for the differential success of the reversal instruction observed by Seabrooke et al. (2016) and in the current experiment is that it arose from differences in exposure to the instruction. In both experiments, the reversal instruction was presented at the start of the transfer test and then continuously at the bottom of the screen throughout the transfer test. In Seabrooke et al.'s (2016) design, the reward stimuli were presented for 3000ms before participants were able to respond (and the reversal instruction was presented at the bottom of the screen during this time). The 3000ms delay was not included in the current design so that it did not undermine the speed manipulation in the Fast group (by giving participants time to prepare their response). The absence of the delay may have inadvertently reduced attention to the instruction. This interpretation is clearly speculative, and more research is needed for confirmation. A simple test would be to replicate the current experiment, but ensure that there is greater opportunity to attend to the instruction. If the failure to demonstrate a reversed PIT effect in the current experiment was due to reduced exposure to instruction, a reversed PIT effect should now be observed. This possibility was explored in Experiment 2 in the context of a concurrent load manipulation. The speed manipulation was not very effective in either instruction group in Experiment 1. A concurrent load task was therefore implemented in Experiment 2, in the hope that it would provide greater insight into the role of automatic and controlled processes in PIT.

Before concluding this discussion, it is worth noting some interesting patterns in the reaction time data. The Non-Reversal Slow group responded more quickly to the beer and chocolate stimuli than the Reversal Slow group. These data are consistent with typical priming effects seen in the ideomotor literature (Elsner & Hommel, 2001; Flach et al., 2006; Watson et al., 2015). Recall that Elsner and Hommel (2001), for example, trained participants to perform two responses in an initial training phase. One response was followed by a high tone and the other response was followed by a low tone. In the subsequent test phase, the tones were presented as imperative stimuli and participants were instructed to select one of the trained responses following each tone. For half of the participants, the instructed tone-response mappings during the test phase were congruent with the learned response-tone mappings during the training phase. The trained and instructed test mappings were incongruent for the remaining participants. A now-classic ideomotor effect was observed, where the congruent group responded more quickly than the incongruent group. The current experiment produced a similar result in that the Non-Reversal group was faster to respond than the Reversal group. The result therefore demonstrates that very similar results can be obtained in a PIT procedure, where there is little opportunity for a direct association to form between the stimulus and response.

Finally, no significant correlations were observed between the magnitude of the PIT effect and working memory capacity (as measured using the OSPAN task). In general, OSPAN scores were relatively low. It is possible that there *is* a relationship between working memory capacity and PIT, but that it was not detectable because the range of OSPAN scores was not sufficiently distributed. This issue will be discussed further in section 2.4 (General Discussion).

In sum, Experiment 1 established a non-reversal PIT effect that was observed even under time pressure. The reversal instruction abolished the non-reversal PIT effect but produced no clear evidence of a reversed PIT effect. Furthermore, the speed manipulation had no significant effect in the Reversal instruction group. The aims of Experiment 2 were two-fold. The experiment first aimed to replicate the reversed cueing effect observed by Seabrooke et al. (2016) in a typical PIT task. To this end, the PIT procedure of Experiment 1 was repeated, but a delay was introduced between the stimulus onset and the opportunity to respond during the transfer test (thus making the conditions more comparable to that of Seabrooke et al., 2016). The experiment secondly aimed to test whether the non-reversal and reversal PIT effects would be sensitive to a demanding concurrent load task. Similar to the speed manipulation used in Experiment 1, the concurrent load task was expected to reduce participants' capacity to use controlled reasoning processes during the transfer test. The speed manipulation did not produce any discernible effects on performance, so Experiment 2 tested whether a concurrent load task would be more successful.

2.3 Experiment 2

Experiment 2 followed the approach of Experiment 1 in that it sought evidence of a PIT effect when propositional processes were unlikely to prevail. Participants initially learnt to perform one response to earn beer points and another response to earn chocolate points (R1-O1, R2-O2). Response choice (R1 versus R2) was then tested in the presence of a beer, chocolate or neutral stimulus. Consistent with Experiment 1, participants were randomly allocated to a Non-Reversal or Reversal instruction group at the start of the experiment. Crucially, half of the participants in each instruction group completed the transfer test whilst engaged in a demanding concurrent load task. The concurrent load task aimed to consume controlled propositional processes, and thereby seek evidence of underlying automatic cue-control. The concurrent load task was developed by Wills et al. (2011), who demonstrated that it was sufficiently demanding to generate a switch from inferential, rule-based generalisation to non-deliberative, feature-based generalisation.

The implementation of the concurrent load task during the transfer test meant that the average trial duration was considerably longer than in Experiment 1. The No Load group did not receive the concurrent load task, but the trial duration was matched. Exposure to the reversal instruction was therefore much longer in both groups than in Experiment 1, because the instruction was presented continuously at the bottom of the screen throughout the transfer test. If the failure to demonstrate a reversed PIT effect in Experiment 1 was due to insufficient exposure to the instruction (as was suggested), then a reversed PIT effect should now be observed in the Reversal No Load group. The demonstration of a reversed PIT effect would be consistent with the results of Seabrooke et al. (2016), and would suggest that PIT is, at least sometimes, mediated by higher-order propositional processes. A failure to demonstrate a reversed PIT effect, on the other hand, would suggest that the failure to reverse in Experiment 1 was not due to reduced exposure to the reversal instruction. Rather, it would complement the results of Experiment 1 in suggesting that propositional processes do not play a causal role in PIT.

The propositional and dual-process accounts make very similar predictions to Experiment 1. The crucial predictions are with respect to the Reversal Load group, so this group will be focused on. If the concurrent load successfully consumes participants' finite reasoning processes, the propositional approach predicts that response choice will be at chance in the Reversal Load group. The dual-process account, by contrast, predicts that automaticity will be revealed in the Reversal Load group. Under these circumstances, the dual-process account predicts that response choice will be mediated by an automatic S-O-R link mechanism, which should be immune to the reversal instruction.

2.3.1 Method

The method was the same as Experiment 1, except in the following respects.

Participants. Sixty-one participants (31 males, aged 18-27; *M* = 20.23, *SEM* = 0.25 years), took part in the experiment for course credit or on a voluntary basis.

Procedure

Practice concurrent load task. In place of the practice speed task, all groups began the experiment by completing ten practice trials of the concurrent load task. Each trial began with a blank screen that was presented for 500ms. Six unique, randomly chosen, single-digit numbers were then presented through participants' headphones. Participants were instructed that they should remember those numbers in order. The numbers were presented at 330ms intervals and were voice-synthesized. A fixation cross was then presented centrally for 500ms, followed by a blank screen for a further 500ms. The letter C or M was then presented for half of the trials each, and participants were required to select the corresponding key on the computer keyboard (responding was not time-limited). The letters, which were presented at the top centre and bottom centre of the screen, were presented to increase the interval (in place of the pictures presented during the transfer test) between the number sequence and the test that occurred at the end of each sequence (see below). Finally, a number that had been heard previously was presented on-screen, and participants were required to select the number that came *next* in the sequence. For example, if participants heard the sequence "3, 4, 6, 1, 5, 2” and the number ‘six’ was then presented on screen, the correct answer would be ‘one’. Responses were not time-limited, feedback was not given, and the trials were separated by 750-1250ms intervals.

Instrumental training. At the end of the practice load task, participants completed the instrumental training phase and instrumental knowledge test of Experiment 1.

Transfer test. The transfer test began with the instructions of Experiment 1; participants were told that that they could continue to earn beer and chocolate points as before, that they would now sometimes see pictures before they selected a response, and that they would only be informed of their winnings at the end of the experiment.

The Load group were additionally informed that numbers would be presented over their headphones, and that they should respond to this memory task in the same way they did in the practice phase. The No Load group were told to simply select the number that appeared on-screen at the end of each trial. Both groups were informed that their performance would determine the amount of beer and chocolate they would win (the instruction did not explicitly refer to the PIT task or the concurrent load task). Finally, the Reversal instruction group were told that “The pictures indicate which arrow key will NOT be rewarded!” This reversal instruction was presented with the initial instructions and continuously at the bottom of the screen throughout the transfer test.

In the Load condition, the transfer test followed the same format as the practice load task. Each trial began with a blank screen for 500ms, before six unique, single-digit numbers were chosen and presented randomly through the participants’ headphones (in 330ms intervals). The No Load group, by contrast, simply saw a blank screen for the equivalent time (2480ms). A centrally-presented fixation cross (500ms) was then presented for both groups, followed by a blank screen (500ms). A beer, chocolate or neutral stimulus was then presented, above the choice symbol used during instrumental training (“← or →”), and participants were required to select the left or right arrow key. Responses were not time-limited, and no feedback was given. Finally, a probe number was presented on screen. The No Load group was required to press the key corresponding to the number on the screen, while the Load group was required to select the number that followed the on-screen number in the sequence presented at the start of the trial. There were 48 trials that were divided into eight cycles of six trials. Each cycle contained two presentations of each stimulus (beer, chocolate and neutral), and trial order was random within each cycle. The trials were separated by 750-1250ms intervals. At the end of the transfer test, participants completed the expectancy ratings and the OSPAN task of Experiment 1, and were finally fully debriefed.

2.3.2 Results

Exclusions. One participant was excluded for failing the instrumental knowledge test.

Transfer test

Response choice. Figure 2.5 shows the results of the transfer test. The graph indicates that a non-reversal PIT effect was present in the Non-Reversal No Load and Load groups. A reversed PIT effect was also apparent in the Reversal No Load group, but not in the Reversal Load group.

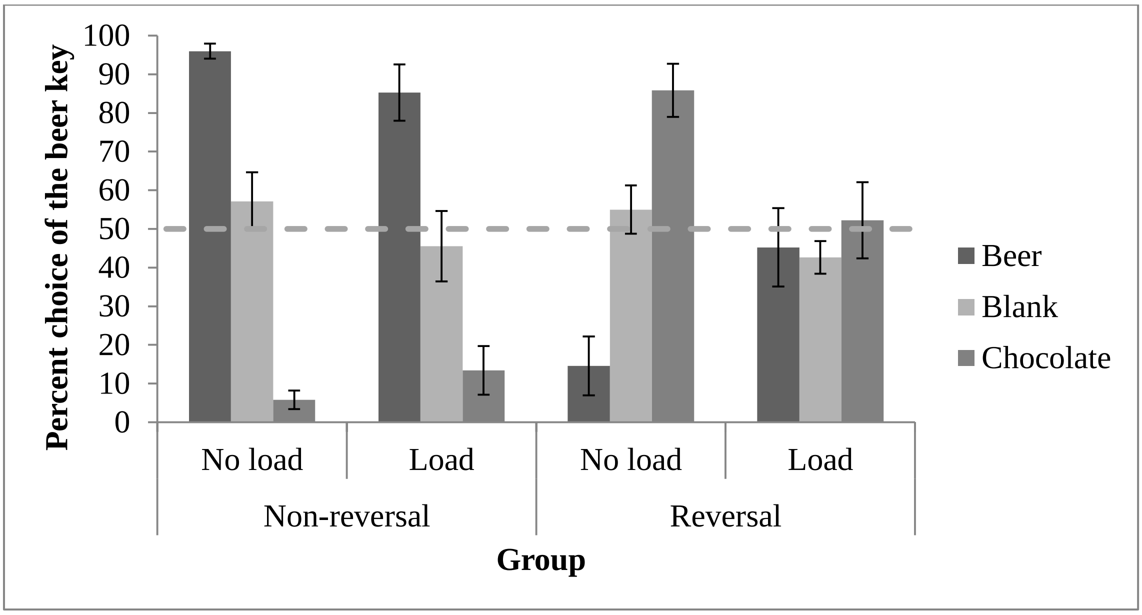

Figure 2.5. Instrumental response choice during the transfer test of Experiment 2. Response choice was tested in the presence of a beer, chocolate or neutral stimulus. The 50% mid-point represents no bias in response choice. Scores greater and lower than 50% represent a bias towards the beer and chocolate key, respectively. Error bars represent SEM.

The data were analysed using the approach of Experiment 1. Greenhouse-Geisser corrected values are reported where necessary to correct for violations of sphericity. There was a main effect of stimulus, F (1.48, 82.59) = 7.18, $p < .01$, $\eta_p^2 = .11$, but not instruction, $F < 1$, or load group, F (1, 56) = 2.03, $p > .05$, $\eta_p^2 = .04$. There was a significant interaction between stimulus and instruction group, F (1.48, 82.59) = 58.98, $p < .001$, $\eta_p^2 = .51$, but not between instruction and load, $F < 1$, or stimulus and load, F (1.48, 82.59) = 2.75, $p = .09$, $\eta_p^2 = .05$. Finally, there was a three-way interaction between the stimulus, load and instruction group variables, F (1.48, 82.59) = 6.96, $p < .01$, $\eta_p^2 = .11$.

The significant three-way interaction prompted separate analyses exploring the effect of stimulus and load in each instruction group. In the Non-Reversal group, there was an effect of stimulus, F (2, 52) = 105.48, $p < .001$, $\eta_p^2 = .80$, but not of load, $F < 1$.

The stimulus × load interaction did not reach significance, *F* (2, 52) = 1.89, *p* > .05, η_p^2 = .07. Bonferroni-corrected pairwise comparisons revealed that the beer stimulus increased the beer response compared to the neutral stimulus, *t* (27) = 6.08, *p* < .001, 95% CI = [22.76, 55.81], and the chocolate stimulus, *t* (27) = 15.44, *p* < .001, 95% CI = [67.60, 94.45]. Conversely, the chocolate stimulus increased the chocolate response compared to the neutral stimulus, *t* (27) = 8.49, *p* < .001, 95% CI = [29.17, 54.32]. Thus, a non-reversal PIT effect was observed in the Non-Reversal group, irrespective of the concurrent load manipulation.

Comparable analyses in the Reversal instruction group revealed an effect of stimulus, *F* (1.21, 36.34) = 9.21, *p* < .01, η_p^2 = .24, but not of load, *F* (1, 30) = 1.93, *p* > .05, η_p^2 = .06. There was a significant interaction between the stimulus and load variables, *F* (1.21, 36.34) = 6.45, *p* = .01, η_p^2 = .18, which prompted separate, Bonferroni-corrected pairwise comparisons in each load group. In the Reversal No Load group, the beer stimulus increased choice of the chocolate response compared to the neutral stimulus, *t* (14) = 4.27, *p* = .001, 95% CI = [16.42, 64.42], and the chocolate stimulus, *t* (14) = 4.00, *p* = .001, 95% CI = [26.03, 116.47]. Conversely, the chocolate stimulus increased choice of the beer response compared to the neutral stimulus, *t* (14) = 2.79, *p* < .05, 95% CI = [2.77, 58.90]. There were no significant effects of stimulus in the Reversal Load group, *t*s < 1. Thus, a reversed PIT effect was observed in the No Load group, but not in the Load group.

The null effect of stimulus in the Reversal Load group was further explored with a Bayes Factor calculation. For the purposes of calculating the priors, the dual-process model was assumed to predict a non-reversal PIT effect of a similar size to the non-reversal PIT effect observed in the Non-Reversal Fast group of Experiment 1. The mean difference in the percent choice of the beer key between the beer and chocolate stimulus

($S_{beer} – S_{chocolate}$) was used to calculate the size of the PIT effect for the Bayes analysis. The mean difference score in the Non-Reversal Fast group of Experiment 1 was 50. The equivalent mean difference score in the Reversal Load group of the current experiment was -6.99 (SEM = 19.60). A half-normal distribution with the standard deviation set as the plausible mean difference score (50) produced a Bayes Factor of 0.28. This Bayes Factor is below the critical lower threshold of one third, and so provides evidence for the null hypothesis (Dienes, 2011). Hence, the data support the conclusion that there was no significant difference in response choice in the presence of the beer and chocolate stimuli in the Reversal Load group.

Finally, the relationship between accuracy on the concurrent load task (i.e. whether participants responded correctly to the number probe) and the size of the overall transfer effect (which was calculated in the same way as Experiment 1) was examined. There was a marginal but non-significant negative correlation between accuracy on the concurrent load task and the size of the transfer effect in the Non-Reversal Load group, $r = -.51$, $p = .06$. Thus, participants who did well on the concurrent load task tended to show a reduced PIT effect. Comparable correlations did not approach significance in any of the other groups, *p*s > .53.

Reaction times. The transfer test reaction time data are shown in Figure 2.6. The graph suggests that the Non-Reversal group responded more quickly than the Reversal group. Mean reaction times were analysed in a comparable way to Experiment 1. There was a main effect of instruction, with longer reaction times in the Reversal group than the Non-Reversal group, F (1, 56) = 8.05, $p < .01, \eta_p^2 = .13$. There was no significant effect of stimulus, $F < 1$, or load, F (1, 56) = 2.42, $p > .05$, $\eta_p^2 = .04$. None of the two- or three-way interactions reached significance, *F*s < 2.88, *p*s > .05.

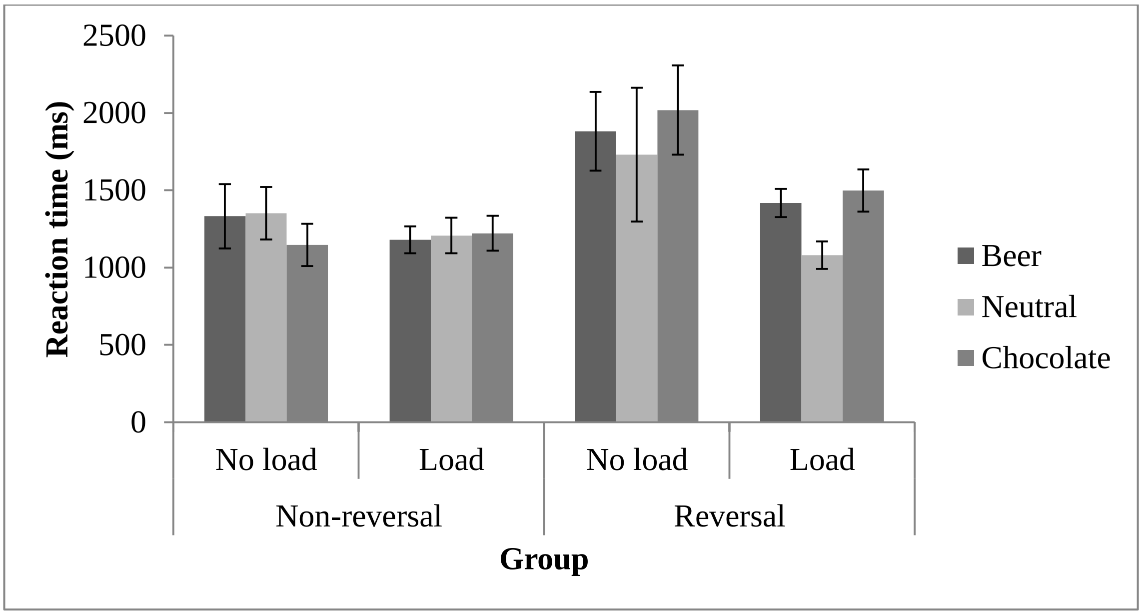

Figure 2.6. Mean reaction times during the transfer test of Experiment 2. Error bars represent SEM.

Expectancy ratings

Mean ratings. Figure 2.7 shows the mean expectancy ratings for each outcome in the instruction and concurrent load groups. The graph indicates that expectancy ratings were reduced in the Reversal instruction group, particularly in the No Load group. The data were analysed in a comparable way to Experiment 1. There was a main effect of instruction, with lower expectancy ratings in the Reversal group than the Non-Reversal group, F (1, 56) = 22.95, $p < .001$, $\eta_p^2 = .29$. There was also a main effect of outcome, with participants reporting greater expectancy for the chocolate than the beer, F (1, 56) = 4.76, $p < .05$, $\eta_p^2 = .08$. There was no significant effect of load, F (1, 56) = 2.62, $p > .05$, $\eta_p^2 = .05$, but there was an interaction between the instruction and load groups, F (1, 56) = 7.67, $p < .01$, $\eta_p^2 = .12$. No other two-way interactions were observed, Fs < 1.39, ps > .05. Bonferroni-corrected pairwise comparisons revealed that the load manipulation did not significantly alter expectancy ratings in the Non-Reversal condition, *t* < 1 , but increased expectancy ratings in the Reversal condition, *t* (30) = 3.21, *p* < .01, 95% CI = [0.67, 2.88].

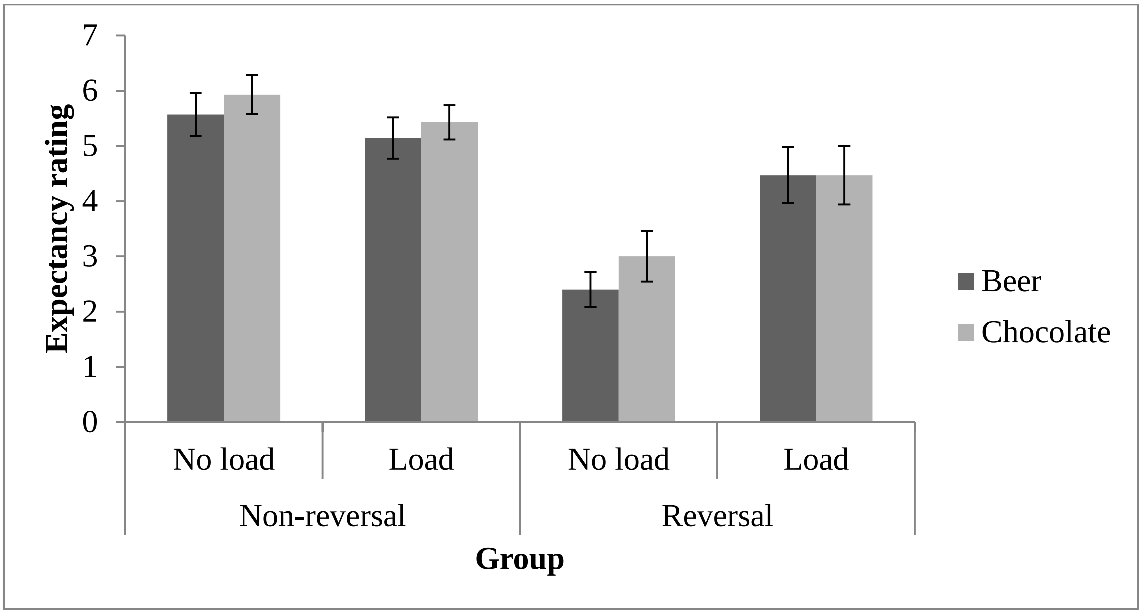

Figure 2.7. Mean expectancy ratings reported in Experiment 2. Participants were shown the beer and chocolate stimuli in turn and rated the extent to which they thought that the consonant key was more likely to be rewarded (1 = Not at all, 7 = Very much). Error bars represent SEM.

Correlations. Figure 2.8 shows the relationship between transfer effect scores (calculated in the same was as in Experiment 1) and self-reported expectancy of the cued outcome. Collapsed across outcomes, there was a positive correlation between expectancy ratings and the size of the transfer effect, *r* = .75, *p* < .001. This correlation was present in both the Non-Reversal, *r* = .61, *p* = .001, and the Reversal instruction group, *r* = .68, *p* < .001.

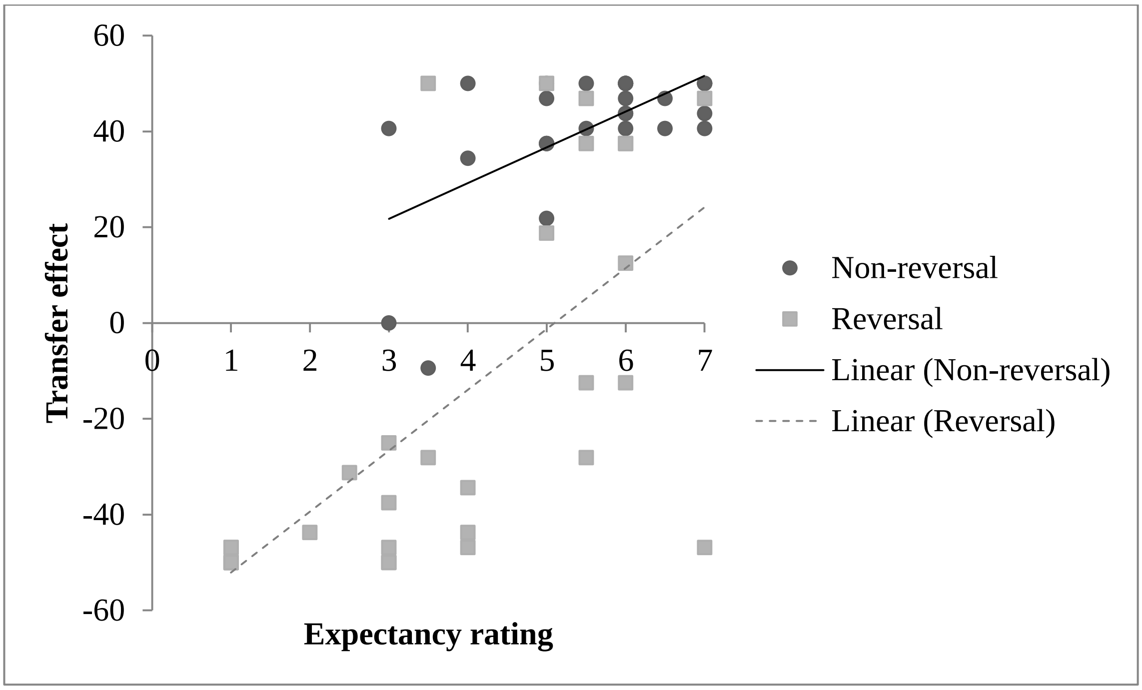

Figure 2.8. The relationship between self-reported expectancy ratings and transfer effect scores in Experiment 2.

OSPAN task. OSPAN scores were calculated in the same way as Experiment 1. The data from one participant are missing because of a computer failure. The mean OSPAN score for the remaining participants was 2.81 (*SEM* = 0.24). OSPAN scores did not significantly correlate with the size of the overall transfer effect, *r* = -.15, *p* > .05. These correlations did not reach significance in either the Non-Reversal, *r* = -. 008, *p* > .05, or the Reversal instruction group, *r* = -.29, *p* > .05.

2.3.3 Discussion

Participants in Experiment 2 first learnt to perform one instrumental response to earn beer points and another instrumental response to produce chocolate points. Instrumental response choice was then assessed (in extinction) in the presence of a beer, chocolate, or neutral stimulus. Half of the participants were instructed that the pictures signalled which response would *not* be rewarded. Within this instructional manipulation, half of the participants also completed a demanding concurrent load task throughout the transfer test.

The first noteworthy result is that an outcome-selective PIT effect was observed in the Non-Reversal group. That is, the beer and chocolate stimuli selectively increased choice of the signalled outcome. The *opposite* pattern was observed in the Reversal No Load group. Here, the beer and chocolate stimuli increased not the response that was paired with the cued outcome during instrumental training, but the response that was more likely to be reinforced according to the instruction. This instructional sensitivity is consistent with the results reported by Seabrooke et al. (2016), and suggests that high-level propositional processes can play an important role in PIT.

Interestingly, a non-reversal PIT effect was observed even in the Non-Reversal Load group. This result is consistent with the non-reversal PIT effect that was observed in the Non-Reversal Fast condition of Experiment 1. Together, these data provide preliminary evidence for an ‘automatic’ PIT effect, at least in the sense that the PIT effects reported here do not depend on a great deal of time (Experiment 1) or controlled processing (Experiment 2).

The reversed PIT effect, by contrast to its non-reversed counterpart, was abolished by the load manipulation. Clearly, the concurrent load task was sufficiently demanding to consume the cognitive processes necessary to produce the instructed, reversed PIT effect. Under these circumstances, the propositional account predicts that response choice should be at chance in the presence of all three stimuli. The non-significant effect of stimulus in the Reversal Load group (and the associated Bayes Factor) therefore supports this propositional prediction.

The dual-process account, by contrast, predicted a non-reversal PIT effect in the Reversal Load group. The data are less readily reconciled with this prediction. One possibility is that the concurrent load task reduced but did not *eliminate* participants' ability to use propositional reasoning processes. The remaining propositional processes may have then summed together with the automatic link mechanism to produce the null result observed. Clearly, this is one way in which the dual-process account could be reconciled with the data observed in the Reversal Load group. A natural prediction of this account is that more demanding versions of either the load or the PIT task would be more likely to produce evidence of a non-reversal PIT effect in the Reversal Load group. This issue will be discussed further in section 2.4 (General Discussion).

Other interesting results came from the expectancy ratings and reaction time data. The Non-Reversal group firstly reported stronger expectations that the cued response was more likely to be rewarded than the Reversal instruction group. This effect is consistent with the results of Experiment 1, and suggests that verbalizable expectancies may play an important role in PIT (Hogarth et al., 2007; Seabrooke et al., 2016). Further support for the role of expectancies in PIT comes from the positive correlation between the strength of the expectancy ratings and the size of the transfer effect (a correlation that was also apparent in Experiment 1). Although causal conclusions cannot be drawn from correlational data, the consistency of the relationship does suggest a clear association between self-reported expectancy ratings and the magnitude of the PIT effect. Interestingly, the Reversal Load group reported significantly higher expectancy ratings than the Reversal No Load group. One possibility is that the Load group paid less attention to the reversal instruction (presented at the bottom of the screen throughout) because they were engaged with the concurrent load task. This explanation, which would be consistent with both the dual-process and propositional account, may explain (at least in part) why the Reversal Load group did not show any evidence of a reversed PIT effect.

Finally, the Non-Reversal group responded significantly more quickly in the presence of the beer and chocolate stimuli than the Reversal instruction group. This result replicates the reaction time effect observed in Experiment 1, and is consistent with the reaction time effects that have been reported in the ideomotor literature (Elsner & Hommel, 2001; Flach et al., 2006; Watson et al., 2015). This discussion will be resumed below.

2.4 General Discussion

In two experiments, participants learnt to perform one response to earn beer points and another response to earn chocolate points. Response choice was then tested in the presence of a beer, chocolate, or neutral stimulus. Half of the participants were instructed during the transfer test that the stimuli signalled which response would *not* be rewarded. Half of the participants in each instruction group also completed the transfer test under speeded conditions (Experiment 1), or whilst completing a demanding concurrent load task (Experiment 2).

A typical PIT effect was observed in the Non-Reversal condition of both experiments. That is, the beer and chocolate pictures selectively biased response choice towards the instrumental response that had previously produced those outcomes, relative to the neutral stimulus. Notably, non-reversal PIT effects were observed even under considerable time pressure in Experiment 1, and whilst participants completed a demanding concurrent load task in Experiment 2. The propositional model makes the key prediction that PIT effects should only be observed when participants are able to use controlled reasoning processes, which require time and working memory resources. The observation of a non-reversal PIT effect under speed and concurrent load is therefore problematic for the propositional account of PIT. Rather, the data suggest that

PIT effects can have an automatic quality, at least in the sense that they do not require a great deal of time or controlled processes to execute.

The reversal instruction data, in contrast, speak against the idea that PIT is automatic. In Experiment 2, a complete reversal of the PIT effect was observed in participants who were instructed that the stimuli presented during the transfer test signalled which response would *not* be rewarded. It is important to recognise that this effect was not observed in Experiment 1, and so it may seem premature to conclude that PIT is sensitive to instructional manipulations. However, the conclusion that PIT (and related phenomena) is sensitive to verbal instructions is consistent with previous reports (Hogarth et al., 2014; Seabrooke et al., 2016), as well as other unpublished data from our laboratory. Thus, it seems sensible to conclude that PIT is, at least sometimes, sensitive to instructional manipulations, which suggests that PIT effects can be mediated by propositional processes.

Two aspects of Experiment 2 allow stronger conclusions to be made about the effect of the concurrent load manipulation in the Reversal group. First, a reversed PIT effect was observed in the Reversal No Load group. Second, the load manipulation completely abolished the reversed PIT effect. The latter result suggests that the concurrent load task was successful in consuming the cognitive resources necessary to produce the reversed PIT effect. Under these circumstances, the propositional and dual-process accounts make different predictions with respect to the Reversal Load group. The dual-process model predicts that automaticity should be revealed when participants are unable to use controlled reasoning processes. Thus, the dual-process model predicts that an automatic, non-reversal PIT effect should be observed in the Reversal Load group. The propositional model, by contrast, predicts that the standard, non-reversal PIT effect is a non-automatic effect that depends entirely on controlled cognitive processes.

It does not, therefore, predict that an automatic non-reversal PIT effect should be observed when the implementation of reversal instructions is eliminated through cognitive load (in the Reversal Load group). Rather, the propositional model predicts that response choice should be at chance throughout the transfer test (irrespective of the stimulus present) – as was observed. Crucially, there was no evidence of an automatic non-reversal PIT effect, and this null result was supported by the Bayes Factor analysis. This aspect of the data therefore seems to be most naturally accounted for by the propositional account.

The question remains as to why participants failed to demonstrate a reversed PIT effect in Experiment 1. In the absence of Experiment 2, this result is clearly problematic for the propositional account. It was speculated that participants may not have had sufficient exposure to the reversal instruction in Experiment 1. Indeed, the Reversal group of Experiment 1 had less exposure to the instruction than in any other experiment in which sensitivity to verbal instructions has been observed (Hogarth et al., 2014; Seabrooke et al., 2016). The reduced exposure to the instruction in Experiment 1 may have led participants to pay less attention to it. Some support for this ‘reduced attention’ interpretation comes from participants’ self-report expectancy ratings (of the extent to which the pictorial beer and chocolate stimuli signalled that the associated response was more likely to be rewarded). In Experiment 1, 65% of participants in the Reversal Slow condition reported mean expectancy ratings of 3.5 (the midpoint on the expectancy scale) or higher. These ratings suggest that many of the participants in the Reversal Slow condition continued to believe that the picture signalled that the associated response was more likely to be rewarded (despite the reversal instruction). In Experiment 2, by comparison, only 33.33% reported mean expectancy ratings of 3.5 of higher in the (equivalent) Reversal No Load condition. It seems that (for whatever reason), participants had greater confidence in the reversal instruction in Experiment 2 than in Experiment 1. It therefore seems prudent to conclude that the failure to observe a reversed PIT effect in Experiment 1 was most likely due to reduced confidence in the reversal instruction (although this conclusion remains to be tested directly).

The immunity of the non-reversal PIT effect to the speed (Experiment 1) and concurrent load (Experiment 2) manipulations supports an ‘automatic’ account of PIT. Yet the reversed PIT effect observed in Experiment 2 supports the propositional account. These results are therefore paradoxical. One way to reconcile the results would be to speculate that both the non-reversal and the reversal PIT effects are mediated by propositional processes, but that the reversal PIT effect was relatively *more* demanding than the non-reversal PIT effect. This is an intuitive assumption from a propositional perspective, because the reversed PIT effect requires participants to remember and integrate knowledge about both the trained instrumental contingencies *and* the reversal instruction. The non-reversal PIT effect, in contrast, only requires a recollection of the trained instrumental contingencies. Hence, it is possible that the concurrent load task was strong enough to abolish the reversed PIT effect, but not strong enough to abolish the less demanding non-reversal PIT effect. The non-reversal PIT effect may depend on only very limited propositional processes, which may explain why participants were able to generate a non-reversal PIT effect even whilst completing the load task. In sum, it is possible that the PIT effect persisted in the Non-Reversal Load condition because the load task reduced but did not *eliminate* participants’ ability to utilise controlled reasoning processes.

The analysis above implies that more demanding versions of the non-reversal PIT task would be more sensitive to the speed and load manipulations. Some support comes from the positive correlation observed between reaction times and the size of the transfer effect in Experiment 1. There was also a trend towards a comparable result in the Non-Reversal Load group in Experiment 2. Here, a marginal negative correlation was observed between accuracy on the load task and the size of the transfer effect. These correlations suggest that participants who responded either very quickly (Experiment 1) or very accurately on the load task (Experiment 2) were more likely to show an attenuated PIT effect. Hence, PIT effects were reduced when the speed and load manipulations were more effective. This lends credence to the idea that more demanding versions of the PIT task (perhaps using a procedure similar to de Wit et al., 2013) might be more effective in producing an effect of speed and load on the Non-Reversal PIT task. One way to test this idea empirically would be to increase the number of trained instrumental contingencies. The current experiments only required participants to learn two concurrent instrumental contingencies, which may not have been particularly demanding. Future experiments could, for instance, establish four instrumental contingencies, and then test whether PIT effects are more sensitive to concurrent load than when only two contingencies are trained. The propositional account predicts that the greater the number of contingencies, the more influence the concurrent load task should have. If the immunity of the non-reversal PIT effect to the concurrent load manipulation in Experiment 2 was due to the formation of automatic associative links, then the same result should be observed with a greater number of instrumental contingencies (assuming participants learn the contingencies).

Before concluding this discussion, it is also worth noting some interesting patterns in the reaction time data. In each experiment, the Non-Reversal group responded more quickly than the Reversal group to the beer and chocolate stimuli. This is akin to classic ideomotor effects (Elsner & Hommel, 2001). From an ideomotor perspective, the beer and chocolate stimuli may have primed the instrumental response that was paired with the common outcome during training. This priming effect may then have made it more challenging and time-consuming to perform the alternative response in line with the reversal instruction. Ideomotor effects are often assumed to operate automatically (Ridderinkhof, 2014), and without any intention to learn the action-effect relationships (Elsner & Hommel, 2001). However, this is still a matter of debate (Watson et al., 2016), and the reaction time effects observed here are also entirely consistent with the propositional account. The propositional account predicts that the reversed PIT effect would be more time-consuming than the non-reversed PIT effect because successful reversal requires participants to retain knowledge of both the original contingencies *and* the instructed relations. Hence, while the observation that the Non-Reversal group responded more quickly than the Reversal instruction group is interesting, it is consistent with both the ideomotor and the propositional accounts.

Finally, OSPAN scores (measuring working memory capacity) did not correlate with the transfer effect in either Experiment 1 or Experiment 2. OSPAN scores were, in general, rather low in both experiments. It is possible that floor effects reduced the potential to observe significant correlations between OSPAN scores and the PIT effect. In future, it would be useful to employ a less demanding test of working memory, to further explore the relationship between working memory capacity and PIT.

The speed and concurrent load procedures employed in Experiments 1 and 2 are useful for testing the dual-process model of PIT against the propositional model of PIT. The experiments had the potential to produce a non-reversal PIT effect in the Reversal Fast (Experiment 1) and Reversal Load (Experiment 2) groups. Such a result would have been extremely revealing, and would have provided unique evidence for a dual-process account of PIT. Unfortunately, the data did not support this conclusion. Future work could assess whether other concurrent load tasks are more successful in producing evidence for the dual-process account. The experiments in Chapter 3, however, move away from the speed and concurrent load manipulations, in the hope of establishing a more definitive test of the propositional and S-O-R accounts of PIT.

The other reason for moving away from the speed and concurrent load tasks employed in the present chapter is that they do not provide insight into one of the most counterintuitive aspects of PIT: its insensitivity to outcome devaluation (Colwill & Rescorla, 1990a; Corbit et al., 2007; Hogarth, 2012; Hogarth & Chase, 2011; Holland, 2004; Rescorla, 1994b; Watson et al., 2014). In particular, Hogarth and Chase (2011) and Hogarth (2012) have both demonstrated insensitivity of PIT to outcome devaluation using procedures that are very similar to the control conditions of the current experiments (without time restrictions or cognitive load). Hence, insensitivity to devaluation was demonstrated when participants should have been able to utilise higher order propositional processes. Insensitivity to devaluation is usually regarded as the definitive test for automatic or habitual control (de Wit & Dickinson, 2009; Dickinson, 1985). The standard PIT effect is therefore usually considered to be automatic (even though propositional processes should be able to readily operate), because it is insensitive to devaluation. In light of this counterintuitive result, several experiments that explored the effect of a very strong outcome devaluation manipulation on PIT were conducted. These experiments generated more immediately promising results than the speed and concurrent load experiments, so this line of research was prioritised. The experiments in Chapter 3 therefore change direction to further examine the effect of outcome devaluation on PIT.

Chapter 3: Outcome devaluation

3.1 Introduction

The previous chapters demonstrated an apparent paradox in the PIT literature. On the one hand, PIT is attenuated (Hogarth et al., 2014) and can be reversed (Experiment 2 of the current thesis) by verbal instructions. Sensitivity to verbal instructions is usually regarded as evidence for the role of controlled, propositional processes (e.g., De Houwer et al., 2005; Lovibond, 2003; Mitchell et al., 2009; Mitchell, Griffiths, Seetoo, & Lovibond, 2012). On the other hand, PIT is often insensitive to outcome devaluation manipulations (Colwill & Rescorla, 1990a; Corbit et al., 2007; Hogarth, 2012; Hogarth & Chase, 2011; Holland, 2004; Rescorla, 1994b; Watson et al., 2014, but see Allman et al., 2010; Eder & Dignath, 2016a, 2016b for exceptions). Insensitivity to devaluation is usually interpreted as evidence for automatic or habitual control (de Wit & Dickinson, 2009; Dickinson, 1985). These results are considered paradoxical because the instructional sensitivity implicates a controlled, propositional process, while the insensitivity to outcome devaluation points to an automatic process.

Before accepting that PIT is both sensitive to verbal instructions and insensitive to devaluation, it is important to confirm that both effects are robust. Experiment 2 provided evidence, in addition to that in the existing literature, to suggest that PIT is sensitive to instructional manipulations. The instructional sensitivity reported by Hogarth et al. (2014) and Seabrooke et al. (2016) therefore appears to be secure. However, there is still ambiguity with respect to the insensitivity to devaluation that is usually observed in PIT experiments (Allman et al., 2010; Eder & Dignath, 2016a, 2016b). In particular, Eder and Dignath (2016b) have argued that PIT may only be insensitive to devaluation when weak devaluation procedures are used. Their explanation would resolve the apparent paradox very simply, by suggesting that PIT is in fact sensitive to strong devaluation manipulations – a conclusion that would be entirely consistent with the observed sensitivity to verbal instructions.

Eder and Dignath's (2016b) argument was based on two experiments in which participants learnt to perform two instrumental responses to earn different lemonade drinks. Pavlovian stimuli were also trained to predict each lemonade outcome, before participants sampled each outcome[4]. Importantly, one outcome was devalued by mixing it with a substance (Tween 20) to make it taste unpleasant. The post-devaluation transfer test was divided into two blocks. In Experiment 1, participants were required to drink their (valued and devalued) lemonade winnings immediately after each block. Crucially, the PIT effect was sensitive to the devaluation manipulation; the Pavlovian cues increased instrumental responding for the common outcome when that outcome was valued, but not when the outcome had been devalued. As Eder and Dignath (2016b) noted, however, their procedure was somewhat atypical. Usually, participants are either not given the rewards “earned” during the transfer test (Hogarth, 2012; Hogarth & Chase, 2011), or they are given the rewards at the very end of the transfer test (Watson et al., 2014). Eder and Dignath (2016b) therefore ran another experiment that was identical to the first, except that the transfer test winnings were simply bottled for participants to take away. Under these circumstances, PIT effects were observed for both the valued *and* devalued outcome. Hence, PIT was only insensitive to devaluation when participants were not required to consume the devalued outcomes immediately after each block of the transfer test. Eder and Dignath (2016b) therefore made a strong argument that PIT is only insensitive to weak devaluation procedures.

Eder and Dignath's (2016b) data suggest that PIT can be sensitive to outcome devaluation. Their results are also consistent with some other recent observations in the

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[4] A typical PIT effect was also observed in a pre-devaluation transfer test.

human literature (Allman et al., 2010; Eder & Dignath, 2016a). There are, however, many procedural differences between these experimental tasks and those that demonstrated insensitivity to outcome devaluation. For example, Eder and Dignath (2016a, 2016b) explicitly instructed their participants that the Pavlovian stimuli were not important during the transfer test. This is arguably an instructed Pavlovian extinction procedure, which may have made the PIT effect more susceptible to the devaluation manipulation.

Eder and Dignath's (2016a, 2016b) instruction is particularly noteworthy for the propositional EU account of PIT. Here, PIT effects are assumed to reflect explicit judgements about the expected probability (Op) and value (Ov) of each outcome. The Pavlovian stimuli presented during the transfer test are suggested to increase the perceived probability Op of the associated outcome. This increase in perceived probability Op is assumed to underlie both the standard PIT effect and its insensitivity to outcome devaluation; participants respond for the devalued outcome when it is cued because it is considered to be much more available than the alternative (non-cued) outcome. Notably, the EU account assumes that the probability judgements are propositional. One might expect, therefore, that Eder and Dignath's (2016a, 2016b) instruction – that the pictures were not relevant to which response would be rewarded – would influence (and perhaps discredit) these propositional judgements about cue-elicited outcome probability. Hence, it is possible that a typical PIT task would produce insensitivity to devaluation with even a very strong devaluation procedure.

In light of the above discussion, Experiment 3 tested whether PIT would be sensitive to a very strong devaluation manipulation (described below), using a procedure that is otherwise very similar to those that have demonstrated insensitivity to outcome devaluation (Hogarth, 2012; Hogarth & Chase, 2011). Participants were not told that the pictures were irrelevant during the transfer test. Sensitivity to devaluation would be revealed if the PIT effect for the still-valued outcome was significantly larger than the PIT effect for the devalued outcome. Such a result, using the current very strong devaluation procedure, would support the suggestion that the previous demonstrations of insensitivity to outcome devaluation were simply due to the use of weak devaluation procedures (Eder & Dignath, 2016b). Insensitivity to devaluation, by contrast, would be demonstrated if both the valued and devalued Pavlovian stimuli biased response choice towards the associated outcome (relative to the neutral stimulus) to a similar extent. Such insensitivity to devaluation would be more consistent with the majority of the literature, and would support the claim that outcome value plays no role in outcome-selective PIT (e.g. Hogarth et al., 2013; Hogarth & Chase, 2011; Martinovic et al., 2014).

3.2 Experiment 3

Experiment 3, which is summarised in Table 3.1, tested whether PIT would be sensitive to a novel, very strong outcome devaluation procedure. Participants were first shown a bag of crisps and popcorn (outcomes O1 and O2, counterbalanced) and were told that they could win points corresponding to each type of food during the experiment. Savoury food outcomes (as opposed to the beer and chocolate props used in Experiments 1 and 2) were used because pilot testing had revealed that they worked particularly well with the devaluation manipulation. As in Experiments 1 and 2, participants then learned to perform two instrumental responses to obtain each outcome. Each response was selectively paired with one outcome (R1-O1, R2-O2). The outcome devaluation procedure took place between the instrumental training and transfer test phases. Here, participants sampled each outcome, but importantly, the devalued was covered with ground cloves and olive oil. This procedure made the devalued outcome taste very unpleasant. In the final transfer test, instrumental response choice (R1 versus R2) was tested in the presence of pictorial stimuli that were associated with each outcome (stimulus S1, S2, or a neutral stimulus S0).

Table 3.1

Design of Experiment 3.

Instrumental training	Outcome devaluation	Transfer test
R1 – O1	O1 or O2 devalued	S0: R1/R2?
R2 – O2		S1: R1/R2?
		S2: R1/R2?

Note: R1 and R2 represent instrumental responses (left and right arrow key presses). O1 and O2 are outcomes (crisps and popcorn). S0 is a neutral stimulus, and S1 and S2 are pictures of O1 and O2.

Overall response choice was expected to be biased towards the still-valued outcome during the transfer test. This bias would be consistent with previous reports in demonstrating that overall responding is goal-directed (e.g., Eder & Dignath, 2016a, 2016b; Hogarth, 2012; Hogarth & Chase, 2011; Watson et al., 2014). The bias towards the valued outcome was also expected to be enhanced by the stimulus that was associated with that valued outcome. The question was whether the stimulus that signalled the devalued outcome would increase responding for that outcome, relative to the neutral stimulus. Insensitivity to outcome devaluation would be revealed if the stimulus signalling the devalued outcome elevated instrumental responding for that outcome, compared to the neutral stimulus. Moreover, the PIT effect for the devalued outcome should be of a comparable magnitude to the PIT effect for the still-valued outcome. Sensitivity to outcome devaluation, by contrast, would be revealed if the PIT effect for the valued outcome was significantly larger than the PIT effect for the devalued outcome.

3.2.1 Method

Participants. Sixty participants (49 females, aged between 18 and 30; *M* =21.41 years, *SEM* = 0.39 years) were recruited from Plymouth University and received either £4 or course credit for participation. Participants were screened for food allergies and intolerances at the start of the experiment. All other aspects were identical to Experiment 1.

Apparatus and materials. A bag of Walkers extra crunchy ready salted crisps (150g) and Tyrrell's sea salted "Poshcorn" (70g) served as visual props. These brands were also used for the devaluation manipulation. Here, the outcomes were decanted into separate transparent, plastic containers before the experiment. The name of the food was written clearly on the lid of each container. For the devalued outcomes, ground cloves were combined with olive oil (11 grams oil per 5 grams cloves) to form a paste that was brushed heavily onto the devalued food (Appendix 1 details the precise amounts used for each food). The non-devalued outcome was simply transferred from the original packaging to its container.

A picture of crisps or popcorn (depicting the outcomes in their valued state) served as Pavlovian stimuli during the transfer test. Hence, as in Experiments 1 and 2, pictorial stimuli whose Pavlovian relationships were established outside of the laboratory were used (Hogarth & Chase, 2011). This procedure has the advantage of producing robust and replicable effects, at the expense of full experimental control of the Pavlovian contingencies. To minimise potential problems regarding the picture stimuli, a Pavlovian knowledge test was administered at the end of the experiment to ensure that all participants knew which outcome the stimuli represented. All other aspects of the apparatus and materials were identical to Experiment 1.

Procedure. Participants were warned before the experiment that they would be required to sample foods during the experiment, that the foods might not match the participants' expectations, and that they might taste unpleasant. Participants provided informed consent and signed a form stating that they had no allergies or intolerances. The crisps and popcorn food props were presented, and participants were told they could win points towards the foods during the experiment.

Liking ratings. Participants initially rated their desire to eat each food (based on the food props) by pressing a key between one ("Not at all") and seven ("Very much"). The foods were rated in a random order and were separated by an interval that varied randomly between 750 and 1250ms.

Instrumental training. The instrumental training phase followed a very similar procedure to the training used in Experiments 1 and 2. The props were removed and the experimenter read aloud the following instructions: "In this task, you can earn the two outcomes shown before by pressing the left or right arrow keys. Your task is to learn which keys earn each outcome." There were 48 trials. Each trial began with a centrally presented choice symbol ("← or →"), which remained until participants pressed either the left or right arrow key. Each key was selectively paired with either crisps or popcorn, and this was counterbalanced between-subjects. The keys were also counterbalanced with respect to whether they earned the subsequently valued or devalued outcome. One outcome was scheduled to be available on each trial (availability of O1 or O2 on any given trial was random), and so each key had a 50% chance of yielding the associated reward. Instrumental responses were followed by the statement, "You earn one [CRISPS/POPCORN] point", or "You earn NOTHING" if the available outcome was not selected. The outcome (crisps/popcorn/nothing) was presented in bold text and the rewards (crisps/popcorn) were presented in green or red (counterbalanced) to help participants discriminate between them. All other text was presented in black. Feedback was presented for 3000ms and the trials were separated by 750-1250ms intervals.

Instrumental knowledge test. After instrumental training, participants completed an instrumental knowledge test that was identical to Experiment 1, except that the questions related to the crisps and popcorn outcomes. Confidence ratings were also recorded (1 = “Not at all”, 7 = “Very much”).

Outcome devaluation. At the start of the devaluation procedure, participants were informed that they would have the opportunity to try the foods that were available for the rest of the experiment. The still-valued outcome was always sampled first, before the devalued outcome was revealed. After sampling both outcomes, the containers were placed on the table together, and participants were informed that the devalued outcome was past its expiry date. Liking ratings were then taken in the same way as at the start of the experiment.

Transfer test. At the start of the transfer test, the experimenter read aloud the following instructions: “In this part of the task, you can earn the two outcomes by pressing the left or right arrow key in the same way as before. You will only be told how many of each reward you have earned at the end of the experiment. Also, sometimes pictures of the foods will be presented before you choose the left or right arrow key. NOTE: You will be required to eat all of the food you have earned at the end of the experiment, so please choose carefully. Press any key to begin a practice round.” A crisps, popcorn or neutral stimulus was presented at the start of each trial for 3000ms. The choice symbol (“← or →”) then appeared beneath the stimulus and remained until participants selected the left or right arrow key. The test phase was conducted in nominal extinction, and so no feedback was given. The trials were separated by 750-1250ms intervals. There were eight cycles of six trials (48 trials in total). In each cycle, the three stimuli were presented twice in a random order. Before the transfer test, participants completed one practice cycle to provide time to adjust to the task. The practice cycle was identical to the test cycles but the data were not analysed. After the practice cycle, the following instructions were presented: “That is the end of the practice phase. Please ask the experimenter now if you have any questions. REMEMBER: You will be required to eat all of the food you have earned at the end of the experiment, so please choose carefully. Press any key to begin.” The experimenter read aloud the instructions, answered any questions and removed the food containers before participants finished the transfer test.

Knowledge tests. Participants completed a second instrumental knowledge test (identical to the first) to check their knowledge of the instrumental relationships. A Pavlovian knowledge test was also administered. Here, the crisps and popcorn stimuli were presented in a random order and participants selected the outcome that the stimulus represented. A post-experimental questionnaire was also used to collect demographic information and feedback about the outcomes. Finally, participants were fully debriefed and were asked not discuss the experiment outside the laboratory.

3.2.2 Results

Exclusions. Ten participants were excluded for failing either the instrumental (N = 8) or Pavlovian (N = 2) knowledge tests. Given the relatively high proportion of excluded participants, the mean transfer test results for these participants are provided in Appendix 2. The data from the remaining 50 participants were entered into the analyses.

Liking ratings. Figure 3.1 shows the mean liking ratings for each outcome, in the pre- and post-devaluation liking tests. There was a main effect of liking test, with higher liking ratings given in the pre-devaluation test than the post-devaluation test, F (1, 49) = 48.97, $p < .001$, $\eta_p^2 = .50$. There was also a main effect of outcome, with the valued outcome receiving higher ratings than the devalued outcome when collapsed across the liking tests, F (1, 49) = 237.95, $p < .001$, $\eta_p^2 = .83$. Most importantly, there was a significant interaction between the liking test and outcome variables, F (1, 49) = 313.47, $p < .001$, $\eta_p^2 = .87$. Planned pairwise comparisons demonstrated that the outcomes were rated similarly before devaluation, $t < 1$, but the valued outcome received much higher liking ratings than the devalued outcome after devaluation, t (49) = 29.01, $p < .001$, 95% CI = [4.62, 5.30].

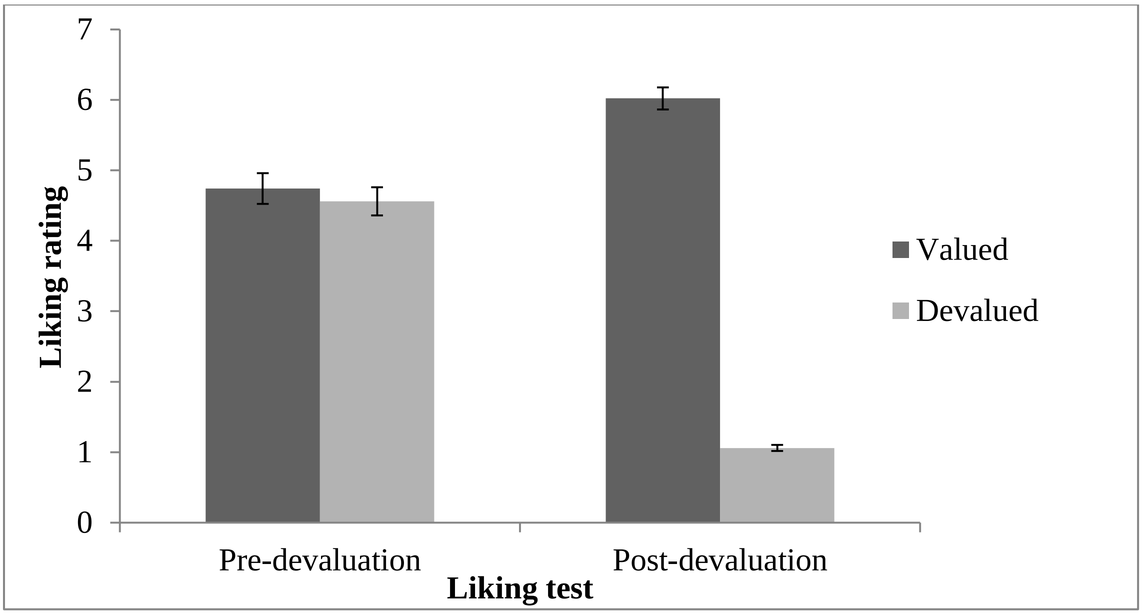

Figure 3.1. Mean liking ratings in Experiment 3. Ratings were taken for each outcome (valued, devalued) at the start of the experiment (pre-devaluation), and immediately after the devaluation procedure (post-devaluation). Ratings of one and seven represent wanting to eat the outcome "Not at all" and "Very much", respectively. Error bars represent SEM.

Transfer test. Figure 3.2 shows the transfer test results. There was an overall effect of stimulus, F (2, 98) = 18.42, $p < .001$, $\eta_p^2 = .27$. Bonferroni corrected pairwise comparisons revealed an outcome-selective PIT effect. The stimulus signalling the valued outcome biased responding towards that outcome compared to the neutral stimulus, t (49) = 4.21, $p < .001$, 95% CI = [7.80, 30.20], and the stimulus that signalled the devalued outcome, t (49) = 5.33, $p < .001$, 95% CI = [17.13, 46.87]. Importantly, the stimulus that signalled the devalued outcome decreased responding for the valued outcome, relative to the neutral stimulus, *t* (49) = 2.46, *p* = .05, 95% CI = [-0.11, 26.11]. Finally, a one-sample *t*-test demonstrated that overall response choice (averaged across the three stimuli) was biased towards the still-valued outcome, *t* (49) = 8.68, *p* < .001, 95% CI = [20.18, 32.32].

To compare the magnitude of the PIT effects, PIT scores were calculated in a similar way to Experiments 1 and 2. For each outcome, the dependent variable was the percent choice of the instrumental response that was paired with the still-valued outcome. The PIT score for the valued outcome was calculated by subtracting choice on the neutral stimulus trials from choice on trials where the stimulus signalling the valued outcome was present ($S_{valued} – S_{neutral}$). This calculation was reversed for the devalued outcome ($S_{neutral} – S_{devalued}$). Most importantly, the PIT effect scores for the valued and devalued outcome did not significantly differ, *t* < 1.

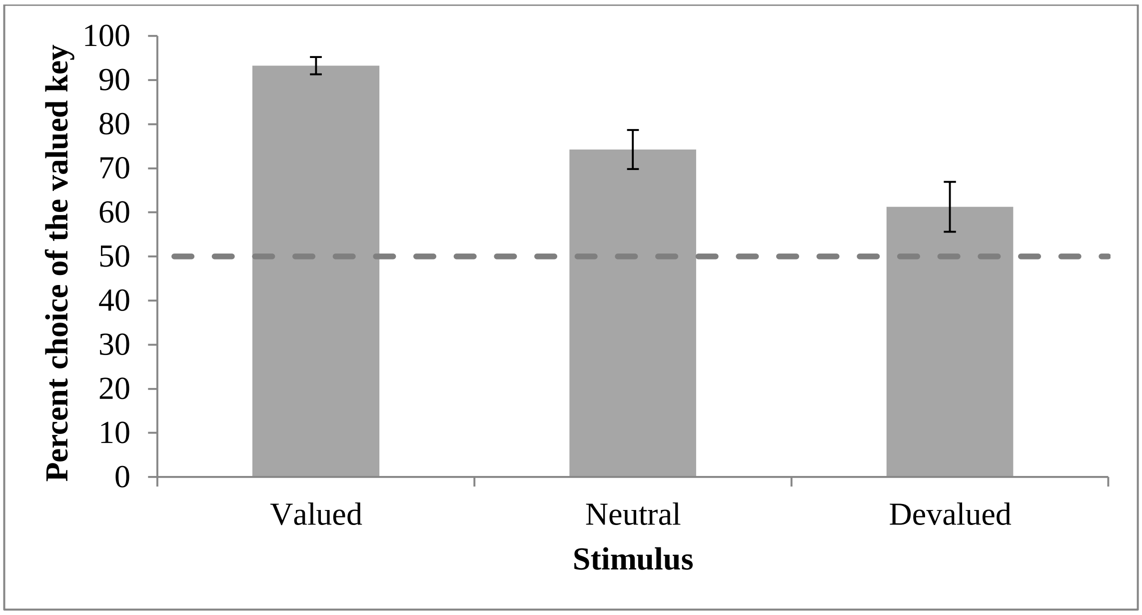

Figure 3.2. Transfer test results of Experiment 3. Response choice was tested in the presence of pictorial stimuli depicting each outcome, or a neutral stimulus. Scores above and below the 50% mid-point represent a bias towards the valued and devalued outcome, respectively. Error bars represent SEM.

3.2.3 Discussion

Experiment 3 tested whether PIT would be insensitive to a strong outcome devaluation procedure. Overall, response choice was biased towards the still-valued outcome during the transfer test. This indicates that overall response choice was goal-directed, because it clearly reflected an integration of knowledge about the instrumental contingencies, and the current outcome values (de Wit & Dickinson, 2009; Dickinson, 1985). A PIT effect was also observed; the Pavlovian stimuli increased responding for their associated outcomes relative to the neutral stimulus. Importantly, this effect was not diminished by the devaluation manipulation. Thus, the PIT effect was “insensitive” to devaluation, because the stimuli exerted a similar effect on instrumental response choice, irrespective of whether the associated outcome had been devalued or not. This insensitivity to devaluation is consistent with much of the literature (Colwill & Rescorla, 1990a; Corbit et al., 2007; Hogarth, 2012; Hogarth & Chase, 2011; Holland, 2004; Rescorla, 1994b; Watson et al., 2014), and suggests that Pavlovian stimuli can, at least under these circumstances, motivate instrumental responding for undesirable or aversive outcomes.

As noted earlier, there are three recent human PIT studies that have reported sensitivity to devaluation (Allman et al., 2010; Eder & Dignath, 2016a, 2016b). The current results are clearly inconsistent with these studies. Eder and Dignath (2016b) recently proposed that PIT may only be insensitive to weak devaluation procedures. Their conclusion was based on the observation that PIT was sensitive to outcome devaluation when participants consumed the devalued outcome periodically throughout the transfer test, but not when the outcomes were given away after the experiment. When consumption of the devalued outcome was delayed (and potentially avoided entirely), the Pavlovian stimulus that signalled the devalued outcome increased instrumental responding for that devalued outcome compared to a neutral stimulus. Our participants did not consume the outcomes after the experiment for ethical reasons, which may explain the observed insensitivity to devaluation. However, the instructions *did* emphasise that participants would be required to eat all of the food earned after the experiment, and participants had no way of knowing that this was a deception[5]. Furthermore, the liking rating data suggested that the devaluation manipulation produced a very strong reduction in liking for the devalued outcome. This aversion was also apparent in the transfer test, where participants demonstrated a strong overall bias towards the still-valued outcome. The insensitivity to outcome devaluation observed in the current experiment does not, therefore, appear to be for lack of a strong devaluation procedure. Clearly, there is a tendency for Pavlovian stimuli to facilitate instrumental responses for associated outcomes, even when those outcomes are of very low value. It seems likely, therefore, that the recent demonstrations of sensitivity to devaluation arose from other procedural differences. Possible procedural differences are explored more thoroughly in section 3.6 (General Discussion).

Insensitivity to outcome devaluation in PIT experiments is usually taken as evidence of automaticity, because PIT is not seemingly flexible to changes in outcome value. It accords particularly well with S-O-R theory, which suggests that PIT effects occur automatically and without retrieving a representation of the outcome’s value (e.g., Hogarth et al., 2013; Hogarth & Chase, 2011; Holland, 2004). However, S-O-R theory has difficulty explaining the sensitivity to verbal instructions observed previously (Hogarth et al., 2014; Seabrooke et al., 2016). Thus, the issue of why PIT is sensitive to instructional manipulations yet insensitive to outcome devaluation remains. The

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[5] Participants were also explicitly asked not to discuss the experiment (and the devaluation procedure specifically) with other potential participants during the debriefing session. It is very difficult to check compliance with respect to this request, but steps were nevertheless taken to minimise the possibility of participants discussing the experiment with others outside of the laboratory.

following experiments therefore aim to further examine the conditions that foster insensitivity to outcome devaluation in PIT.

3.3 Experiment 4

An overarching theme of this thesis is to reconcile PIT's insensitivity to devaluation with its sensitivity to verbal instructions. Experiment 2 provided additional evidence demonstrating PIT's sensitivity to instructional manipulations. Experiment 3 demonstrated that the devaluation effect is also robust. Experiment 4 therefore aimed to show both effects within a single experiment. To this end, the experiment first sought evidence of an 'instructed' PIT effect. That is, the instrumental contingencies were simply instructed rather than trained. An instructed PIT effect would demonstrate that PIT effects can be produced by instructions alone, and would therefore support the propositional approach. Experiment 4 also tested whether an instructed, propositional PIT effect would be sensitive to outcome devaluation. If, like the standard PIT effect, the instructed effect is insensitive to outcome devaluation, then insensitivity to outcome devaluation can no longer only be interpreted as evidence for an automatic S-O-R process. Rather, it would suggest that insensitivity to devaluation can (at least sometimes) be produced by propositional processes.

The search for an instructed PIT effect fits with a broader theme in associative learning, where evidence for propositional processes is sought by exploring whether effects that are traditionally developed through experience (conditioning) can be obtained through verbal instructions alone (e.g., Cook & Harris, 1937; De Houwer, 2006; Lovibond, 2003; Mertens, Raes, & De Houwer, 2016). Verbal instructions are unlikely to produce associative links, because they do not involve the repeated, contiguous activation of two mental representations. In contrast to the associative link-based account, the propositional account predicts that effects derived from experience or instructions should be comparable.

Experiment 4 tested whether a PIT effect would be observed when the instrumental contingencies were simply instructed rather than established through trial-by-trial conditioning. Table 3.2 shows the design. Two instrumental responses were initially paired with crisps and popcorn points, but the contingencies were instructed rather than trained (R1-O1, R2-O2). Participants then completed a transfer test, where response choice was tested in the presence of a crisps, popcorn or neutral stimulus. The demonstration of a PIT effect would suggest that PIT effects can be generated by the propositional system alone. One outcome was then devalued, to test whether the instructed PIT effect would be insensitive to devaluation. Response choice was finally tested in a second, post-devaluation transfer test that was identical to the first. Both transfer tests were conducted in nominal extinction so that instrumental associative links could not form during either transfer test (because the instrumental contingencies were never experienced). If an instructed PIT effect is observed and is insensitive to outcome devaluation, it would suggest that insensitivity to outcome devaluation can, at least sometimes, reflect a propositional process.

Table 3.2

Design of Experiment 4

Instructed instrumental	Pre-devaluation transfer test	Outcome devaluation	Post-devaluation transfer test
R1 – O1	S0: R1/R2?	O1 or O2 devalued	S0: R1/R2?
R2 – O2	S1: R1/R2?		S1: R1/R2?
	S2: R1/R2?		S2: R1/R2?

Note: R1 and R2 represent instrumental responses (left and right arrow keys) that were verbally instructed at the start of the experiment. O1 and O2 refer to outcomes (crisps and popcorn). S0 represents a neutral stimulus, and S1 and S2 refer to pictures of O1 and O2, respectively.

3.3.1 Method

The method was identical to Experiment 3, except in the following respects.

Participants. Twenty-four undergraduate psychology students (18 females, aged 18-28; *M* = 20.00 years, *SEM* = 0.51 years) completed the experiment for course credit.

Procedure

Pre-devaluation transfer test. After completing initial liking ratings for each outcome, participants were instructed to press the left and right arrow keys to win crisps and popcorn points. In place of the instrumental training given in Experiment 1, the instrumental contingencies were simply presented on-screen and verbally confirmed. These instructed contingencies were counterbalanced with respect to the outcome (crisps, popcorn) that the response produced, and whether the outcome was subsequently devalued or not. The instructions were framed as a response-outcome contingency (e.g. LEFT ARROW = CRISPS) or an outcome-response contingency (e.g. CRISPS = LEFT ARROW) for half of the participants each. The presentation of the instruction was not intended to be an experimental manipulation. By counterbalancing the instructions in this way, we simply hoped to control for any underlying bias generated by the presentation of the instructions.

The transfer test was identical to Experiment 3, except that the instructed instrumental contingencies were presented at the bottom of the screen throughout. Each instruction was presented in either red or green (counterbalanced) to help discriminate them. After the pre-devaluation transfer test, instrumental knowledge was tested as in Experiment 3.

***Outcome devaluation*.** The outcome devaluation procedure was identical to Experiment 3, except that the devalued outcome was not described as past its expiry date. Many of the participants in Experiment 3 reported that they did not believe this instruction, so it was omitted from the procedure.

Post-devaluation transfer test and knowledge tests. The second, post-devaluation transfer test was identical to the first. Participants subsequently completed the instrumental and Pavlovian knowledge tests, and the post-experimental questionnaire from Experiment 3.

3.3.2 Results

Exclusions. One participant was excluded for failing both the instrumental and Pavlovian contingency knowledge tests.

Liking ratings. Figure 3.3 shows the mean liking ratings for each outcome at the start of the experiment (pre-devaluation) and after the devaluation procedure (post-devaluation). The pattern was very similar to that of Experiment 3. There was a main effect of liking test, with higher ratings given in the pre-devaluation test than in the post-devaluation test, F (1, 22) = 26.79, $p < .001$, $\eta_p^2 = .55$. A main effect of outcome was also observed, with the valued outcome receiving higher ratings than the devalued outcome, F (1, 22) = 109.39, $p < .001$, $\eta_p^2 = .83$. Most importantly, there was an interaction between the liking test and outcome variables, F (1, 22) = 81.13, $p < .001$, $\eta_p^2 = .79$. Planned pairwise comparisons revealed that the valued outcome was rated more highly than the devalued outcome in both the pre-devaluation, t (22) = 2.91, $p < .01$, 95% CI = [0.20, 1.19][6], and post-devaluation liking test, t (22) = 11.39, $p < .001$, 95% CI = [3.70, 5.35].

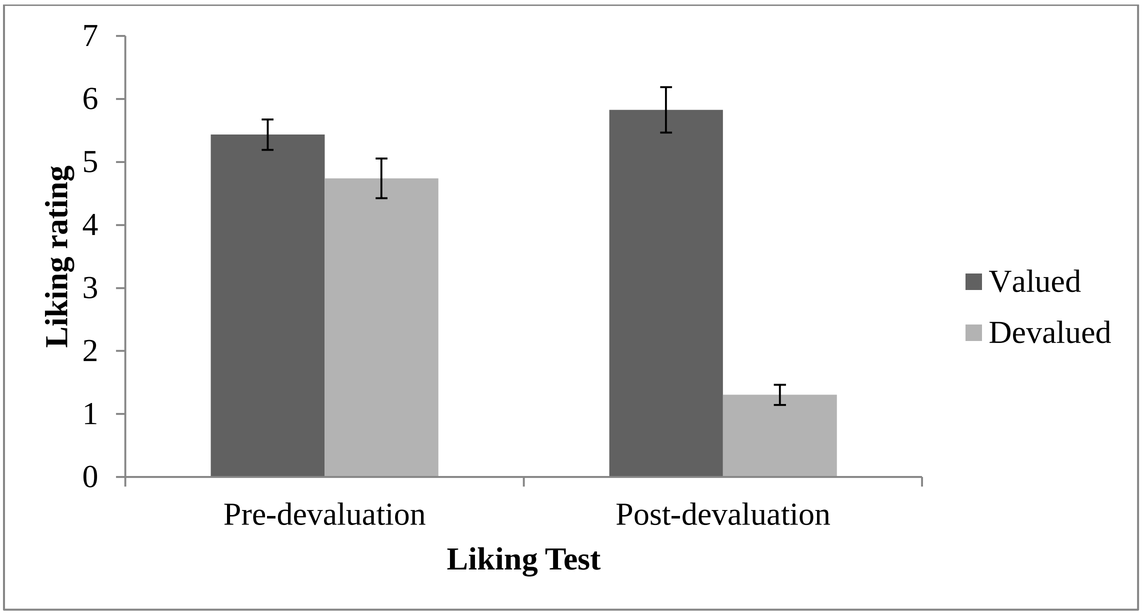

Figure 3.3. Mean liking ratings in Experiment 4. Ratings were taken for each outcome (valued, devalued) at the start of the experiment (pre-devaluation), and immediately after the devaluation procedure (post-devaluation). Ratings of one and seven represent wanting to eat the outcome "Not at all" and "Very much", respectively. Error bars represent SEM.

Transfer tests. Figure 3.4 shows the pre- and post-devaluation transfer test results. The graph indicates that a PIT effect was apparent in the pre-devaluation transfer test, where the stimuli increased choice of the instrumental responses that had been instructed to produce the cued outcomes. Overall response choice was biased towards the valued outcome in the post-devaluation transfer effect, but a PIT effect for the devalued outcome was still apparent.

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[6] The preference towards the valued outcome in the pre-devaluation liking test was unexpected, and is therefore discussed more thoroughly below.

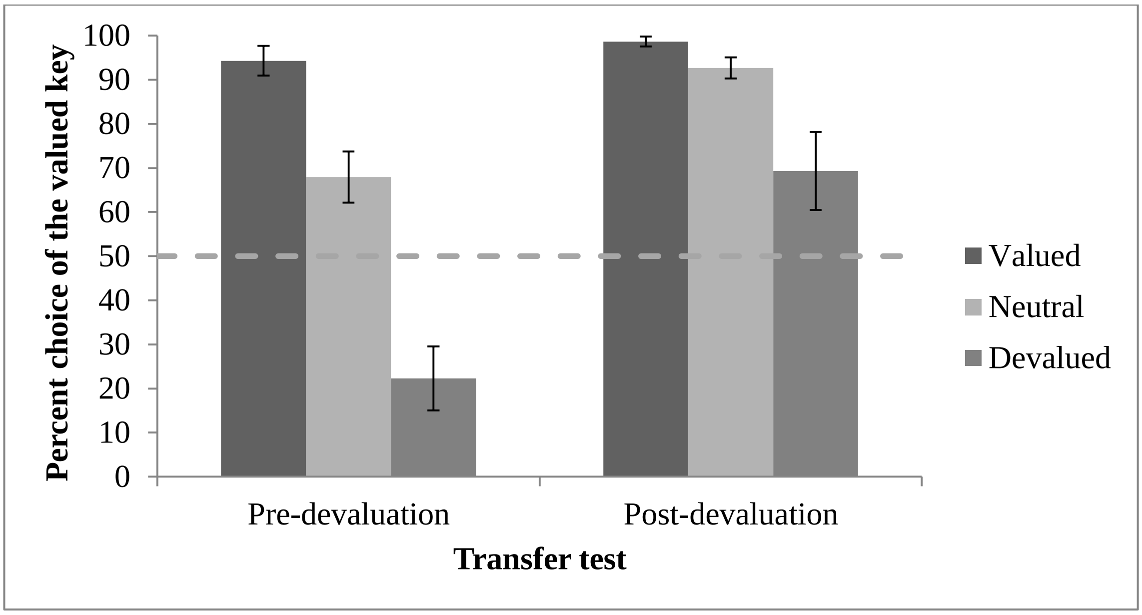

Figure 3.4. Pre- and post-devaluation transfer test results of Experiment 4. Response choice was tested in the presence of pictorial stimuli depicting each outcome, or a neutral stimulus. Scores above and below the 50% mid-point represent a bias towards the valued and devalued outcome, respectively. Error bars represent SEM.

The results were analysed using a repeated measures ANOVA on the transfer test (pre-devaluation, post-devaluation), stimulus (valued, neutral, and devalued) and instruction (O-R, R-O) variables. The instruction variable was not expected to influence the pattern of results, but it was nevertheless included in the analysis. To confirm, there was no main effect of instruction or any significant interactions with the transfer test and stimulus variables (*F*s < 1.37, *p*s > .26). The instruction groups were therefore collapsed for presentation. However, the means and standard deviations for each instruction group are provided in Appendix 3: for transparency. Greenhouse-Geisser corrections were applied where appropriate to correct for sphericity.

There was a main effect of transfer test, with participants responding more for the valued outcome after devaluation than before, F (1, 21) = 54.89, $p < .001$, $\eta_p^2 = .72$. There was also a main effect of stimulus, F (1.16, 24.27) = 42.64, $p < .001$, $\eta_p^2 = .67$, and a significant stimulus × transfer test interaction, F (1.33, 27.91) = 11.46, $p = .001$, $\eta_p^2 = .35$.

Bonferroni corrected pairwise comparisons revealed that, in the pre-devaluation transfer test, the stimulus signalling the subsequently still-valued outcome increased responding for that outcome compared to the neutral stimulus, *t* (22) = 4.66, *p* < .001, 95% CI = [11.76, 41.46] , and the stimulus that signalled the subsequently devalued outcome, *t* (22) = 8.47, *p* < .001, 95% CI = [5.01, 94.31]. The stimulus that signalled the subsequently devalued outcome also increased responding for that outcome compared to the neutral stimulus, *t* (22) = 6.13, *p* < .001, 95% CI = [26.22, 64.88]. Thus, a PIT effect was observed in the pre-devaluation transfer test. The magnitude of the two PIT effects was not significantly different, *t* (22) = 1.92, *p* = .07, 95% CI = [-1.58, 40.16].

Comparable analyses in the post-devaluation transfer test revealed that the stimulus that signalled the valued outcome increased responding for that outcome compared to the neutral stimulus, *t* (22) = 2.69, *p* < .05, 95% CI = [0.19, 11.75], and the stimulus that signalled the devalued outcome, *t* (22) = 3.25, *p* < .05, 95% CI = [5.90, 53.24]. Importantly, the stimulus that signalled the devalued outcome also increased responding for that devalued outcome compared to the neutral stimulus, *t* (22) = 3.03, *p* < .05, 95% CI = [3.36, 43.85]. Thus, a PIT effect was observed for both the valued and the devalued outcome. Interestingly, the PIT effect for the devalued outcome was significantly larger than the PIT effect for the valued outcome, *t* (22) = 2.53, *p* = .02, 95% CI = [3.11, 31.67].

The use of a pre-devaluation transfer test in the current experiment also allows a comparison of the size of the transfer effects before and after the devaluation procedure. The transfer effect scores for the valued and devalued outcomes were calculated in the same way as in Experiment 3. For the valued outcome, the size of the pre-devaluation transfer effect (*M* = 26.36, *SEM* = 5.71) was significantly larger than the post-devaluation transfer effect (*M* = 5.98, *SEM* = 2.17), *t* (22) = 3.74, *p* = .001, 95% CI = [9.08, 31.78]. For the devalued outcome, the size of the pre-devaluation (*M* = 45.65, *SEM* = 7.27) and post-devaluation (*M* = 23.37, *SEM* = 7.68) transfer effect did not significantly differ, *t* (22) = 2.01, *p* = .06, 95% CI = [-0.76, 45.33].

Finally, one-sample *t*-tests revealed that, in the pre-devaluation transfer test, overall response choice was biased towards the outcome that subsequently served as the valued outcome, *t* (22) = 2.94, *p* < .01, 95% CI = [3.39, 19.62]. This is consistent with the higher liking ratings given to this outcome at the start of the experiment (despite counterbalancing the outcomes). Overall responding was also biased towards the valued outcome in the post-devaluation transfer test, *t* (22) = 10.49, *p* < .001, 95% CI = [29.58, 44.16].

3.3.3 Discussion

Experiment 4 explored whether trial-by-trial experience of instrumental contingencies is necessary to observe a PIT effect, by investigating whether PIT could be produced by instruction in the absence of experience. The experiment also tested whether an instructed PIT effect would be insensitive to an outcome devaluation manipulation. In the pre-devaluation transfer test, participants demonstrated a clear instructed PIT effect; the pictorial crisps and popcorn stimuli selectively biased response choice towards the response that had been instructed to produce the outcome that was signalled by the stimulus. This result suggests that PIT effects can be produced by the propositional system alone. It is also consistent with the suggestion that explicit contingency knowledge plays an important role in generating PIT effects (Bezzina et al., 2016; Hogarth et al., 2007; Lovibond et al., 2015; Talmi et al., 2008).

In the post-devaluation transfer test, overall response choice was strongly biased towards the response that was instructed to produce the still-valued outcome. A PIT effect was still observed for the devalued outcome though: responding for the devalued outcome was enhanced by the stimulus depicting that outcome, relative to the neutral stimulus. A PIT effect in the case of an instructed response for a devalued outcome is interesting, because insensitivity to outcome devaluation is usually regarded as evidence for automatic or habitual control (de Wit & Dickinson, 2009; Dickinson, 1985). Instructed contingencies, on the other hand, are most naturally explained by the operation of controlled, propositional processes (Mitchell et al., 2009). Hence, the result is paradoxical; although response choice was most likely governed by propositional beliefs – the instrumental contingencies were only ever instructed – a PIT effect was still observed for the devalued outcome. It seems that insensitivity to devaluation may not always reflect automaticity, but may instead (at least sometimes) reflect a controlled reasoning process. This possibility is further explored in section 3.6 (General Discussion).

In contrast to Experiment 3 (and the other published PIT experiments that have demonstrated insensitivity to devaluation), the current experiment included a pre-devaluation transfer test. This allows a direct comparison of the size of the PIT effect for each outcome before and after devaluation. The size of the PIT effect for the valued outcome was smaller after devaluation than before devaluation. There was also a non-significant trend in the same direction for the devalued outcome. Hence, it seems possible that, even though the sizes of valued and devalued PIT effects were not significantly different in the post-devaluation transfer test, the PIT effects were in fact influenced by the devaluation manipulation (see Eder & Dignath, 2016a, 2016b for similar results). It also seems likely that the size of the PIT effect for the valued outcome in the post-devaluation transfer test was reduced because response choice approached ceiling (complete responding for the valued outcome). This possibility will be expanded on in Section 3.6 (General Discussion).

Finally, the valued outcome was preferred to the devalued outcome in even the pre-devaluation liking test. This result was unexpected, because the pre-devaluation liking ratings pertained to the non-devalued outcomes that were presented in their original packaging. In all of the devaluation experiments reported in this thesis, great care was taken to ensure that the devalued and non-devalued foods were not revealed prior to the devaluation procedure. The devaluation foods were kept out of sight and in a separate room. Furthermore, the outcomes served as valued and devalued outcomes in an alternate, between-subjects fashion, precisely to prevent a systematic bias towards either outcome. In the unlikely event that participants saw the outcomes prematurely, they would have consequently seen both the valued and devalued forms of each outcome. Another possibility is that the outcome values became known through word of mouth outside of the laboratory (despite explicit requests to prevent this during the debriefing). Again, the alternate counterbalancing of the outcomes should have prevented this from inducing an overall pre-devaluation bias towards either outcome. In sum, there is no obvious answer to explain the pre-devaluation preference towards the valued outcome. However, it does not affect the critical result – that an instructed PIT effect was observed for a devalued outcome –so it will not be discussed here further.

3.4 Experiment 5

Experiment 4 demonstrated a PIT effect for a devalued outcome, when the instrumental response was merely instructed to produce that outcome. The first objective of Experiment 5 was to replicate the results of Experiment 4. The second objective was to test whether the insensitivity to outcome value in the new instructed PIT effect is comparable to that seen in the standard PIT effect. If both the trained and instructed PIT effects are mediated by a common (propositional) mechanism, then outcome devaluation should have a similar impact on each effect. If the PIT effect following trial-by-trial instrumental training is partly due to an automatic S-O-R mechanism, however, then less sensitivity (greater insensitivity) to outcome devaluation might be seen in this condition. Table 3.3 shows the design.

Table 3.3

Design of Experiment 5

Trained instrumental	Instructed instrumental	Outcome devaluation	Trained transfer	Instructed transfer
R1 – O1	R3 – O1	O1 or O2 devalued	S0: R1/R2?	S0: R3/R4?
R2 – O2	R4 – O2		S1: R1/R2?	S1: R3/R4?
			S2: R1/R2?	S2: R3/R4?

Note: Both the instructed and trained contingencies, and trained and instructed transfer tests, were counterbalanced with respect to the order in which they were presented. R1-R4 represent instrumental responses (left, right, up and down arrow keys), and O1 and O2 refer to outcomes (crisps and popcorn). S0 represents a neutral stimulus, and S1 and S2 refer to pictures of O1 and O2, respectively.

Participants learnt to perform two instrumental responses (R1 and R2) to earn crisps and popcorn points (outcomes O1 and O2, counterbalanced) in an instrumental training phase (R1-O1, R2-O2). Two additional responses (R3 and R4) were instructed to produce the same two outcomes (R3-O1, R4-O2). One outcome was then devalued using the devaluation procedure used previously. Finally, participants completed two transfer tests, where response choice was tested in the presence of crisps, popcorn, and neutral stimuli. The trained instrumental responses (R1/R2) were used in one transfer test, and the instructed responses (R3/R4) in the other. The order of the transfer tests was counterbalanced between-subjects. If insensitivity to devaluation arises entirely from a propositional process, then the trained and instructed PIT effects should be equally insensitive to outcome devaluation. If the insensitivity to outcome devaluation seen in the standard, trained PIT effect (Experiment 3) is partly due to an automatic S-

O-R triggering mechanism, however, then the trained PIT effect should be more insensitive to devaluation than the instructed PIT effect.

3.4.1 Method

The method was identical to Experiment 4, except in the following respects.

Participants. Thirty-one undergraduate psychology students (28 females, aged 18-24; *M* = 18.86 years, *SEM* = 0.23 years), completed the experiment.

Procedure

***Instrumental training and instructions*.** At the start of the experiment, the two arrow key combinations (left and right, up and down) were randomly allocated to the trained (R1/R2) and instructed (R3/R4) instrumental contingencies. Half of the participants experienced the trained instrumental contingencies first, followed by the instructed contingencies. The order was reversed for the remaining participants. After initial liking ratings were taken, the following on-screen instructions were presented: “In this task, you can earn the two outcomes shown before by pressing the four arrow keys. We will tell you which outcome some of the keys earn, but you will have to work the others out for yourself.” Half of the participants then received the instructed contingencies (presented in the same way as in Experiment 4). All participants were then told, “You can now earn crisps and popcorn by pressing the (R1 and R2) arrow keys. Your task is to learn which key earns each outcome.” Instrumental training commenced using the procedure of Experiment 3. At the end of the instrumental training phase, the instructed contingencies were presented to the participants who had not received them before.

Outcome devaluation and liking ratings. The outcome devaluation procedure from Experiment 3 was received after instrumental training. Participants also completed a second liking test.

Transfer tests. Half of the participants received the trained transfer test first, followed by the instructed transfer test. The test order was reversed for the remaining participants. The instructed instrumental contingencies were shown at the bottom of the screen throughout the instructed transfer test. The trained contingencies were never visually presented or verbally confirmed. After each transfer test, participants' knowledge of the relevant instrumental contingencies was tested, as in Experiment 4. Finally, participants completed the Pavlovian knowledge test and post-experimental questionnaire of Experiment 3.

3.4.2 Results

Exclusions. Six participants were excluded for failing the trained ($N = 3$) or instructed ($N = 3$) instrumental knowledge tests. For completeness, the transfer test results for these participants are provided in Appendix 2.

Liking ratings. Figure 3.5 shows the mean liking ratings given for each outcome in the pre- and post-devaluation liking ratings. The results were very similar to the previous experiments. There was a main effect of liking test, with higher ratings given in the pre-devaluation test than the post-devaluation test, F (1, 24) = 37.23, $p < .001$, $\eta_p^2 = .61$. There was also a main effect of outcome, with the higher ratings given to the valued outcome than the devalued outcome, F (1, 24) = 283.95, $p < .001$, $\eta_p^2 = .92$. Most importantly, there was a significant interaction between the liking test and outcome variables, F (1, 24) = 143.72, $p < .001$, $\eta_p^2 = .86$. Bonferroni corrected pairwise comparisons demonstrated that the valued outcome received higher liking ratings than the devalued outcome in both the pre-devaluation, *t* (24) = 2.73, *p* = .01, 95% CI = [0.25, 1.83] and post-devaluation liking test, *t* (24) = 52.96, *p* < .001, 95% CI = [5.50, 5.94].

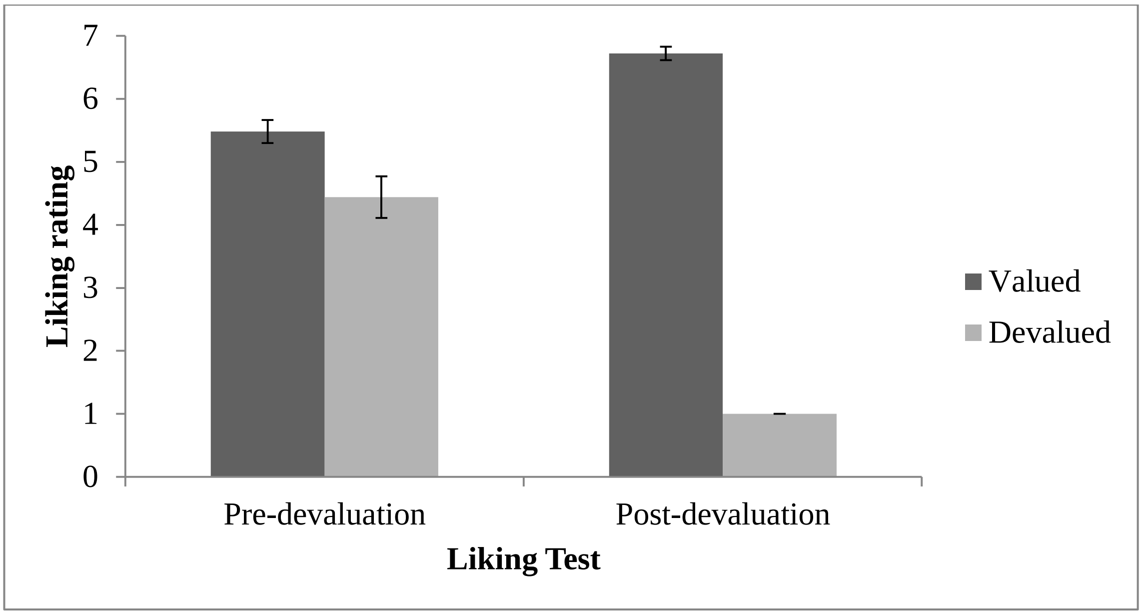

Figure 3.5. *Mean liking ratings in Experiment 5. Ratings were taken for each outcome (valued, devalued) at the start of the experiment (pre-devaluation), and immediately after the devaluation procedure (post-devaluation). Ratings of one and seven represent wanting to eat the outcome "Not at all" and "Very much", respectively. Error bars represent SEM.*

Transfer tests. Figure 3.6 shows the results of the trained and instructed transfer tests in each instruction (O-R, R-O) group. The instruction variable was not expected to influence the results. However, the graph revealed likely differences between the O-R and R-O instruction groups, so it was included in the analysis.

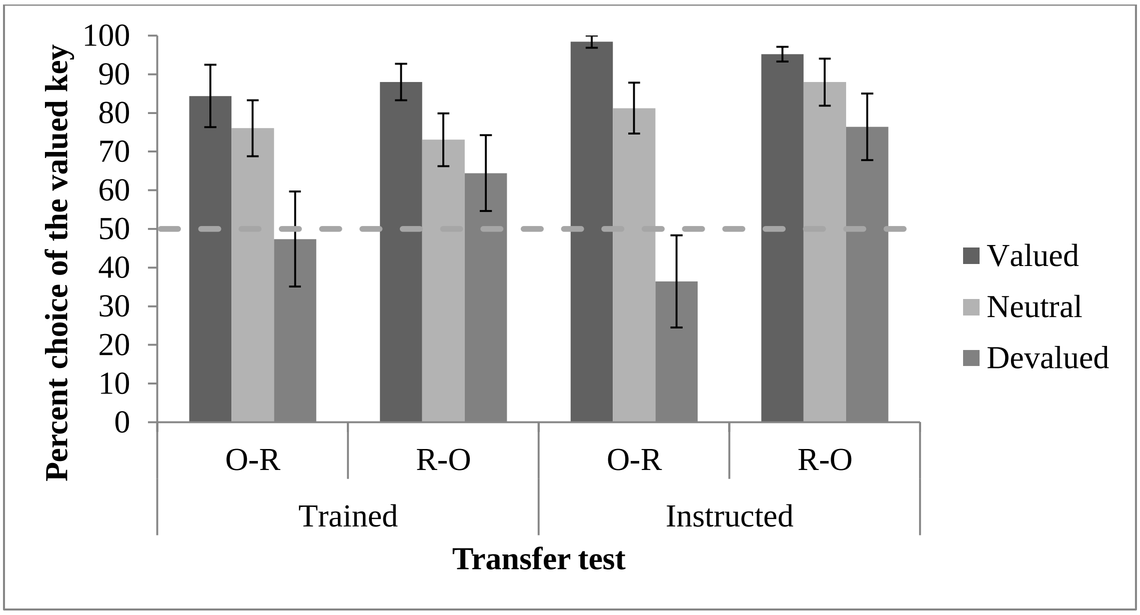

Figure 3.6. Trained and instructed transfer test results in each instruction group of Experiment 5. Response choice was tested in the presence of pictorial stimuli depicting each outcome, or a neutral stimulus. Scores above and below the 50% mid-point represent a bias towards the valued and devalued outcome, respectively. Error bars represent SEM.

A mixed ANOVA revealed a main effect of transfer test, indicating that there was a greater preference for the valued outcome in the instructed transfer test than the trained transfer test , F (1, 23) = 6.86, p = .02, η_p^2 = .23. There was also a main effect of stimulus, F (1.39, 31.90) = 21.19, p < .001, η_p^2 = .48, but not of instruction (O-R versus R-O), F (1, 23) = 1.81, p > .05, η_p^2 = .07. There was a significant stimulus × instruction interaction, suggesting that the effect of stimulus was larger in the O-R instruction group than the R-O instruction group, F (1.30, 31.90) = 4.14, p = .04, η_p^2 = .15. There were no significant interactions between transfer test and instruction, F (1, 23) = 2.53, p > .05, η_p^2 = .10, or transfer test and stimulus, F (1.42, 32.57) = 1.39, p > .05, η_p^2 = .06. Finally, the three-way interaction between transfer test, stimulus and instruction was not significant, F (1.42, 32.57) = 2.42, p > .05, η_p^2 = .10.

The significant stimulus × instruction group (R-O versus O-R) interaction prompted separate, Bonferroni corrected pairwise comparisons exploring the effect of stimulus in each instruction group (collapsed across the transfer tests). In the O-R instruction group, the stimulus that signalled the valued outcome increased responding for that outcome compared to the stimulus that signalled the devalued outcome, *t* (11) = 4.83, *p* < .001, 95% CI = [23.03, 75.93], but not compared to the neutral stimulus, *t* (11) = 2.11, *p* > .05, 95% CI = [-2.87, 28.39]. The stimulus that signalled the devalued outcome increased choice of that devalued outcome compared to the neutral stimulus, *t* (11) = 5.27, *p* < .001, 95% CI = [18.74, 54.70]. Comparable analyses in the R-O instruction group revealed no significant effects of stimulus (possibly due to low power), *t*s < 2.15, *p*s > .13. Thus, there was a significant PIT effect for the devalued outcome in the O-R instruction group, but no evidence of a PIT effect for either outcome in the R-O instruction group.

The non-significant interaction between the stimulus and transfer test (trained versus instructed) variables suggests that the pattern of response choice across the three stimuli did not significantly differ between the instructed and trained transfer tests. The crucial hypothesis, however, concerns whether the trained PIT effect was equally insensitive to the outcome devaluation manipulation as the instructed PIT effect. In fact, the PIT effect for the devalued outcome was numerically larger in the instructed transfer test *(M* = 27.50, *SEM* = 6.30) than in the trained transfer test (*M* = 18.25, *SEM* = 5.58), but this difference did not reach significance *t* (24) = 1.93, *p* = .07, 95% CI = [-0.63, 19.13]. Bayes Factors were therefore calculated to determine whether this null result represents true evidence against the alternative hypothesis (that the instructed PIT effect for the devalued outcome was larger than the comparable trained PIT effect), or whether the null result simply reflects a lack of data sensitivity. The trained and instructed PIT effects were expected to be of a similar magnitude to those obtained in Experiments 3 and 4, respectively. In Experiment 3, the (trained) PIT effect score for the devalued outcome was 13 (calculated using the $S_{neutral} – S_{devalued}$ calculation). In Experiment 4, the size of the equivalent (instructed) PIT effect score in the post-devaluation transfer test was 23.37. Thus, a plausible mean difference score in the current experiment would be 23.37-13 = 10.37. A half-normal distribution was used, with the standard deviation set as the plausible mean difference score (10.37). This produced a Bayes Factor of 3.75. The Bayes Factor is greater than three, and therefore supports the alternative hypothesis (Dienes, 2011). That is, the data support the hypothesis that the instructed PIT effect was more insensitive to devaluation than the trained PIT effect.

Finally, planned one-sample *t*-tests revealed that overall response choice was biased towards the valued outcome in both the trained, t (24) = 5.22, $p < .001$, 95% CI = [13.50, 31.17], and the instructed transfer test, t (24) = 7.59, $p < .001$, 95% CI = [21.54, 37.62].

3.4.3 Discussion

Experiment 5 tested whether training and instructions produce comparable PIT effects after devaluation. Collapsed across the instructed and trained transfer tests, an outcome-selective PIT effect was observed, where the Pavlovian stimuli (outcome pictures) selectively increased the response that had either been trained or instructed to produce the cued outcome. The non-significant interaction between the stimulus and transfer test variables indicated that the size of the trained and instructed PIT effects were comparable. Regarding the devalued outcome specifically, the Bayes Factor suggested that the instructed PIT effect was more insensitive to devaluation than the trained PIT effect. The latter result, that instructed instrumental contingencies produced a greater degree of insensitivity to devaluation than the trained contingencies, seems more in line with the propositional account of PIT than the link-based S-O-R account. Link-based accounts suggest that training should be more likely to foster automaticity than instructions alone, because training allows the mental representations of the response R and the outcome O to be repeatedly activated in a contiguous manner (see Cartoni et al., 2015 for a similar argument). This is precisely why, from the perspective of associative links between mental representations, it makes sense that overtraining renders instrumental responding insensitive to devaluation (Adams, 1982; Tricomi et al., 2009). Thus, the results appear to be more consistent with the propositional model of PIT, where insensitivity to devaluation arises from a controlled reasoning process. Although this process remains to be identified, the results suggest that it can be initiated from either training or instructions.

One unexpected finding was that, in contrast to Experiment 4, the O-R instruction produced a PIT effect but the R-O instruction did not. It is not obvious why this instruction affected response choice in the current experiment but not in Experiment 4. Nevertheless, it was a very clear result and it is therefore necessary to explore possible reasons for it. One possibility is that the O-R instruction (e.g., "crisps = left arrow") was interpreted as a stimulus-response (S-R) instruction. That is, participants may have believed that the instruction referred to which response participants *should* perform in the presence of each outcome picture. It should be noted that the experimenter made no reference to the stimuli when verbally explaining the instruction at the start of the transfer test. Nevertheless, participants may have interpreted the O-R instruction as an S-R instruction (particularly considering the instruction was also present throughout the transfer test). Although this line of reasoning was not anticipated, it is a rational inference. In future, more specific instructions regarding the *instrumental* (outcome-response) function of the responses may be more effective in clarifying the intended meaning of the instructions.

The other noteworthy aspect of the instruction variable (R-O versus O-R) was that it appeared to influence both the instructed and trained PIT effects indiscriminately (as indicated by the non-significant three-way interaction between the stimulus, transfer test, and instruction variables). At first sight, it makes little sense for the instruction variable to influence the trained PIT effect (which involved different, non-instructed instrumental responses). However, the instructed contingencies were presented at the start of the experiment (prior to either transfer test). This was a deliberate decision that was made to ensure that the instructed and trained instrumental responses would be, as far as possible, comparable. However, by presenting the instructed contingencies at the start of the experiment, it is possible for the instructions to influence the trained transfer test. Again, in future, it may be advisable to only present the instructions immediately before the instructed transfer test. Considering the order of the trained and instructed transfer test should be counterbalanced, however, a similar pattern of results might still be expected when the instructed transfer test precedes the trained transfer test. A between-subjects design, in which half the participants receive the training and the other half receives instructions, could circumvent this issue.

Overall, the results suggest that PIT may be insensitive to outcome devaluation even when an associative link mechanism is unlikely to dominate behaviour. It seems possible, therefore, that insensitivity to outcome devaluation may not provide unequivocal evidence for S-O-R theory. Experiment 6 aimed to provide further evidence for this suggestion using a biconditional PIT procedure (described below). The design aimed to control for the formation of instrumental relations during training, so that a PIT effect could not be readily governed by an S-O-R mechanism.

3.5 Experiment 6

The results of Experiments 4 and 5 suggest that the insensitivity of PIT to outcome devaluation may not provide unequivocal evidence for S-O-R theory. When the instrumental contingencies were only instructed (and therefore most likely encoded propositionally), the PIT effect was *still* insensitive to devaluation. This was interpreted as evidence to suggest that insensitivity to outcome devaluation may (at least sometimes) reflect a controlled decision-making process.

Another PIT effect that is not readily explained by S-O-R theory has also recently been observed (Hardy et al., in revision). This design, which is shown in Table 3.4, was inspired by Rescorla's (1990) biconditional experiment in rats. Participants first learnt to perform two responses (R1 and R2) to earn beer and chocolate points (O1 and O2) in a discriminative training phase. Each trial began with the presentation of a pair of blue or black arrow symbols ("← →"), which served as discriminative stimuli (Sd1 and Sd2). Sd1 signalled that R1 and R2 would produce O1and O2, respectively (Sd1: R1-O1, R2-O2). Sd2 signalled the *opposite* contingencies (Sd2: R1-O2, R2-O1). Instrumental response choice (R1 versus R2) was then tested in the presence of each discriminative stimulus in compound with a beer, chocolate or neutral stimulus (S1, S2 or S0).

Table 3.4

Design and results of Hardy et al. (in revision).

Discriminative training		**Transfer test**
Sequential	Intermixed	
Stage 1:	Sd1: R1-O1, R2-O2	Sd1+S1: R1 > R2
Sd1: R1-O1, R2-O2	Sd2: R1-O2, R2-O1	Sd1+S2: R1 < R2
Stage 2:		Sd2+S1: R1 < R2
Sd2: R1-O2, R2-O1		Sd2+S2: R1 > R2

Note: Sd1 and Sd2 refer to discriminative stimuli (blue and black arrow symbols), R1 and R2 denote instrumental responses (left and right arrow key presses), and O1 and O2 refer to outcomes (beer and chocolate points). S1 and S2 refer to pictures of O1 and O2, respectively. During the transfer test, response choice in the presence of the pictorial stimuli (S1 and S2) was assessed in relation to a neutral stimulus that was also presented in compound with each discriminative stimulus (Sd1 and Sd2).

The first important aspect of Hardy et al.'s (in revision) design is that the discriminative stimuli (Sd1 and Sd2) were trained to predict both responses and outcomes equally. S-O-R theory therefore predicts that the Sds should not bias response choice either way, because they should activate both outcomes, which should then prime both responses equally. The second noteworthy feature concerns the pictorial beer and chocolate stimuli (S1 and S2). Although these stimuli should have entered into Pavlovian associations with their outcomes prior to the experiment, they were not paired with either instrumental response during discriminative training. S-O-R theory therefore predicts that these Pavlovian stimuli should activate their associated outcome representations, which should then activate both responses equally (because each outcome was paired with both responses during discriminative training). Thus, S-O-R theory predicts no bias in response choice during the critical transfer test. However, a clear PIT effect was observed during the transfer test. In compound with Sd1, S1 and S2 increased choice of R1 and R2, respectively. The pattern was reversed in the presence of Sd2; S1 and S2 now increased R2 and R1, respectively. Thus, response choice reflected a summation of the discriminative and Pavlovian stimuli that were present on each transfer test trial.

Hardy et al.'s (in revision) biconditional PIT effect is most consistent with the hierarchical theory of PIT, where the Pavlovian and discriminative stimuli collectively signal which instrumental response is most likely to be rewarded. One possibility is that the hierarchical relationships were encoded *propositionally*. That is, response choice may have been mediated by explicit beliefs that the stimuli signalled which response was more likely to be reinforced. Hardy et al.'s data do not provide conclusive evidence regarding the role of propositional processes. Their results do, however, provide evidence of a PIT effect that cannot be readily explained by S-O-R theory. It resembles

Experiments 4 and 5 in this respect, where PIT effects were observed that could not be readily accounted for by S-O-R theory.

Experiment 6 tested whether Hardy et al.'s (in revision) biconditional PIT effect would be sensitive to the outcome devaluation manipulation used in Experiments 3-5. Insensitivity to outcome devaluation is arguably the single strongest line of support for S-O-R theory. Thus, if the biconditional PIT effect is also insensitive to outcome devaluation, it would complement the results of Experiments 4 and 5 in suggesting that insensitivity to outcome devaluation may not provide unequivocal evidence for S-O-R theory.

3.5.1 Method

The method was the same as Experiment 3, except in following respects.

Participants, apparatus and materials. Fifty-nine Plymouth University psychology undergraduates (44 females, aged between 18 and 27; *M* = 20.10 years, *SEM* = 0.29 years) completed the experiment for course credit.

Procedure

Discriminative training. After providing initial liking ratings, the experimenter read aloud the following instructions: “You can now earn crisps and popcorn by pressing the left or right arrow keys. Different arrow shapes indicate which key earns which reward. Your task is to learn this. Press any key to begin.” Discriminative training consisted of a sequential phase followed by an intermixed phase. Each trial began with the central presentation of a black or blue arrow symbol (“← →”). These symbols served as discriminative stimuli (Sd1 and Sd2), and signalled that participants should press either the left or right arrow key. Responses were followed by the text “You win one [CRISPS/POPCORN] point”, depending on the reward earned and the discriminative stimulus that was present. The instrumental responses (left and right arrow keys, R1 and R2) and outcomes (crisps and popcorn, O1 and O2) were fully counterbalanced with respect to the discriminative stimuli (blue and black arrow shapes, Sd1 and Sd2). In the presence of Sd1, R1 and R2 produced outcome O1 and O2, respectively (Sd1: R1-O1, R2-O2). The responses produced the opposite outcomes in the presence of Sd2 (Sd2: R1-O2, R2-O1). During the sequential stage of training, each discriminative stimulus was presented for eight sequential trials, followed by eight sequential trials with the other discriminative stimulus. The order of discriminative stimulus presentation was counterbalanced between-subjects. The trials were separated by 750-1250ms intervals.

Discriminative contingency knowledge was tested after participants completed one cycle of the sequential stage of discriminative training (16 trials). Participants answered four questions to assess explicit knowledge of the discriminative contingencies. The discriminative stimuli were presented in turn, above the question, “When this arrow was present, which key earned [crisps/popcorn] the LEFT or RIGHT key?” Correct answers were followed by the statement “Correct”, beneath the discriminative stimulus. Incorrect answers were followed by a buzzer noise and the statement, “Incorrect. The correct answer was [LEFT/RIGHT]”. Feedback was presented for 1500ms, the four questions were presented in a random order, and they were separated by 750-1250ms intervals. If participants answered any of the questions incorrectly, the sequential phase and knowledge test were repeated until participants answered every question correctly. This extended version of training was used to first be consistent with Hardy et al.'s (in revision) procedure, and secondly because the contingencies were considerably more difficult to learn than in the previous experiments.

The sequential training phase was followed by an intermixed training phase. Intermixed training followed the same procedure as the sequential training phase, except that the 16 trials were randomly intermixed. Participants subsequently completed a discriminative contingency knowledge test that was identical to the first knowledge test. The intermixed training phase and contingency knowledge test was also repeated until participants reported accurate contingency knowledge.

Outcome devaluation. Following successful acquisition of the discriminative contingencies, either the crisps or popcorn was devalued using the cloves procedure of Experiments 3-5. Participants also completed post-devaluation liking ratings after the devaluation procedure.

Transfer test. The instructions for the transfer test were very similar to those given in Experiment 3; participants were told that they could continue to earn crisps and popcorn by pressing the arrow keys, that they would only be told how many they had earned at the end, and that pictures would sometimes be presented before they responded. They were also informed that they would be required to eat all of the food they earned afterwards.

Each transfer test trial began with the presentation of a crisps, popcorn or neutral stimulus for 3000ms. A discriminative stimulus (arrow symbols) then appeared beneath the picture stimulus, which remained until participants selected the left or right arrow key. Response time was not limited, and feedback was not provided. There were four cycles of 12 trials (48 trials in total). In each cycle, each discriminative stimulus (Sd1or Sd2) was presented with each Pavlovian stimulus (crisps, popcorn, or neutral) twice in a random order. Participants completed one cycle of practice trials before continuing on to the main transfer test. After the practice phase, the outcomes were removed and participants were reminded that they would be required to eat all of the food they earned at the end of the experiment.

After completing the transfer test, participants completed a final instrumental knowledge test (without feedback) to assess memory of the instrumental contingencies. The Pavlovian knowledge test of Experiments 3-5 was also administered; participants were shown the crisps and popcorn pictures and selected the outcome that they represented.

3.5.2 Results

Exclusions. Eighteen participants were excluded for failing either the final instrumental (N = 10) or Pavlovian (N = 7) knowledge test, or both (N = 1). Given the high proportion of participants who failed the contingency knowledge tests, the mean transfer test scores in these participants are provided in Appendix 2. One further participant was excluded because s(he) required 23 blocks of discriminative training to pass the instrumental knowledge test, and s(he) did not complete the rest of the experiment (due to time restraints). The data from the remaining 40 participants were entered into the analyses.

Liking ratings. Figure 3.7 shows the mean liking ratings given for each outcome in the pre- and post-devaluation liking rating tests. The results were very similar to the previous experiments. There was a main effect of liking test, with higher ratings given in the pre-devaluation test than the post-devaluation test, F (1, 39) = 50.82, $p < .001$, η_p^2 = .57. There was also a main effect of outcome, with the higher ratings given to the valued outcome than the devalued outcome, F (1, 39) = 111.20, $p < .001$, η_p^2 = .74. Most importantly, there was a significant interaction between the liking test and outcome variables, F (1, 39) =414.09, $p < .001$, η_p^2 = .91. Planned pairwise comparisons revealed that in the pre-devaluation liking test, the subsequently devalued outcome received higher liking ratings than the valued outcome (despite counterbalancing the food outcomes), *t* (39) = 2.10, *p* = .04, 95% CI = [0.03, 1.28]. Most importantly, the still-valued outcome received higher ratings than the devalued outcome in the post-devaluation transfer test, *t* (39) = 28.82, *p* < .001, 95% CI = [4.70, 5.40].

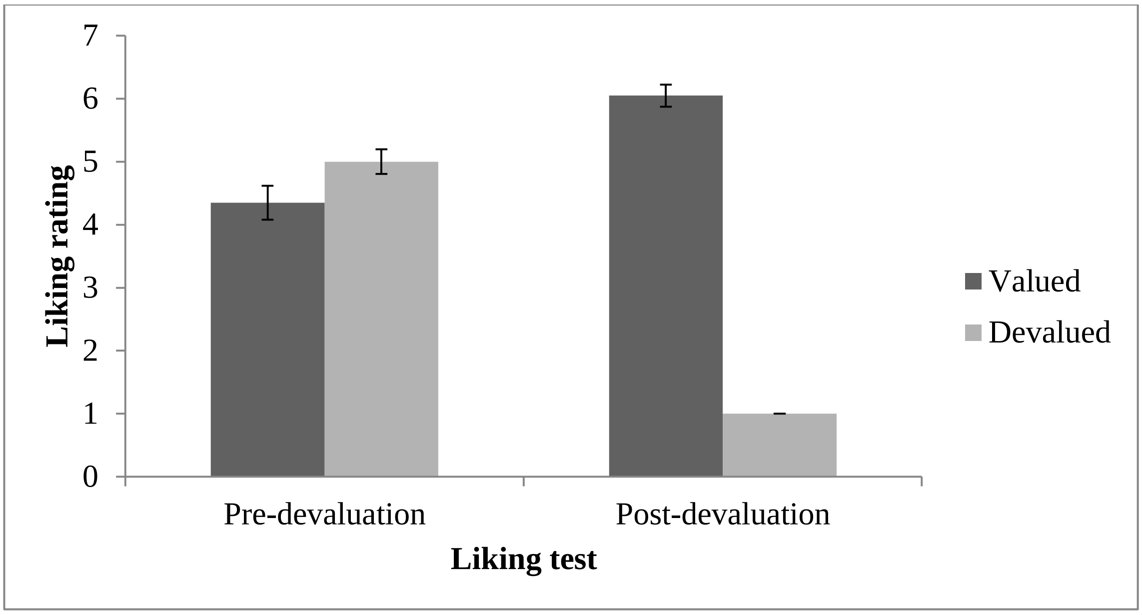

Figure 3.7. Liking ratings in Experiment 6. Ratings were taken for each food outcome (crisps, popcorn) taken at the start of the experiment (pre-devaluation) and immediately after the devaluation procedure (post-devaluation). Ratings of one and seven represent wanting to eat the outcome "Not at all" and "Very much", respectively. Error bars represent SEM.

Transfer test. Figure 3.8 shows the transfer test results of Experiment 6. The graph shows the percent choice of the key that predicts the still-valued outcome (collapsed across discriminative stimuli) in the presence of the Pavlovian stimulus signalling the valued and devalued outcome, and the neutral stimulus. A repeated measures ANOVA (with Greenhouse-Geisser correction applied for sphericity) revealed an effect of stimulus, *F* (1.20, 46.82) = 3.98, *p* < .05, η_p^2 = .09. Bonferroni corrected pairwise comparisons revealed no significant differences between the three stimuli, *t*s < 2.10, *p*s > .13. A one-sampled *t*-test confirmed that overall response choice was biased towards the still-valued outcome (relative to the 50% midpoint), *t* (39) = 11.90, *p* < .001, 95% CI = [31.86, 44.91].

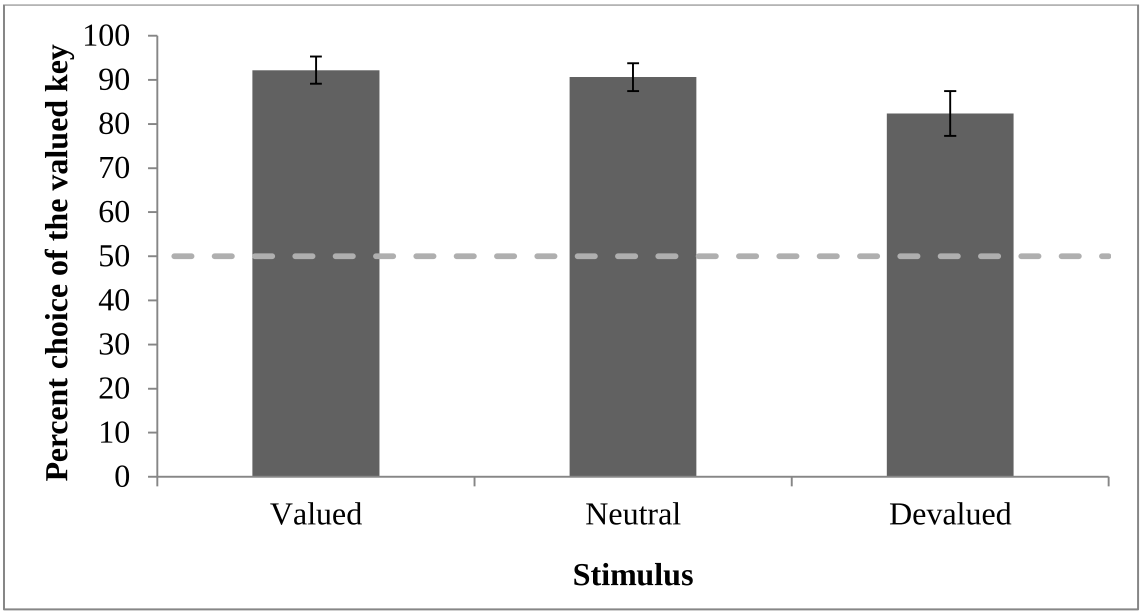

Figure 3.8. Transfer test results of Experiment 6. Response choice was tested in the presence of pictorial stimuli depicting each outcome, or a neutral stimulus. The data are collapsed across the discriminative stimulus (Sd1, Sd2) trials. The dependent variable is the percent choice of the instrumental response that produced the still-valued outcome in the presence of each discriminative stimulus. Scores above and below the 50% mid-point represent a bias towards the valued and devalued outcome, respectively. Error bars represent SEM.

The analysis above suggests that a PIT effect was not observed for either outcome. Given that the crucial hypothesis concerned the devalued outcome, a Bayes Factor was calculated to substantiate the claim that a PIT effect was not observed for the devalued outcome. It is reasonable to expect that the PIT effect for the devalued outcome in the current experiment would be of a similar magnitude to that observed in Experiment 3 (where insensitivity to devaluation was observed in a typical PIT procedure). The mean difference score between the neutral stimulus and the stimulus that signalled the devalued outcome was 13. The comparable mean difference score in the current experiment was 8.28 (*SEM* = 3.95). A half-normal distribution with the standard deviation set as the plausible mean difference score (13) produced a Bayes Factor of 4.25. The Bayes Factor is above the critical threshold of three, and therefore supports the alternative hypothesis (Dienes, 2011). That is, the Bayes Factor suggests that there is substantial support for the suggestion that a PIT effect was observed for the devalued outcome. Finally, the PIT effect for the devalued outcome (*M* = 8.28, *SEM* = 3.95) was numerically larger than the PIT effect for the valued outcome (*M* = 1.56, *SEM* = 1.86), but this difference did not reach significance, *t* (39) = 1.74, *p* = .09, 95% CI = [-1.10, 14.54].

3.5.3 Discussion

Experiment 6 tested whether a biconditional PIT effect would be sensitive to an outcome devaluation manipulation. S-O-R theory cannot readily explain biconditional PIT effects, because the discriminative stimuli and instrumental responses are equally paired with each outcome. Collapsed across the stimuli, there was a strong overall bias towards the still-valued outcome. Indeed, instrumental response choice approached ceiling (complete responding for the valued outcome) in all three stimulus conditions. Ceiling effects are a critical issue in PIT devaluation experiments, and particularly in the current experiments. They are therefore discussed more thoroughly in section 3.6 (General Discussion). Despite the clear preference for the valued outcome, the Bayes Factor indicated that a PIT effect was apparent for the devalued outcome. That is, the stimulus that signalled the devalued outcome increased instrumental responding for that outcome compared to the neutral stimulus. Notably, a PIT effect for the devalued outcome was observed even when an S-O-R associative chain could not readily control behaviour. The data therefore accord with the results of Experiments 4 and 5, in that they demonstrate PIT effects that are not readily explained by S-O-R theory.

3.6 General Discussion

The current experiments explored the effect of a novel, very strong outcome devaluation procedure on PIT. In Experiment 3, overall response choice was biased towards the still-valued outcome during the transfer test. This bias is indicative of goal-directed control, because it shows that overall response choice was sensitive to both the instrumental contingencies and the current value of the outcomes (de Wit & Dickinson, 2009; Dickinson, 1985, 2016). Paradoxically, PIT effects were still observed for each outcome; the crisps and popcorn stimuli selectively increased choice of the instrumental response that was paired with cued outcome, relative to the neutral stimulus. Crucially, there was no significant difference in the size of the PIT effect for the valued and devalued outcomes. The latter result replicates previous work in demonstrating an insensitivity of PIT to outcome devaluation (Colwill & Rescorla, 1990a; Corbit et al., 2007; Hogarth, 2012; Hogarth & Chase, 2011; Holland, 2004; Rescorla, 1994b; Watson et al., 2014). The novel contribution of Experiment 3 was in the use of an original and very strong devaluation procedure. The result therefore extends previous knowledge by demonstrating that PIT effects can be insensitive to even the very strong outcome devaluation manipulation employed here.

In light of the current results, it is important to now examine the three studies in the human PIT literature that have recently reported *sensitivity* to outcome devaluation more closely (Allman et al., 2010; Eder & Dignath, 2016a, 2016b). Clearly, these experiments are inconsistent with both the current results and the wider literature. The first demonstration of sensitivity to outcome devaluation came from Allman et al. (2010), who used a stock market procedure where participants acted as investment bankers. Eder and Dignath (2016a) also used a very similar procedure, so the experiments are discussed collectively. In both cases, instrumental responses were trained to produce monetary outcomes in different currencies, and Pavlovian stimuli represented companies that traded in those currencies. Clear PIT effects were observed in the initial pre-devaluation transfer tests. That is, the Pavlovian stimuli selectively increased the instrumental response that was associated with the same currency (compared to neutral stimuli). One of the outcomes was subsequently devalued by reducing its exchange rate, so that the currency was rendered worthless[7]. Instrumental responding was then assessed in a post-devaluation transfer test. PIT effects were observed in both experiments for the still-valued outcome, but were markedly reduced for the devalued outcome. The authors therefore argued that their PIT effects were sensitive to devaluation, because they were influenced by the instructed devaluation manipulation. These results are important because they speak against strong claims that PIT effects are not mediated by outcome value (e.g., Balleine & O'Doherty, 2010; Hogarth et al., 2013; Hogarth & Chase, 2011; Rescorla, 1994b). Moreover, Allman et al. (2010) suggested that this sensitivity to devaluation (and human PIT effects in general) may be mediated by a higher order propositional process.

Eder and Dignath (2016b, Experiment 1), whose experiment was outlined at the start of this chapter, provided further evidence of sensitivity to devaluation. Briefly, they established two instrumental responses and two Pavlovian stimuli as predictors of two distinct lemonade rewards. One of the lemonade drinks was then devalued by making it taste unpleasant. The final transfer test was divided into two blocks, and participants were required to drink their winnings after each block. Under these circumstances, a PIT effect was observed for the still-valued outcome, but not for the devalued outcome. Again, Eder and Dignath (2016b) suggested that PIT is sensitive to strong outcome devaluation manipulations.

It is clear that Allman et al. (2010) and Eder and Dignath's (2016a, 2016b) experiments have produced some very interesting results with respect to the effect of outcome devaluation on PIT. Their results certainly suggest that PIT effects can be goal-directed, since the effects were either attenuated or abolished by devaluation. Although

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[7] Eder and Dignath (2016a) also tested the effect of an *upvaluation* manipulation, where the value of one outcome was increased. This manipulation had little impact and is therefore not discussed further.

there are many procedural differences between these experiments and those that have reported insensitivity to devaluation, there is one aspect in each that seems potentially important. Allman et al. (2010) and Eder and Dignath (2016a) instructed their participants that one currency outcome was “worthless”. It is possible that this was construed as instructed extinction of the instrumental relationship, leading participants to believe that the response would no longer produce the devalued currency (Hogarth, 2012). A related explanation applies to Eder and Dignath (2016b). They explicitly told their participants that the Pavlovian cues were not meaningful during the transfer test. This is an instructed Pavlovian extinction procedure, and is not typical to PIT tasks. Although these may seem like subtle procedural differences, they may impact heavily on decision-making, particularly if response choice is governed by explicit strategies. As noted in the introduction of the current chapter, these instructions are particularly important with respect to the propositional EU account of PIT.

Recall that the propositional EU theory suggests that PIT effects reflect a judgement about the expected probability (Op) and value (Ov) of each outcome. Pavlovian stimuli that are presented during the transfer test are suggested to increase the perceived probability of the associated outcome Op, providing the associated instrumental response is performed. These judgements are assumed to be propositional in nature, and hence should be highly susceptible to verbal instructions. If Eder and Dignath's (2016b) instruction was interpreted as instructed extinction of the Pavlovian contingencies, then participants may have inferred that the Pavlovian stimuli *did not* signal which response was more likely to be rewarded. A similar argument can be made for both Allman et al.'s (2010) and Eder and Dignath's (2016a) studies. If their devaluation manipulation was construed as instructed extinction of the instrumental relationship, then participants may have abandoned their belief that the Pavlovian stimulus signalled the availability of a currency; a worthless currency is not really a currency at all. Hence, it is possible that Allman et al.’s (2010) and Eder and Dignath's (2016a, 2016b) procedures simultaneously degraded both cue-elicited outcome probability (Op) *and* outcome value (Ov). The fact that the PIT effect was either reduced or abolished under these circumstances supports the suggestion that the standard PIT effect may be mediated by a controlled inference that is based on both perceived outcome probability (Op) and outcome value (Ov). The experiments in Chapter 4 aim to test this prediction of the propositional EU model more systematically.

The discussion above suggests that the propositional EU account may be able to reconcile the previous demonstrations of insensitivity to outcome devaluation (Allman et al., 2010; Eder & Dignath, 2016a, 2016b) with the current demonstration of insensitivity to outcome devaluation (Experiment 3). The remainder of the discussion therefore focuses on Experiments 4-6. Experiment 4 firstly demonstrated a PIT effect that was acquired by instruction in the absence of experience. Participants were first instructed that two responses would produce different outcomes. Crucially, they did not receive any training on these contingencies. During the transfer test, the instruction alone was sufficient to generate a PIT effect; presenting a stimulus that was associated with one of the outcomes was sufficient to bias response choice towards the response that has been instructed to produce the cued outcome. The demonstration of an instructed PIT effect suggests that PIT effects can (at least sometimes) be propositional in nature. It might be argued that this is a premature conclusion. Perhaps the instructions alone, for instance, were able to generate instrumental links. The demonstration of an ‘instructed PIT effect’ would not be surprising under these circumstances, because the stimulus would activate the associated instructed response using an S-O-R mechanism (in much the same way as in the case of a ‘trained’ instrumental response).

It is not immediately obvious how an instructed response could produce an instrumental associative link. Associative links are usually assumed to require the repeated and contiguous presentation of the two events (e.g., Elsner & Hommel, 2004; Shanks & Dickinson, 1988, 1991). It should, therefore, be very difficult for an instructed contingency to generate an associative link (Mitchell et al., 2009). Perhaps the instruction – that the response would produce the outcome – concurrently activated the mental representation of the response and the outcome, which allowed a link to form between them. Of course, a straightforward test of this account would be to instruct participants that the response would *not* produce the outcome, and then test whether that instruction would elicit a PIT effect (Mitchell et al., 2009). More generally, one of the core assumptions of link-based models is that associative links form passively from contiguous pairings of events (in this case, the response R and the outcome O). Verbal instructions are recognised as one of the best ways to tease link-based and propositional accounts of learning apart, because the two theories usually make different predictions in this regard (De Houwer, 2009, 2014; De Houwer et al., 2005; Mitchell et al., 2009). If instrumental links were suggested to form as a result of a mere verbal instruction, it would be one step closer to allowing the link-based and propositional accounts of learning to become indistinguishable (Lovibond, 2003).

Another interesting aspect of Experiment 4 was that the instructed PIT effect appeared to be insensitive to the devaluation manipulation. That is, the stimulus that signalled the devalued outcome increased choice of the response that had been instructed to produce the devalued outcome, relative to the neutral stimulus. This instructed insensitivity to devaluation was also replicated in the O-R instruction group of Experiment 5. Moreover, the Bayes Factor indicated that the instructed PIT effect was *more* insensitive to devaluation than the trained PIT effect. As noted above, the demonstration that PIT is often insensitive to outcome devaluation is usually regarded as evidence of an automatic associative link mechanism, where the stimulus has a signalling rather than motivational role (Hogarth et al., 2013; Hogarth & Chase, 2011; Rescorla, 1994b; Watson et al., 2014). Instructional effects, on the other hand, are less readily attributed to a link-formation mechanism (Mitchell et al., 2009). The data therefore confirm the paradox that was outlined at the start of this chapter; an instructed PIT effect was observed, but was insensitive to outcome devaluation. One way to reconcile this paradox would be to propose that insensitivity to devaluation may, at least sometimes, reflect the operation of controlled, propositional processes. The experiments in Chapter 4 seek evidence for this possibility, by exploring the nature of processes that produce insensitivity to devaluation.

Another possibility is that the insensitivity of the instructed PIT effect to outcome devaluation in Experiment 5 was due to the use of a within-subjects design. As a consequence of being associated with the same outcomes, instructed and trained instrumental responses may have become associated with one another through a process of acquired equivalence (Hall, 1996; Hall, Mitchell, Graham, & Lavis, 2003). That is, R1 and R3 might have been treated as equivalent by virtue of being followed by the same outcome O1 (the same applies to R2 and R4 – both associated with O2). In the instructed transfer test, stimulus S1 may have activated O1, which activated the trained R1 via an S-O-R link mechanism. The instructed R3 might then have been executed because it was perceived as equivalent to R1. Of course, this analysis relies on the assumption that acquired equivalence can be generated for instructed relations alone, and so also seems to reflect an inferential process of generalisation (Smyth, Barnes-Holmes, & Barnes-Holmes, 2008). The instructional effects of Experiments 4 and 5, therefore, appear to be best interpreted within a propositional model of PIT. The section below examines the final experiment in this chapter: Experiment 6.

Experiment 6 explored the effect of outcome devaluation on a biconditional PIT task. Here, each discriminative stimulus and instrumental response was paired with both outcomes equally. Instrumental response choice was then tested in the presence of each discriminative stimulus, alongside a picture of one of the outcomes. S-O-R theory predicts that response choice under these circumstances should be at chance, because each discriminative stimulus and instrumental response was associated with both outcomes equally. The pictorial stimuli were not paired with either response, so they would not be expected to bias response choice either. Following outcome devaluation, overall response choice was heavily biased towards the still-valued outcome. Indeed, a PIT effect was not observed for the valued outcome, probably because response choice was close to ceiling. The Bayes Factor, however, indicated that a PIT effect was still observed for the devalued outcome. Thus, a PIT effect was observed for a devalued outcome in a biconditional PIT procedure, which cannot be readily explained by S-O-R theory. The data therefore complement the interpretations of Experiments 4 and 5 in demonstrating that the insensitivity of PIT to devaluation may not provide unique evidence for S-O-R theory.

It should be noted that the results of Experiment 6 do not provide direct evidence for the propositional approach. Instead, the data support a hierarchical model of PIT, where the stimuli presented during the transfer test increase the perceived strength of the associated instrumental contingency (Hogarth et al., 2014; Hogarth & Troisi, 2015; Rescorla, 1991, 1992b). In light of the results of Experiments 4 and 5 (where instructed responses produced PIT effects that were insensitive to outcome devaluation), however, it seems likely that PIT effects may be mediated by a hierarchical mechanism that is (at least in humans) propositional in nature. That is, participants may infer that the stimuli that are presented during the transfer test signal which instrumental response is more likely to be reinforced. They may even infer that the alternative, non-cued outcome is completely unavailable (has zero probability). This difference in perceived outcome probability (Op) may outweigh the difference in outcome value (Ov), and may therefore explain why PIT is so often insensitive to outcome devaluation manipulations.

The overall pattern of results in this chapter add to a growing body of literature demonstrating that Pavlovian stimuli can facilitate instrumental responding for a common outcome, even if that outcome has been devalued. Consistent with the literature, this effect has been interpreted as evidence to suggest that PIT is often insensitive to outcome devaluation manipulations. However, there is a very important limitation of this interpretation. PIT effects are usually assessed by measuring the extent to which a Pavlovian stimulus increases instrumental responding for the same outcome, compared to a neutral stimulus. After outcome devaluation, response choice in the presence of the neutral stimulus is generally biased towards the still-valued outcome; participants respond for the outcome they like best. This bias was particularly apparent in the current experiments because the devaluation manipulation was so strong. However, it is usually present to some degree in PIT procedures that use other devaluation manipulations, such as selective satiation (Hogarth, 2012; Watson et al., 2014) and health warnings (Hogarth & Chase, 2011). Indeed, the non-cued bias towards the still-valued outcome is regarded as an important component of the overall result, because it shows that non-cued instrumental responding is goal-directed. Thus, following outcome devaluation, the PIT effect is assessed using a baseline that is biased towards the still-valued outcome. What is not usually discussed is that, as responding for the still-valued outcome approaches ceiling, there is less opportunity to detect a PIT effect for the valued outcome. Conversely, there is relatively *greater* scope to observe a PIT effect for the devalued outcome. This means that the size of the PIT effect for the valued outcome may be underestimated in the standard task, and the effect for the devalued outcome exaggerated. Hence, when PIT effects are of a comparable magnitude for the valued and devalued outcomes, it may simply reflect an artefact of the measurement technique.

The ceiling effects issue outlined above has important implications for the theories of PIT. Insensitivity to devaluation is a key line of support for S-O-R theory, because it suggests that PIT is mediated by an automatic mechanism that is not flexible to motivational changes. The propositional EU model, on the other hand, is less readily supported by insensitivity to devaluation. Recall that the EU model proposes that PIT effects are driven by propositional judgements about both the perceived outcome value (Ov) and probability (Op). The finding that PIT is insensitive to devaluation speaks against this prediction, because it suggests that a judgement about outcome value Ov does not influence response choice during the transfer test. The ceiling effects issue outlined above, however, may reconcile the insensitivity of PIT to devaluation with the propositional EU account. When only one outcome is cued, there is a small tendency to respond for that outcome, even when it has been devalued. The standard PIT task (such as that used in Experiment 3) may overestimate this tendency due to the ceiling effect on response choice in the neutral stimulus condition. The experiments in Chapter 4 aim to test this EU model of PIT, by examining whether sensitivity to outcome devaluation is observed when the possible influence of ceiling effects is eliminated.

In summary, the current set of experiments demonstrated that PIT effects can persist even after a very strong outcome devaluation manipulation (Experiment 3). This insensitivity to devaluation was also apparent in Experiment 4 when the instrumental contingencies were instructed rather than established through trial-by-trial conditioning. Moreover, the instructed PIT effect for the devalued outcome was numerically larger than the comparable trained PIT effect in Experiment 5. These data provide unique evidence for the role of propositional processes in PIT. Finally, a PIT effect for a devalued outcome was observed in a biconditional procedure in Experiment 6. The latter result cannot be readily attributed to an S-O-R associative chain. The data suggest, therefore, that insensitivity to devaluation may not always reflect an S-O-R link mechanism, but may instead (at least sometimes) reflect a controlled reasoning process that is possibly based on judgements of EU. These EU estimates were suggested to reflect propositional inferences about the perceived probability (Op) and value (Ov) of each outcome. The presentation of a Pavlovian stimulus during the transfer test may increase the perceived probability of that outcome, providing the associated response is performed. When only the devalued outcome is cued, there is a tendency for participants to choose that devalued outcome, perhaps because it is perceived to be the only available outcome (i.e. the Op for the valued outcome is zero). This cue-elicited increase in perceived probability Op gives rise to an apparent insensitivity to devaluation, which is particularly pronounced when baseline response choice is biased towards the still-valued outcome. This is because there is less room to observe a PIT effect for the still-valued outcome, and relatively greater scope to detect a PIT effect for the devalued outcome. The experiments in Chapter 4 aim to test the propositional EU account more thoroughly, by exploring whether PIT is sensitive to outcome devaluation when PIT effects are not assessed relative to a biased baseline.

Chapter 4: Expected utility

4.1 Introduction

Chapter 3 demonstrated that PIT effects can be insensitive to even a very strong devaluation procedure. In Experiment 3, for example, participants reported a clear aversion to the devalued outcome in the post-devaluation liking test. They also demonstrated an overall preference towards the valued outcome in the transfer test. Yet, a PIT effect was still observed for the devalued outcome. Most importantly, the PIT effect was of a comparable magnitude to that observed for the valued outcome. The PIT effect was therefore said to be insensitive to devaluation. This insensitivity to outcome devaluation is consistent with the majority of the literature (Colwill & Rescorla, 1990a; Corbit et al., 2007; Hogarth, 2012; Hogarth & Chase, 2011; Holland, 2004; Rescorla, 1994b; Watson et al., 2014). It is also entirely consistent with the predictions of S-O-R theory. However, as noted earlier, S-O-R theory has difficulty explaining the instructional sensitivity that has been observed in previous PIT experiments (see Hogarth et al., 2014 and Experiment 2 of the current thesis). A propositional EU account, in which response choice is mediated by judgements about perceived outcome probability (Op) and outcome value (Ov), was therefore proposed to explain the data. The experiments in this chapter aim to test some predictions of this propositional EU account of PIT.

The propositional EU account suggests the Pavlovian stimulus that is presented during the PIT transfer test increases the perceived probability of the associated outcome, providing the associated instrumental response is performed (Cartoni et al., 2015; Hogarth, 2012; Hogarth et al., 2014; Seabrooke et al., 2016). When a stimulus that signals a devalued outcome is presented, it is suggested to signal an outcome that has a high probability (Op), but a low value (Ov). It also signals that the alternative, non-cued outcome currently has a low probability Op, even though it retains a high value Ov. Indeed, the non-cued outcome may even be interpreted to be completely unavailable (zero probability). The propositional EU account proposes that these cue-elicited changes in outcome probability Op give rise to a stronger utility estimate for the cued, devalued outcome than the non-cued, valued outcome. Simply put, the difference in perceived outcome probability is suggested to outweigh the difference in outcome value. This difference is then suggested to increase responding for the devalued outcome when it is cued.

The current chapter reports five experiments that systematically manipulate cue-elicited outcome probability (Op) and outcome value (Ov) to test the propositional EU account of PIT. Furthermore, the experiments utilise novel PIT procedures that do not measure PIT effects against baseline response choice in the presence of a neutral stimulus. As noted in Chapter 3, baseline response choice is usually biased towards the valued outcome in PIT devaluation experiments. When PIT effects are assessed relative to this baseline, the PIT effect for the valued and devalued outcomes often appear to be of a similar magnitude. However, the biased baseline leaves less room to detect a PIT effect for the valued outcome (because of the ceiling effect on response choice), and relatively greater scope to detect a PIT effect for the devalued outcome. The current experiments therefore eliminate this shift in baseline responding to establish a better estimate of the role of outcome value in PIT.

4.2 Experiment 7

Experiment 7 was very similar to an experiment in rats by Rescorla (1994b). Table 4.1 shows the design. Participants first learnt to perform two instrumental responses (R1 and R2) to earn points towards four outcomes: crisps, popcorn, cashew nuts and nachos (outcomes O1-O4, counterbalanced). Response R1 was scheduled to produce O1 and O3 on a random half of the trials each, while R2 was scheduled to produce O2 and O4 on half of the trials each. Outcomes O3 and O4 were then devalued using the cloves procedure of Experiments 3-6. Response choice was finally tested in the presence of two stimulus compounds: S1 and S4, or S2 and S3. These stimulus compounds each signal both one valued and one devalued outcome. They also signal one outcome associated with R1 and a second outcome associated with R2. The S1+S4 compound, for example, is associated with outcomes O1 and O4, which were paired with R1 and R2, respectively. Hence, both responses were associated with a cued outcome on every trial. Crucially, only stimulus S1 signals a valued outcome (O1), because O4 (signalled by S4) was devalued. If PIT is sensitive to outcome devaluation when cue-elicited probability Op is controlled, then a selective bias will be observed towards the cued, valued outcome. Hence, the S1+S4 compound will increase R1 responses, because R1 produced the valued O1 (and R2 produced the devalued O4). By the same logic, the S2+S3 compound will increase R2 responses.

Table 4.1

Design of Experiment 7

Instrumental training	Outcome devaluation	Transfer test
R1 – O1, O3	O3 *and* O4 devalued	S1+S4: R1/R2?
R2 – O2, O4		S2+S3: R1/R2?

Note: R1 and R2 represent instrumental responses (left and right arrow key presses), and O1-O4 are outcomes (crisps, popcorn, cashew nuts and nachos). S1-S4 are pictorial stimuli that are associated with outcomes O1-O4, respectively.

S-O-R theory makes a very different prediction: the stimulus compounds should activate outcome representations that prime both responses equally. Furthermore, only the identity of the outcomes (not their values) will be activated - this is the assumption that allows S-O-R theory to explain the usual insensitivity to devaluation (such as that observed in Experiment 3). Thus, S-O-R theory predicts that the stimuli will induce conflict, and no bias will be observed. That is, participants should be equally likely to choose R1 and R2 in the presence of S1+S4 or S2+S3 (as Rescorla, 1994b found in rats).

4.2.1 Method

The method was the same as Experiment 3, except in the following respects.

Participants. Thirty-three UNSW Australia undergraduates (20 female, aged 17-24; *M* = 19.00, *SEM* = 0.26 years) participated for course credit. The experiment was approved by the UNSW Australia School of Psychology Human Research Ethics Advisory Panel.

Apparatus and materials. Cobs natural sea salt popcorn (80g), Doritos original salted nachos (170g), Smith's original crisps (170g) and Nobby's salted cashew nuts (300g) were used as props and for the outcome devaluation procedure.

Procedure

Instrumental training. After completing initial liking ratings, participants were instructed that they could earn the four outcomes (crisps, popcorn, cashew nuts and nachos) by pressing the left and right arrow keys. One outcome associated with each response was scheduled to be available on each trial (outcome availability on any given trial was random). R1 was followed by either O1 or O3, depending on which outcome was available. R2 similarly produced either O2 or O4 on each trial. The outcomes associated with each response were presented in red or green (counterbalanced) to help discriminate them. All other aspects of instrumental training and the subsequent instrumental knowledge test were identical to Experiment 1.

Outcome devaluation. Outcomes O3 and O4 were then devalued using the cloves procedure of Experiment 3. The non-devalued outcomes (O1 and O2) were sampled first, followed by the devalued outcomes (O3 and O4). The outcomes were randomly sampled within this constraint.

Transfer test. Each transfer test trial presented compound cues containing S1 and S4 (pictures of outcomes O1 and O4), or S2 and S3 (pictures of O2 and O3). The cues were presented at the top centre and bottom centre of the screen, with cue location counterbalanced across trials. Participants were instructed that the cue location was not important. After 3000ms, the choice symbol ("← or →") was centrally presented, between the two cues, until participants performed a left or right arrow key response. There were four trial types (S1+S4 and S2+S3, with counterbalanced cue location). Each trial was presented once per cycle, and there were eight cycles (32 trials in total). Trial order was random within each cycle. Participants completed one practice cycle before continuing on to the main transfer test.

4.2.2 Results

Exclusions. Seven participants were excluded for failing the instrumental knowledge tests. The mean transfer test scores for these participants are provided in Appendix 2. Another participant was excluded for giving high liking ratings ($M = 4$) to the devalued outcomes in the post-devaluation liking test. This participant also showed no evidence of devaluation in the post-experimental questionnaire.

Liking ratings. Figure 4.1 shows the mean liking ratings for the valued (O1, O2) and devalued (O3, O4) outcomes, at the start of the experiment (pre-devaluation) and immediately after the devaluation procedure (post-devaluation). The results are very similar to those reported in Chapter 3. There was a main effect of liking test, with participants giving higher liking ratings in the pre-devaluation liking test than the post-devaluation liking test, F (1, 24) = 5.48, $p < .03$, $\eta_p^2 = .19$. There was also a main effect of outcome, with the valued outcome receiving higher ratings than the devalued outcomes when collapsed across liking tests, F (1, 24) = 94.32, $p < .001$, $\eta_p^2 = .80$. Most importantly, there was an interaction between the liking test and outcome variables, F (1, 24) = 148.39, $p < .001$, $\eta_p^2 = .86$. Bonferroni-corrected pairwise comparisons revealed that the outcomes were rated equally before devaluation, $t < 1$, and that the still-valued outcomes received higher ratings than the devalued outcomes after devaluation, t (24) = 18.11, $p < .001$, 95% CI = [4.29, 5.39].

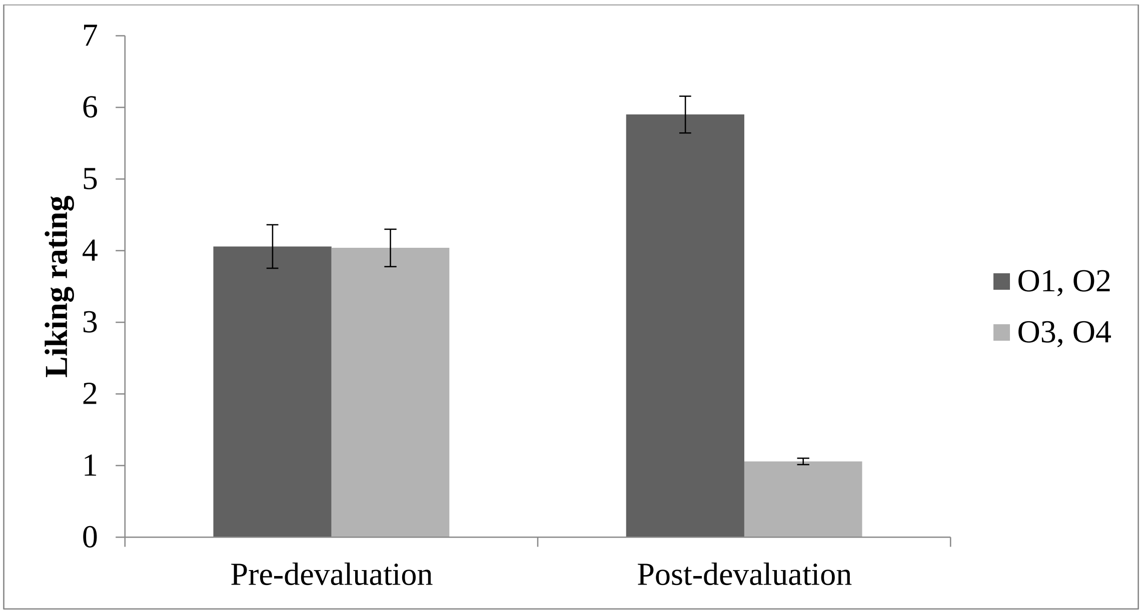

Figure 4.1 Mean liking ratings in Experiment 7. Ratings were taken for each outcome (O1-O4) at the start of the experiment (pre-devaluation), and immediately after the devaluation procedure (post-devaluation). Ratings of one and seven represent wanting to eat the outcome "Not at all" and "Very much", respectively. Error bars represent SEM.

Transfer test. Figure 4.2 shows the transfer test results. The S1+S4 compound elicited more R1 responses than the S2+S3 compound, t (24) = 10.85, $p < .001$, 95% CI = [62.75, 92.25].

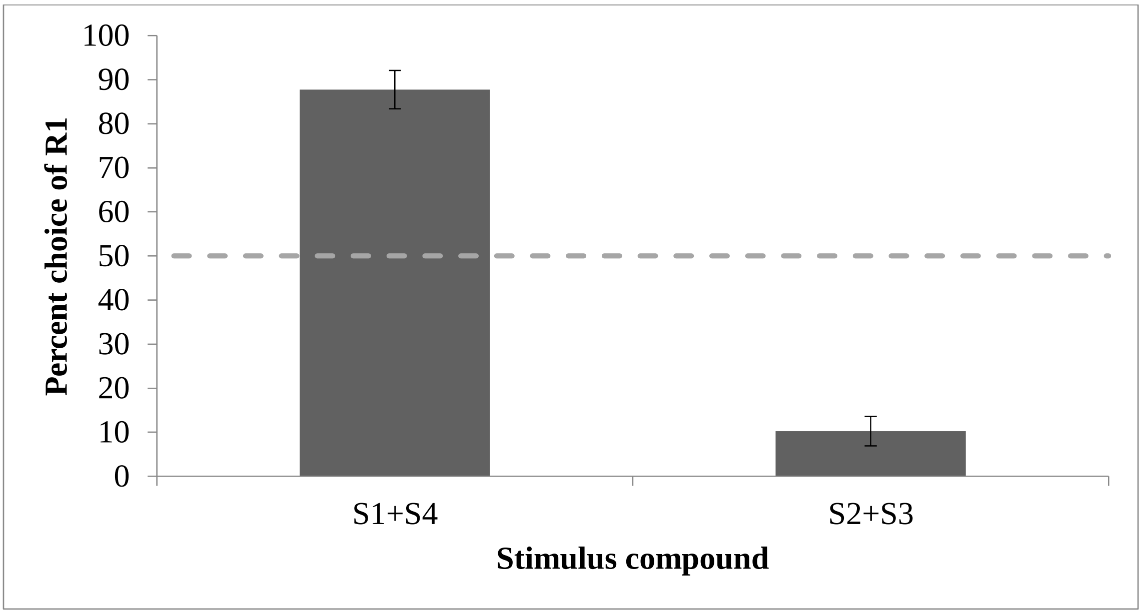

Figure 4.2. Transfer test results of Experiment 7. Response choice was tested in the presence of compound stimuli depicting outcomes O1 and O4 (S1+S4), and O2 and O3 (S2+S3). The 50% mid-point represents no bias in response choice. Scores above 50% demonstrate a bias towards R1, which was paired with O1 (valued) and O3 (devalued) during instrumental training. Scores below 50% represent a bias towards R2, which was paired with O2 (valued) and O4 (devalued) during instrumental training. Error bars represent SEM.

4.2.3 Discussion

In Experiment 7, response choice during the transfer test was highly influenced by outcome value. When a stimulus compound signalled two outcomes that varied in their value, participants selectively responded for the high-value signalled outcome. These data speak against the dominant S-O-R model of PIT, in which outcome value is not activated in the S-O-R chain (Hogarth & Chase, 2011; Holland, 2004; Rescorla, 1994b). Instead, the results support a goal-directed model in which PIT is highly sensitive to outcome devaluation, at least when both responses are associated with a cued outcome.

Notably, the results are quite different from those of a related study by Rescorla (1994b), where rats showed insensitivity to devaluation in the presence of compound stimuli. One possibility is that this discrepancy reflects a fundamental difference in the processes that underlie PIT in animals (where an S-O-R process dominates) versus humans (where a propositional process dominates). Alternatively, the discrepancy may reflect procedural differences between the two experiments. Perhaps the current devaluation manipulation was especially strong and thus better able to influence responding. Another possibility is that the complex design rendered the task simply too difficult for Rescorla's rats. However, we remain cautious in accepting either of these accounts. For one thing, many experiments have shown strong devaluation effects using the lithium chloride-induced aversion procedure that Rescorla used. For another, rats have been shown to successfully solve other tasks that are of a similar complexity (Rescorla, 1991). More research is needed in both humans and rats to progress this discussion beyond speculation.

4.3 Experiment 8

The results of Experiment 7 suggest that PIT is sensitive to outcome value when outcomes associated with both responses are cued on every transfer test trial. The data appear to provide unique support for the propositional EU account of PIT, because they suggest that PIT is highly sensitive to outcome value when outcome probability Op is controlled. That is, participants selectively respond for the cued, high-value outcome when both instrumental responses are signalled on every transfer test trial. However, S-O-R theory predicted only the null result in Experiment 7; an S-O-R mechanism had no opportunity to influence response choice during the transfer test. Experiment 8 therefore sought positive evidence for the S-O-R account. To this end, a novel PIT procedure was designed to ensure that the propositional and S-O-R accounts made *opposite* predictions. Hence, the theories were directly set against one another. Table 4.2 shows the design.

Table 4.2

Design of Experiment 8

Instrumental training	Transfer test
R1 – O1-/O3+	S1+S3: R1/R2
R2 – O2-/O3+	S2+S3: R1/R2

Note: R1 and R2 represent instrumental responses (left and right arrow key presses), and O1-O3 are outcomes (crisps, popcorn and cashew nuts). '+' and '-' denote rewarding and aversive outcomes, respectively. S1-S3 are pictorial stimuli that are associated with outcomes O1-O3, respectively.

During instrumental training, participants learnt to perform two instrumental responses (R1 and R2) to earn three different food outcomes: crisps, popcorn and cashew nuts. Two outcomes, O1- and O2-, were rendered aversive prior to training by coating them in ground cloves and olive oil. Outcome O3+ remained positive. R1 responses were followed by outcome O1- on some trials, and O3+ on others (R1-O1-/O3+), while R2 produced O2- or O3+ (R2-O2-/O3+). Response choice (R1 versus R2) was finally assessed in the presence of pictorial stimulus S3 (depicting O3+) in compound with either S1 or S2 (representing O1- and O2-).

A unique aspect of the procedure described above is in the use of a pre-training devaluation procedure. Outcome value was established before instrumental training to test whether pre-training devaluation would foster an automatic S-O-R mechanism in the current design. The S-O-R mechanism is often assumed to operate via an ideomotor mechanism, where anticipating an outcome *automatically* primes associated instrumental responses, regardless of the current valence of the outcome (e.g., de Wit & Dickinson, 2009, 2015; Hogarth et al., 2013; Watson et al., 2015). As noted above, this is precisely why the ideomotor mechanism is favoured as a mechanism to explain the insensitivity to devaluation that is usually seen in PIT experiments. Much of the empirical support for ideomotor theory comes from studies that used motivationally neutral outcomes (see Shin, Proctor, & Capaldi, 2010 for a review). There is also some evidence to suggest that ideomotor priming effects occur for even undesirable or aversive outcomes (Beckers, De Houwer, & Eelen, 2002; Eder et al., 2014). These results provide support for the ideomotor account because, as Dickinson (2016) points out, even aversive outcomes should automatically prime associated motor responses according to ideomotor theory. A straightforward prediction of this interpretation of S-O-R theory, then, is that PIT effects should be observed for undesirable outcomes, even when outcome value is established before instrumental training. Experiment 8 tested this prediction. Evidence of automaticity following a pre-training devaluation procedure would provide especially strong evidence for S-O-R theory.

S-O-R theory (as interpreted by Dickinson, 2016) predicts that the stimulus compounds will prime and trigger the instrumental responses that produced the cued outcomes, irrespective of outcome value. Thus, an S1+S3 compound will trigger R1, because R1 is associated with *both* signalled outcomes O1- and O3+, whereas R2 is only associated with one of the cued outcomes – O3+. By the same logic, an S2+S3 compound will increase R2 responses. The propositional EU account proposes that response choice reflects a controlled inference, and therefore that participants may seek to avoid the aversive outcomes. Hence, the S1+S3 compound should *decrease* R1 responses, because participants will try to avoid the cued, aversive O1-. Conversely, participants should preferentially choose R2 to earn the positive O3+. By the same logic, the S2+S3 compound should similarly decrease R2 responses (and increase R1 responses). The critical novel feature of this design is that both responses share the positive O3+ with the stimulus compounds S1+S3 and S2+S3. Outcome probability will therefore be high for both responses on all test trials, allowing the opportunity to detect a role of outcome value. Baseline response choice should not be biased in either direction, because the instrumental responses are equally associated with a valued and devalued outcome.

4.3.1 Method

The method was the same as Experiment 7, except in the following respects.

Participants. Twenty-four Plymouth University psychology undergraduates (22 females, aged 18-25; *M* = 19.21, *SEM* = 0.35 years) completed the experiment for course credit. The experiment was approved by the Plymouth University Ethics Committee.

Apparatus and materials. Walker’s extra crunchy ready salted crisps (150g), Tyrrell’s sea salted “Poshcorn” (70g), and Sainsbury’s salted jumbo cashew nuts (400g) were used for the taste test.

Procedure

Taste and liking test. Participants initially sampled the crisps, popcorn and cashew nuts that were available to win. The foods were counterbalanced with respect to the outcome for which they served (O1-, O2- or O3+). Participants always sampled O3+ first, before the other outcomes were revealed. The aversive outcomes O1- and O2-, coated in cloves and olive oil, were then sampled randomly. After the taste test, participants rated their desire to eat each food (1 = “Not at all”, 7 = “Very much”), with the questions presented in a random order. The outcomes were removed after these ratings had been provided.

Instrumental training. Instrumental training began with the following instructions: “In this part of the task, you can earn the three outcomes shown before by pressing the left or right arrow keys. Your task is to learn which keys earn each outcome.” There were 48 trials. Each trial began with a centrally presented choice symbol ("← or →"), which remained until participants pressed either the left or right arrow key. Responses were followed by the statement "You earn one [crisps/popcorn/cashews] point", depending on the reward earned. The rewarding outcome O3+ was scheduled to be available on half of the trials, and the aversive outcomes O1- and O2- on the remaining trials. The trials were randomly distributed throughout training. When outcome O3+ was available, either response produced this outcome. When the aversive outcomes were available, left arrow key responses (R1) produced O1-, and right arrow key responses (R2) produced O2-. All other aspects of instrumental training were the same as Experiment 7.

Instrumental knowledge test. Following instrumental training, participants were asked which key produced each outcome. They chose between the options "Left arrow", "Both" and "Right arrow", using the mouse. Their answer was outlined in red for 1000ms, and participants then rated their confidence between one ("not at all") and seven ("very confident"). The questions were randomly ordered.

Transfer test. The transfer test began with instructions that were very similar to the instructions given in Experiment 7; participants were told that they could continue to earn the three outcomes by pressing the left or right arrow key, but that they would now only be told how many they had earned at the end of the experiment. They were also informed that pictures would be presented, and that would be required to eat all of the food they earned at the end of the experiment.

Each trial began with the presentation of pictorial stimuli representing outcome O3+ (S3), and either O1- (S1) or O2- (S2).The stimuli were presented at the top centre and bottom centre of the screen. After 3000ms, the choice symbol ("← or →") was presented centrally, between the two stimuli, and participants were required to choose the left or right arrow key. There were eight cycles of four trials (32 trials). In each cycle, S3 was presented with S1 twice and with S2 twice, with counterbalanced cue location. All other aspects of the transfer test were identical to Experiment 7. After completing the transfer test, participants completed another instrumental knowledge test, a Pavlovian knowledge test for the three stimuli, and the post-experimental knowledge test of the previous experiments.

4.3.2 Results

Knowledge tests. Twelve participants failed the first or second instrumental knowledge test and were labelled “unaware”. The remaining 12 participants were considered “aware”. Considering there was an equal number of participants in each group, rather than rejecting the unaware participants, awareness was included as a variable in the analysis.

Liking ratings. Figure 4.3 shows the liking ratings for the aversive (O1-, O2-) and rewarding (O3+) outcomes for participants who were aware and unaware of the instrumental contingencies. Most importantly, O3+ received higher ratings than O1-/O2-, F (1, 22) = 490.62, $p < .001$, $\eta_p^2 = .96$. There was no effect of group or interaction between the outcome and group variables, Fs < 1.

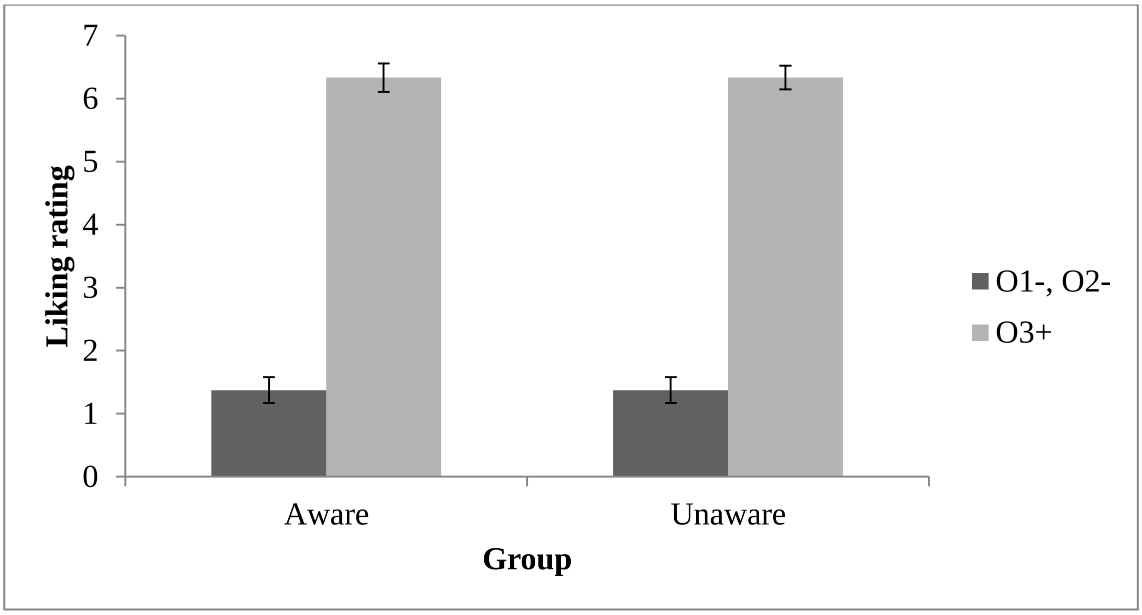

Figure 4.3 Mean liking ratings in Experiment 8. Ratings of one and seven represent wanting to eat the outcome "Not at all" and "Very much", respectively. The data are divided according to whether participants demonstrated awareness of the instrumental contingencies. Error bars represent SEM.

Transfer test. Figure 4.4 shows the transfer test results. There was a main effect of stimulus compound, with participants performing more R1 responses in the presence of the S2+S3 compound than the S1+S3 compound, *F* (1, 22) = 15.99, $p < .001$, $\eta_p^2 = .42$. There was no main effect of awareness, $F < 1$, but there was an interaction between awareness and stimulus compound, *F* (1, 22) = 21.60, $p < .001$, $\eta_p^2 = .50$. Bonferroni-corrected pairwise comparisons revealed that the S2+S3 compound elicited more R1 responses than the S1+S3 compound in the aware group, *t* (22) = 6.11, $p < .001$, 95% CI = [41.30, 83.70]. No effect of stimulus compound was observed in the unaware group, *t* < 1.

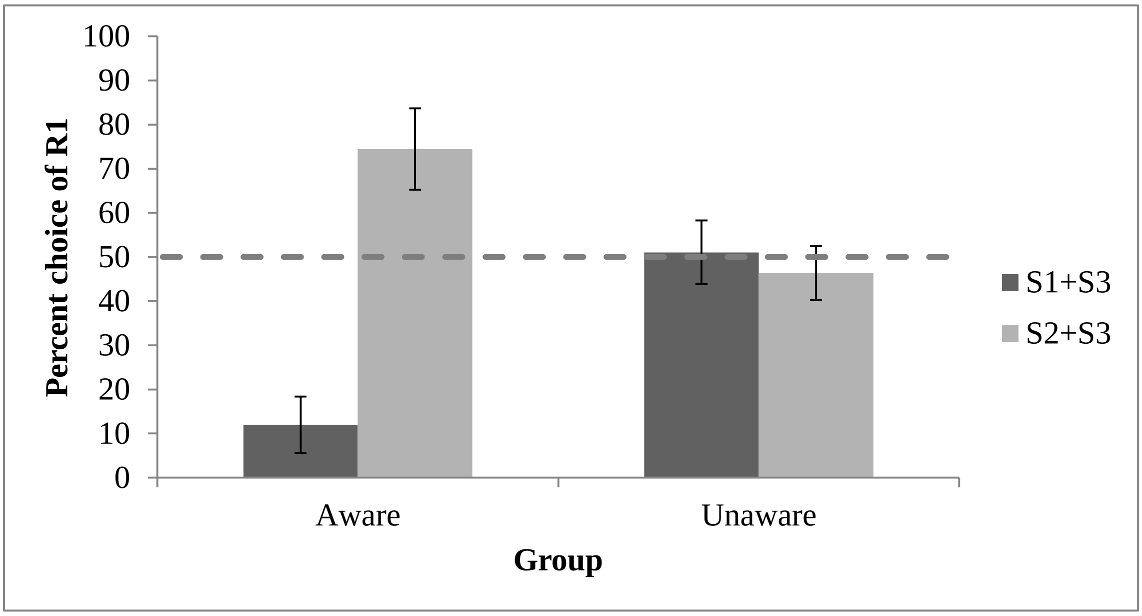

Figure 4.4. Transfer test results of Experiment 8. Response choice was tested in the presence of stimulus compounds depicting either O1- or O2- with O3+ (S1+S3, S2+S3). The 50% mid-point represents no bias in response choice. The data are subdivided according to whether participants reported accurate knowledge of the instrumental contingencies. Error bars represent SEM.

4.3.3 Discussion

Experiment 8 used a novel PIT procedure that directly set the propositional and S-O-R theories against one another. Each transfer test trial signalled a rewarding outcome that was common to both instrumental responses, and an aversive outcome that was unique to one response. The stimuli primed not the response that was most strongly associated with the signalled outcomes, but the response that *did not* produce the cued, aversive outcome. These data provide compelling evidence to suggest that, at least under these circumstances, PIT is highly sensitive to outcome value.

Interestingly, sensitivity to outcome value was only apparent in participants who demonstrated explicit awareness of instrumental contingencies. That is, the stimulus compounds failed to bias response choice (in either direction) in participants who failed the instrumental contingency knowledge tests; these participants responded at chance. The finding that explicit contingency knowledge is necessary to observe an effect of stimulus compound in the current design accords with observations in typical PIT procedures (Hogarth et al., 2007; Lovibond et al., 2015; Talmi et al., 2008; Trick et al., 2011). Moreover, it supports the suggestion that propositional knowledge plays an important role in PIT.

4.4 Experiment 9

In Experiment 8, each transfer test trial signalled two outcomes that varied in their value. Under these circumstances, PIT was highly sensitive to outcome value. This procedure can be contrasted to typical PIT experiments (such as Experiment 3), in which only one high or low value outcome is cued on each trial. These conditions typically promote insensitivity to outcome devaluation (e.g., Hogarth & Chase, 2011; Hogarth, 2012; Watson et al., 2014). Experiment 9 sought to test whether this difference in experimental design is crucial to whether PIT is sensitive to devaluation or not.

The current design specifically tested whether cueing the positive outcome O3+ on every trial is necessary to observe the effect seen in Experiment 8. The experiment followed a similar procedure to Experiment 8; two instrumental responses were initially trained to predict unique aversive food outcomes (O1- and O2-), as well as a common rewarding outcome O3+ (R1-O1-, O3+, R2-O2-, O3+). For one group (Compound-cue), response choice was then tested in the presence of the compound stimuli used in Experiment 8: S1+S3 and S2+S3. For the other group (Single-cue), the aversive outcomes were simply presented twice on each test trial: S1+S1 and S2+S2. The Compound-cue group were expected to replicate the effect observed in Experiment 8. That is, the S1+S3 compound was expected to increase choice of R2, and the S2+S3 compound was expected to increase choice of R1. If the sensitivity to outcome value observed in Experiment 8 was because the positive O3+ was cued on every test trial, then the effect should not be observed in the Single-cue group. The Single-cue condition is more similar to the standard PIT procedure in which only one outcome and response is cued on every test trial, and so might be more likely to produce insensitivity to devaluation. Insensitivity to devaluation would be observed if the S1+S1 compound increased choice of R1 compared to the S2+S2 compound. Similarly, the S2+S2 compound should increase choice of R2 compare to the S1+S1 compound.

4.5.1 Method

The method was the same as Experiment 8, except in the following respects.

Participants. Forty-one participants from Plymouth University (23 females, aged 18-28; *M* = 20.54, *SEM* = 0.34 years) completed the experiment for £4.

Procedure. A large proportion of participants failed the contingency knowledge tests in Experiment 8. Several steps were therefore taken to make the instrumental contingencies more memorable in the current experiment. First, the instrumental responses were changed to the “A” and “L” keys instead of the left and right arrow keys. The rationale here was that the responses required the use of different hands, which should make them more distinguishable. The instructions for the instrumental training phase were also more explicit: “In this part of the task, you can earn the three outcomes shown before by pressing the A or L key. Both keys will produce [O3+]. Your task is to learn which keys produce [O1-] and [O2-]. Press any key to begin.” The terms in brackets were replaced by their respective outcomes. Instrumental training was identical to Experiment 8, except that the choice symbol was replaced with “Choose a key: A or L?”, and participants selected either the A or L key instead of the arrow keys. These changes were also made to the transfer test. The instrumental knowledge tests were also identical to Experiment 1, except that participants chose between the three new options - A key, L key, or both.

The transfer test for the compound-cue group was identical to the transfer test used in Experiment 8. The transfer test for the single-cue group was also the same, except that stimulus S1 or S2 was presented twice on every trial, in place of S3. After the transfer test, participants completed the instrumental and Pavlovian knowledge tests of Experiment 8, except that the Single-cue group were not shown S3 during the Pavlovian knowledge test (since it was not presented during the experiment at all for this group).

4.5.2 Results

Exclusions. Fourteen participants were excluded for failing the instrumental (*N* = 10) or Pavlovian (*N* = 3) knowledge tests, or both (*N* = 1). Given the high proportion of excluded participants, the transfer test results for these participants are provided in Appendix 2. One additional participant was excluded for giving a higher liking rating (6) to one of the aversive outcomes than to the positive outcome (5). This left a total of 26 participants (Single-cue = 13, Compound-cue = 13), whose data were entered into the remaining analyses.

Liking ratings. Figure 4.5 shows the liking rating data for the aversive (O1-, O2-) and rewarding (O3+) outcomes in each group. Most importantly, O3+ received higher liking ratings than O1- and O2- when collapsed across groups, F (1, 24) = 382.61, $p < .001$, $\eta_p^2 = .94$. There was no effect of group, $F < 1$, nor was there an interaction between the outcome and group variables, F (1, 24) = 2.05, $p > .05$, $\eta_p^2 = .08$.

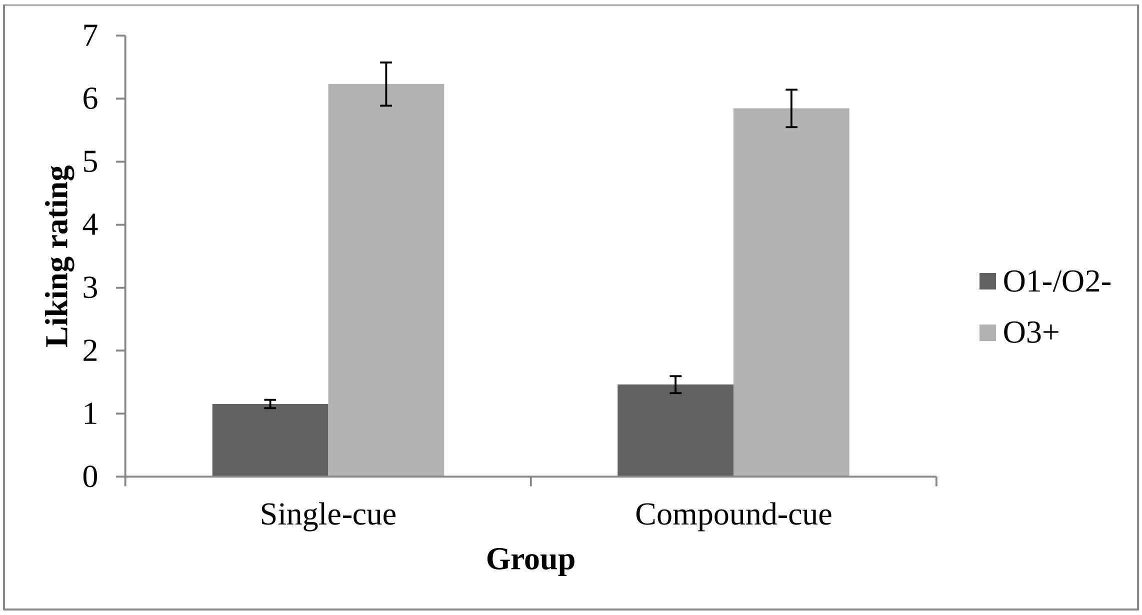

Figure 4.5. Mean liking ratings in Experiment 9. Ratings of one and seven represent to wanting to eat the outcome "Not at all" and "Very much", respectively. Error bars represent SEM.

Transfer test. Figure 4.6 shows the transfer test results. No main effects were observed for stimulus compound, *F* (1, 24) = 2.03, $p > .05$, $\eta_p^2 = .08$, or group, *F* (1, 24) = 1.52, $p > .05$, $\eta_p^2 = .06$. There was a marginal but non-significant interaction between the stimulus compound and group variables, *F* (1, 24) = 3.33, $p = .08$, $\eta_p^2 = .12$.

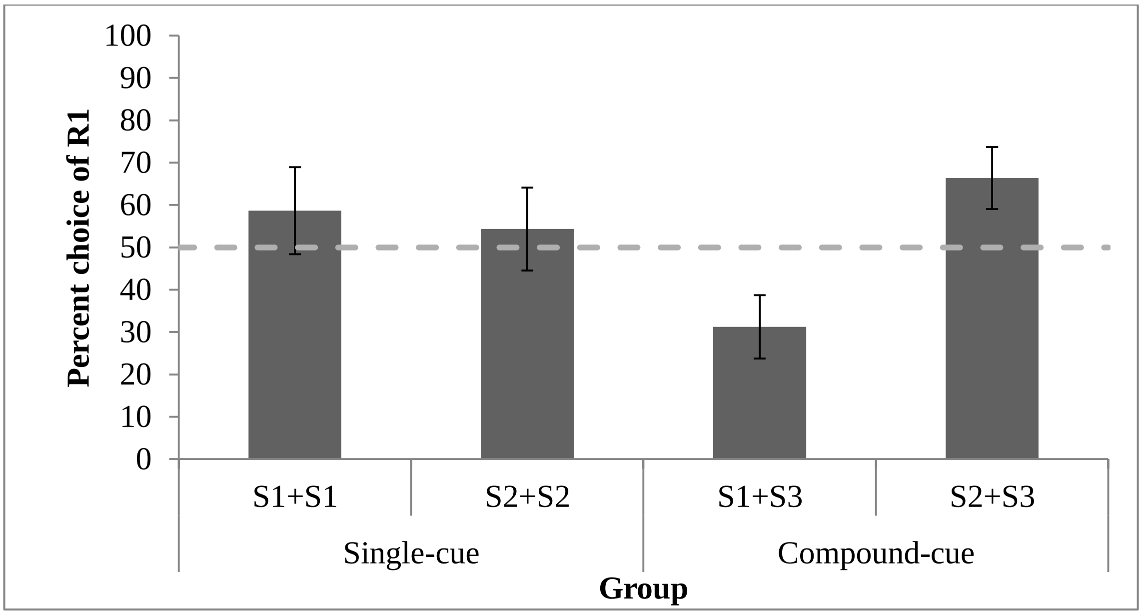

Figure 4.6. Transfer test results of Experiment 9. Response choice was tested in the presence of stimulus compounds S1+S1/S2+S2 (Single-cue), or S1+S3/S2+S3 (Compound-cue). The 50% mid-point represents no bias in response choice. Error bars represent SEM.

The non-significant stimulus compound × group interaction suggests that the effect of the stimulus compounds on response choice did not significantly differ across the two groups. However, Figure 4.6 indicates that an effect of stimulus was observed in the Compound-cue group. It is important to assess this effect statistically to confirm whether the effect observed in Experiment 8 was replicated in the comparable Compound-cue condition of the current experiment. Pairwise comparisons were therefore computed for each group, followed by Bayes Factors for confirmation. In the Compound-cue group, the S2+S3 compound increased the R1 response more than the S1+S3 compound, t (24) = 2.30, $p < .05$, 95% CI = [3.55, 66.64]. There was no significant effect of stimulus in the Single-cue group, $t < 1$.

Given the non-significant stimulus compound × group interaction, Bayes Factors were calculated to confirm the analyses reported above. It was assumed that the effect of stimulus compound in the Compound-cue group would be of a similar magnitude to that obtained in ‘aware’ group of Experiment 8. The mean difference (S2+S3 – S1+S3) for the ‘aware’ group of Experiment 8 was 62.50. The comparable score for the Compound-cue group of the current experiment was 35.10 (*SEM* = 13.82). A half-normal distribution with the standard deviation set as the plausible mean difference score (62.50) produced a Bayes Factor of 9.28. Thus, there was strong evidence to suggest that the S2+S3 compound produced more R1 responses than the S1+S3 compound in the Compound-cue group.

Comparable Bayes Factors were also calculated for the Single-cue group. The mean difference (S2+S2 – S1+S1) for the Single-cue group was -4.43 (*SEM* = 16.62). A half-normal distribution with the standard deviation set as the plausible mean difference score (62.50) produced a Bayes Factor of 0.21. This Bayes Factor is below the critical threshold of one third, and so provides evidence for the null hypothesis. That is, the data support the conclusion that the S2+S2 stimuli did not increase R1 responses compared to the S1+S1 stimuli.

Finally, a Bayes Factor was also calculated to explore whether the Single-cue group showed any evidence of automaticity, which would be revealed if the S1+S1 stimuli increased choice of the R1 response compared to the S2+S2 stimuli. For the purpose of calculating the priors, the maximum plausible effect was assumed to be of an opposite but comparable magnitude to the effect seen in the ‘aware’ group of Experiment 8 (62.50). The mean difference score for the Single-cue group of the current experiment was entered as +4.43 (*SEM* = 16.62), because the means went in the same direction as the experimental hypothesis (i.e. S1+S1 > S2+S2 with the percent choice of R1 as the dependent variable). A uniform distribution (with the upper and lower bound set as 62.50 and 0, respectively) produced a Bayes Factor of 0.42. This value is between the critical thresholds of one third and three, and is therefore inconclusive. That is, there was insufficient evidence for the either the null hypothesis or the experimental hypothesis.

4.5.3 Discussion

Experiment 9 first replicated the effect observed in Experiment 8 in the Compound-cue group. When compound stimuli signalled both a rewarding and an aversive outcome on every test trial, participants demonstrated a selective bias *away* from the cued, aversive outcome. A comparable effect was not observed in the Single-cue group, where the aversive outcomes were cued in the absence of the stimulus that signalled the rewarding outcome. Participants responded at chance under these circumstances; the stimuli failed to bias response choice in either direction.

The non-significant interaction between the stimulus compound and group variables is a weakness of the experiment. One possibility is that the experiment was underpowered. Across the two groups, a relatively high proportion of participants were excluded for failing the contingency knowledge tests, and this would have reduced the experimental power. However, given the clear results observed in each group, it seems unlikely that a replication with greater power would change the overall pattern of results.

Overall, the results suggest that signalling the positive O3+ during the transfer effect may be important to the effect observed in Experiment 8. This supports the suggestion that participants infer that the Pavlovian stimuli that are presented during the transfer test signal which outcomes are available, and which outcomes are not. The experiment was also useful in that the Compound-cue group served to replicate the effect observed in Experiment 8.

It was anticipated that the Single-cue group might have demonstrated the usual insensitivity to devaluation that is seen in typical PIT tasks; only one low-value outcome was cued on every test trial, which resembles the conditions of the transfer test in the standard PIT procedure. Insensitivity to devaluation would have been demonstrated by the *opposite* pattern to that observed in the Compound-cue group; the S1+S1 and S2+S2 stimuli should have increased R1 and R2 responses, respectively. However, this effect was not observed; participants performed at chance during the transfer test, rather than showing any evidence of a standard PIT effect that was insensitive to devaluation. In typical PIT tasks, each response is trained to predict unique outcomes. Each response was also trained to predict different (aversive) outcomes in the current design, but they also predicted the common positive O3+. It is possible that this difference in training is responsible for the null result observed in the Single-cue group (as opposed to a standard PIT effect). Although this is an intriguing possibility, it was not pursued further for two reasons. Firstly, a null result in the Single-cue group would not be particularly informative, even if the interaction reached significance. Secondly, there was a more important issue that needed to be addressed. In particular, a post-training devaluation procedure needed to be employed to further test the effect of outcome devaluation on PIT. This issue was therefore prioritised in Experiment 10.

4.5 Experiment 10

Experiments 8 and 9 demonstrated that when stimulus compounds signalled both an aversive and a rewarding outcome, participants show a clear bias away from the cued, aversive outcome. These data appear to provide unique support for the propositional EU model of PIT. However, there is reason to warrant caution. An important feature of those experiments is that outcome value was established at the start of the experiment, before instrumental training commenced. This can be contrasted to the previous PIT studies that demonstrated insensitivity to devaluation, where the outcomes were devalued after training but before the critical transfer test (Hogarth, 2012; Hogarth & Chase, 2011; Watson et al., 2014).

S-O-R theory often assumes that the Pavlovian stimulus presented during the transfer test activates an ideomotor mechanism, where anticipation of the cued outcome triggers the associated instrumental response. As noted above, S-O-R (ideomotor) theory has been previously interpreted as an automatic mechanism that should operate for even aversive outcomes (Dickinson, 2016). Other researchers have, however, argued that such a system would be highly dysfunctional from an evolutionary perspective (Eder & Hommel, 2013; Eder et al., 2014; Marien, Aarts, & Custers, 2015). Anticipating an aversive outcome might intuitively be expected to suppress responses that produce them, perhaps via the formation of an inhibitory link between the mental representations of the response R and the aversive outcome O (Dickinson, 1994). One possibility, then, is that sensitivity to outcome value was observed in Experiments 8 and 9 because inhibitory links formed between the instrumental responses and aversive outcomes during the instrumental training phase. According to this account, the stimulus compounds presented during the transfer test would have activated the associated outcome representations. The inhibitory links would have then *supressed* the instrumental response that was trained to produce the cued, aversive outcome. Clearly, this account would explain the results of Experiments 8 and 9 very well.

Experiment 10 followed the design of Experiment 8 but with a more standard *post-training* devaluation procedure. This means that all outcomes were positive during training, and so no inhibitory associations should have formed. If the effect observed in Experiment 8 was due to the formation of inhibitory instrumental links with the aversive outcomes, then the opposite result should now be observed. Hence, the S-O-R account predicts that the S1+S3 compound will increase R1 on test, and the S2+S3 compound will increase R2. If participants made a deliberate choice to avoid the aversive outcomes in Experiment 8, however, then response choice should follow the same pattern as in Experiment 8.

4.5.1 Method

The method was the same as Experiment 9, except in the following respects.

Participants. Thirty participants from Plymouth University (21 females, aged 18-23; *M* = 20.50, *SEM* = 0.25 years) completed the experiment in exchange for £4.

Procedure. Participants were initially shown the food outcomes (crisps, popcorn and cashew nuts) in their original packaging and were told that they could win points towards them. The props were placed in front of the computer, in a consonant location to the keys that produced them (i.e., O3+ was placed centrally, and O1- and O2- were placed to the left and right of O3+, respectively). Participants completed liking ratings for each outcome, before the props were removed. All text was presented in black during instrumental training. The devaluation taste test took place after the first instrumental knowledge test (which followed instrumental training). Participants then completed a post-devaluation liking test prior to the transfer test. All participants completed the transfer test that was employed in Experiment 8.

4.5.2 Results

Exclusions. Four participants were excluded for failing the instrumental knowledge tests. Two additional participants were excluded for giving higher liking ratings of O2- or O3- after devaluation than before. The data from the remaining 24 participants were entered into the analyses.

Liking ratings. Figure 4.7 shows the mean liking ratings for the valued (O3+) and devalued (O1-/O2-) outcomes in the first (pre-devaluation) and second (post-devaluation) liking rating test. There was a main effect of liking test, with participants giving higher liking ratings in the pre-devaluation test than the post-devaluation test, *F* (1, 23) = 41.12, $p < .001$, $\eta_p^2 = .64$. There was also a main effect of outcome, with the valued O3+ receiving higher liking ratings than the devalued O1-/O2- when collapsed across liking tests, *F* (1, 23) = 103.72, $p < .001$, $\eta_p^2 = .82$. Most importantly, there was an interaction between the liking test and outcome variables, *F* (1, 23) = 161.51, $p < .001$, $\eta_p^2 = .88$. Bonferroni-corrected pairwise comparisons revealed that the outcomes were rated equally before devaluation, $t < 1$, and that O1-/O2- received lower ratings than O3+ after devaluation, *t* (23) = 23.91, $p < .001$, 95% CI = [4.74, 5.64].

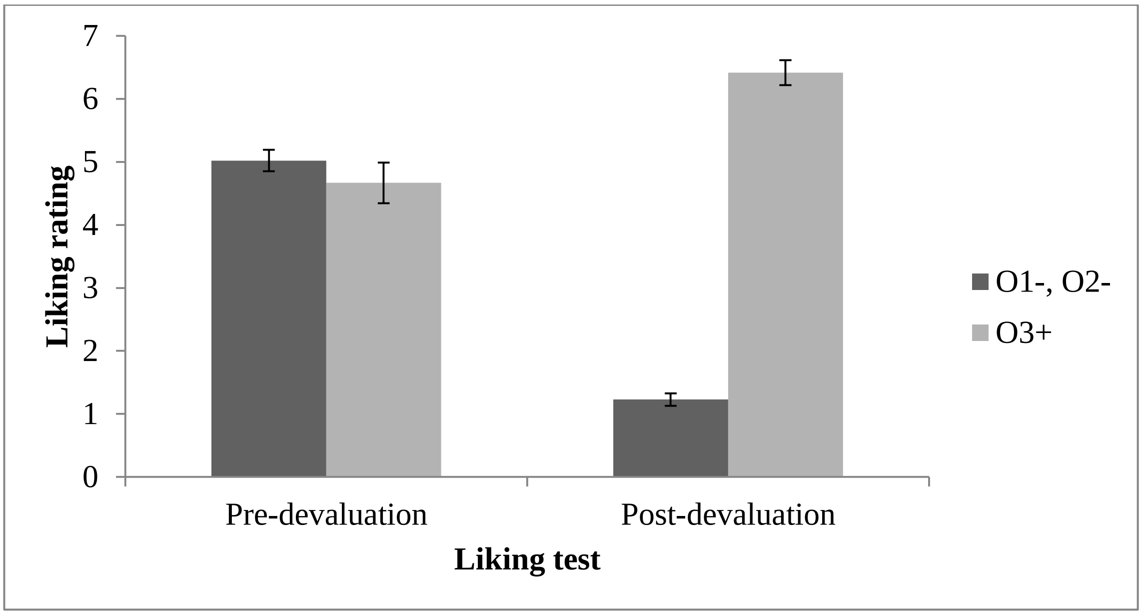

Figure 4.7. Mean liking rating in Experiment 10. Ratings were taken for each outcome (O1-, O2- and O3+) at the start of the experiment (pre-devaluation), and immediately after the devaluation procedure (post-devaluation). Ratings of one and seven represent wanting to eat the outcome "Not at all" and "Very much", respectively. Error bars represent SEM.

Transfer test. Figure 4.8 shows the transfer test results. Most importantly, the S2+S3 compound increased R1 responses compared to the S1+S3 compound, *t* (23) = 6.58, $p < .001$, 95% CI = [39.28, 75.30].

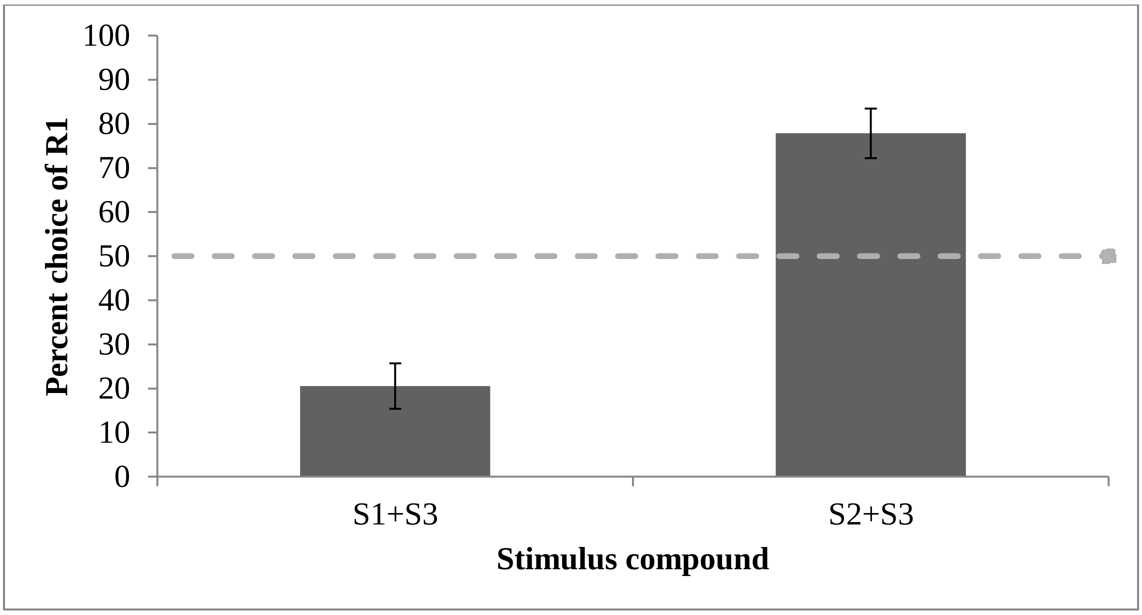

Figure 4.8. Transfer test results of Experiment 10. Response choice was tested in the presence of stimulus compounds depicting either O1- or O2- with O3+ (S1+S3, S2+S3). The 50% mid-point represents no bias in response choice. Error bars represent SEM.

4.5.3 Discussion

When compound stimuli signalled two outcomes, participants demonstrated a strong tendency to selectively avoid the cued, devalued outcome. This is consistent with the results of Experiments 8 and 9. Furthermore, it demonstrates that the effect can be replicated even when the outcomes are devalued after the instrumental training phase. Together, these results are at odds with the claim that PIT effects are mediated by a representation of the outcome’s identity, but not its value (Hogarth et al., 2013; Hogarth & Chase, 2011; Holland, 2004). The data suggest that not only does the stimulus retrieve the outcome’s value, but that PIT is highly sensitive to *changes* in outcome value, at least when multiple outcomes are cued.

4.6 Experiment 11

Experiments 8-10 set the propositional and S-O-R accounts of PIT against one another using a novel experimental design. When stimulus compounds signalled both an aversive and a rewarding outcome on every test trial, response choice was highly sensitive to outcome value. Under these circumstances, participants sought to *avoid* the cued, aversive outcome. This effect was observed regardless of whether the outcomes were devalued before or after the instrumental contingencies were established. These data provide strong support for the propositional EU account of PIT, in which participants selectively respond for the signalled outcome that has the highest value. That is, response choice appears to reflect an integration of knowledge about the perceived probability (Op) and value (Ov) of each outcome.

The data support a goal-directed account of PIT, where cue-elicited response choice is sensitive to changes in outcome value, at least when multiple outcomes are signalled. It should be noted, however, that this does not demonstrate that an automatic S-O-R process plays *no* role in PIT. It is certainly still possible that both goal-directed, propositional and automatic S-O-R processes contribute to PIT effects, but that Experiments 8-10 were not optimised to detect the latter process. Experiment 11 tested this possibility by incorporating the concurrent load task used in Experiment 2 into the design of Experiment 10. The aim here was to make it difficult for participants to engage in high-level, propositional reasoning throughout the transfer test. If response choice is mediated in part by an automatic S-O-R mechanism, then this mechanism should be revealed under concurrent load. That is, S-O-R theory predicts the *opposite* effect to that observed in Experiment 10 under concurrent load. If PIT effects are entirely mediated by a goal-directed process, however, then a generalised impairment in response choice should be observed. Crucially, no evidence of automaticity would be expected.

Participants were randomly allocated to a No Load or Load condition at the start of the experiment. The concurrent load task manipulation was applied during the transfer test (in much the same way as in Experiment 2). The No Load group were expected to replicate the results of Experiment 10. Based on the results of Experiment 2, the concurrent load task was also expected to effectively reduce participants' ability to use controlled reasoning processes. The propositional account predicts that the effect observed in Experiment 10 will be reduced to chance performance in the Load condition. If response choice is partly mediated by an S-O-R mechanism, however, then automaticity should now be observed. That is, the S1+S3 and S2+S3 compounds should now bias response choice towards R1 and R2, respectively.

4.6.1 Method

The method was the same as Experiment 10, except in the following respects.

Participants. Fifty-five participants from Plymouth University (35 females, aged 18-30; *M* = 21.15, *SEM* = 0.41 years) completed the experiment for £4. Participants were randomly allocated to the No Load or Load group at the start of the experiment.

Procedure

Practice concurrent load task. After providing informed consent, both groups completed 10 practice trials of the concurrent load task, which was very similar to the task used in Experiment 2. In brief, each trial began with the presentation of either the letter 'M' or 'C' at the top centre and bottom centre of the screen, while six unique, single-digit, randomly-chosen numbers were played at 330ms intervals through participants' headphones. On-screen instructions to "Press the key" were then presented centrally (between the two letter stimuli), until participants selected either the M or C key. A number that was part of the sequence heard at the start of the trial was then presented on-screen, and participants were required to select the number that came *next* in the sequence that they had heard. The trials were separated by 750-1250ms intervals. Participants wore headphones throughout the duration of the experiment. They subsequently completed the liking ratings, instrumental training and knowledge test, and the outcome devaluation procedure of Experiment 10.

Transfer test. All participants initially received the instructions that were given for the transfer test of Experiment 10. The Load group were also informed that numbers would be presented through their headphones and that when a number was presented on-screen, they should select the number that came *next* in the sequence. The No Load group were told to simply select the number that appeared on the screen. Both groups were informed that the trials would repeat at the end of the experiment if they responded to the probe number incorrectly, and so they should respond accurately to complete the experiment on time. In truth, incorrect trials were not repeated. The instruction was given simply to encourage participants to fully engage with the concurrent load task.

The transfer test followed a similar format to Experiment 10. Each trial began with two pictorial stimuli representing the common rewarding outcome O3 (S3), and either the devalued O1 (S1) or O2 (S2). Immediately following the onset of the stimuli, the Load group heard six numbers through their headphones. The numbers were presented in the same way as in the practice load task. The No Load group did not hear any numbers, but were required to wait for the equivalent time (1980ms). For both groups, the statement “Choose a key: A or L?” was then presented centrally, between the stimuli, until an instrumental response was selected. Finally, a number was presented on-screen, and the Load group were required to select the number that came next in the sequence that was presented at the start of the trial. The No Load group simply selected the number shown. All other aspects of the transfer test were identical to Experiment 10. Finally, participants completed the instrumental and Pavlovian knowledge tests of the previous experiments, and were fully debriefed at the end.

4.6.2 Results

Exclusions. Sixteen participants were excluded for failing the instrumental (*N* = 10) or Pavlovian (*N* = 3) knowledge tests, or both (*N* = 3). Given the high proportion of participants who failed the contingency knowledge tests, the transfer test results for these participants are provided in Appendix 2. A further two participants were excluded for giving higher liking ratings for a devalued outcome in the post-devaluation liking test than in the pre-devaluation liking test. The data from the remaining 37 participants (No Load = 17, Load = 20), were entered into the analyses.

Liking ratings. Figure 4.9 shows the mean pre- and post-devaluation liking ratings for the valued (O3+) and devalued (O1-, O2-) outcomes in each group. There was a main effect of liking test, with higher ratings given in the pre-devaluation test than the post-devaluation test, $F\ (1, 35) = 29.78$, $p < .001$, $\eta_p^2 = .46$. There was also a main effect of outcome, with the higher ratings given to the valued outcome than the devalued outcome when collapsed across groups and the two tests, $F\ (1, 35) = 183.19$, $p < .001$, $\eta_p^2 = .84$. There was no main effect of group, $F < 1$. Most importantly, there was a significant interaction between the liking test and outcome variables, $F\ (1, 35) = 229.10$, $p < .001$, $\eta_p^2 = .87$. No interactions with group were observed, *F*s < 1. Bonferroni-corrected pairwise comparisons revealed that the outcomes were rated equally in the pre-devaluation liking test, $t < 1$, and the valued O3+ received significantly higher liking ratings than the devalued O1-/O2- in the post-devaluation liking test, $t\ (35) = 26.05$, $p < .001$, 95% CI = [4.76, 5.56].

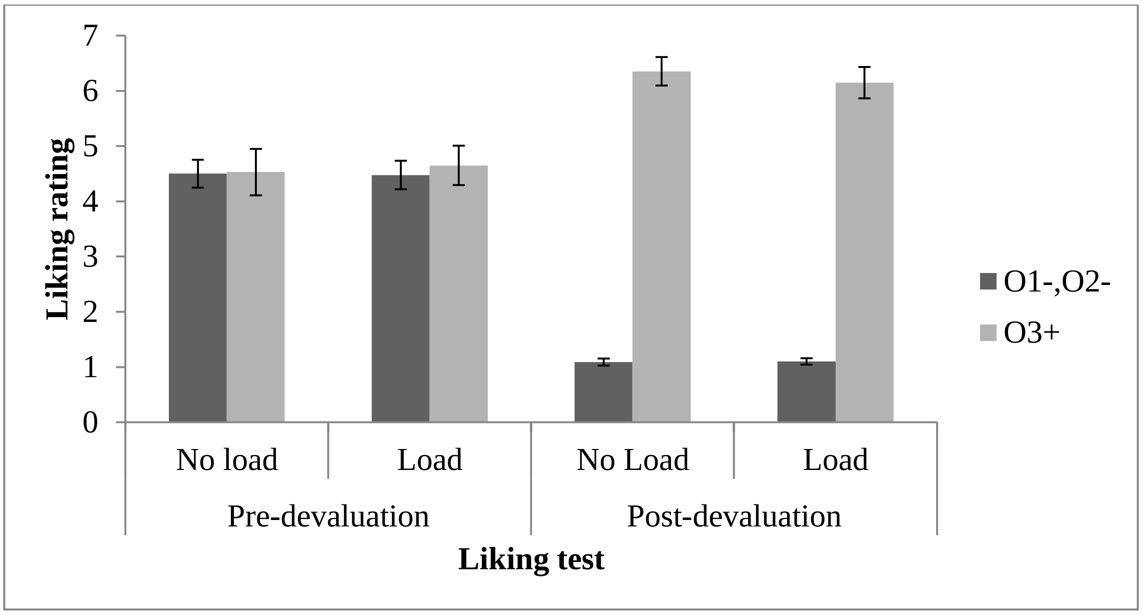

Figure 4.9. *Pre- and post-devaluation liking ratings in Experiment 11. Ratings of one and seven represent wanting to eat the outcome "Not at all" and "Very much", respectively. Error bars represent SEM.*

Transfer test

Response choice. Figure 4.10 shows the transfer test results of Experiment 11. There was a main effect of stimulus compound, with the S2+S3 compound eliciting more R1 responses than the S1+S3 compound, F (1, 35) = 5.78, p = .02, η_p^2 = .14. A main effect of group was not observed, $F < 1$, but there was a significant interaction between the stimulus compound and group variables, F (1, 35) = 6.45, p = .02, η_p^2 = .16. Bonferroni-corrected pairwise comparisons revealed an effect of stimulus compound in the No Load group, t (35) = 3.36, p = .002, 95% CI = [9.04, 36.55], but not in the Load condition, $t < 1$.

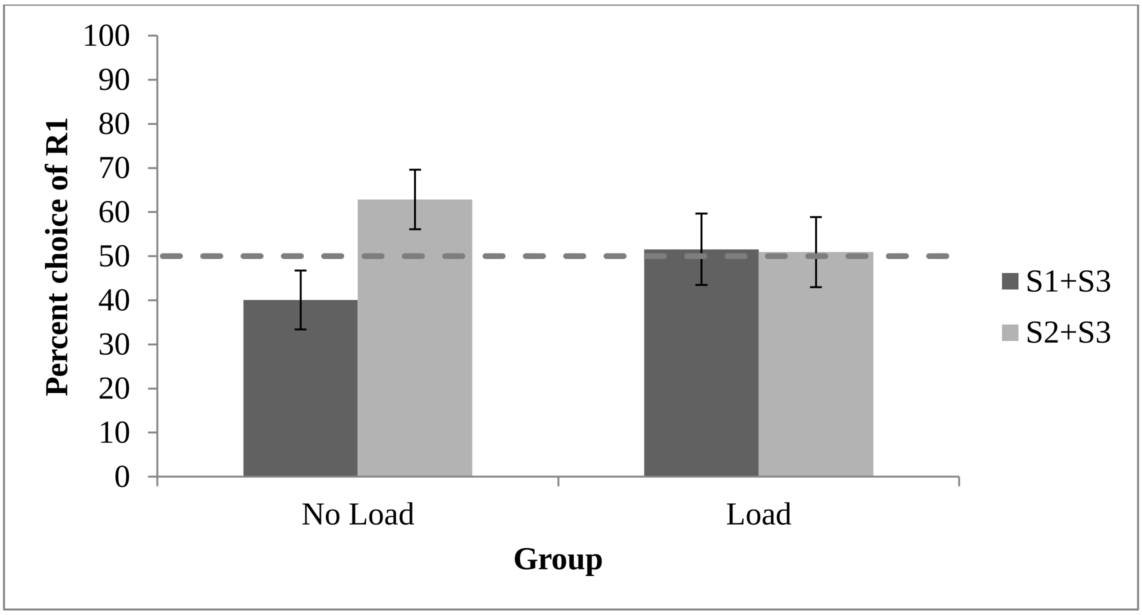

Figure 4.10. Transfer test results of Experiment 11. The 50% mid-point represents no bias in response choice. Scores above 50% demonstrate a bias towards R1, while scores below 50% represent a bias towards R2. Error bars represent SEM.

A Bayes Factor was calculated to determine whether the null effect of stimulus compound in the Load group was genuine evidence for the null result, or whether there was insufficient evidence to distinguish the null hypothesis from the experimental hypothesis (that an automatic PIT effect would be observed). It was assumed that the maximum plausible effect size would be of an opposite but comparable size to the effect observed in Experiment 10. The mean difference in R1 responses between the two compound stimuli (S2+S3 – S1+S3) was 57.29 in Experiment 10. The mean difference between the stimulus compounds in the No Load group was 0.63 (*SEM* = 2.48). A uniform distribution produced a Bayes Factor of 0.07, which provides strong evidence for the null hypothesis. Thus, the data suggest that when multiple responses are primed on every transfer test trial, an automatic PIT effect is not seen under concurrent load.

4.6.3 Discussion

Experiment 11 first replicated the result observed in Experiment 10 in the No Load group. When stimulus compounds signalled a rewarding and a devalued outcome, participants demonstrated a bias away from the cued, devalued outcome. Together, these results confirm that PIT effects are sensitive to post-training outcome devaluation procedures, at least when multiple outcomes and responses are cued on each trial during the transfer test.

The unique contribution of Experiment 11 was in the use of a concurrent load procedure during the transfer test. The sensitivity to outcome devaluation seen in the No Load condition was not observed in participants who completed a concurrent load task during the transfer test. This suggests that the manipulation was effective in reducing participants' ability to employ the controlled processes necessary to produce the behavioural pattern observed in the No Load group. Crucially, there was no evidence of an automatic PIT effect in the Load group, and this was supported by the Bayes Factor analysis. These data therefore support the propositional EU account of PIT, but provide less compelling evidence for the automatic S-O-R model.

4.7 General Discussion

The current experiments tested the propositional EU account of PIT against the S-O-R account. Each experiment examined the effect of outcome devaluation when stimulus compounds signalled outcomes that were associated with both instrumental responses during the PIT transfer test. Experiment 7 first established two instrumental responses to predict different two rewarding outcomes each, before one outcome associated with each response was devalued. When response choice was tested in the presence of stimulus compounds that signalled one valued and one devalued outcome, participants demonstrated a clear bias towards the instrumental response that was associated with the cued and valued outcome. Experiment 8 extended this approach but also sought positive evidence of an automatic S-O-R mechanism. Participants were trained to perform two instrumental responses to earn different aversive outcomes, as well as a common rewarding outcome. Response choice was then assessed in the presence of pictorial stimuli signalling the valued outcome in compound with one of the aversive outcomes. Participants who reported explicit knowledge of the instrumental contingencies demonstrated a strong bias *away* from the cued, aversive outcome. Experiment 9 suggested that this effect depends on the presentation of a stimulus signalling the common rewarding outcome. The effect was then replicated in Experiment 10 using a post-devaluation procedure. Finally, Experiment 11 demonstrated that sensitivity to devaluation observed in Experiment 10 depends on controlled reasoning processes. Participants responded at chance when they completed a demanding concurrent load task during the transfer test. Crucially, no evidence of an automatic S-O-R process (where the S1+ S3 and S2+S3 compounds increased R1 and R2, respectively) was observed under concurrent load. Collectively, these results suggest that PIT is goal-directed when the stimuli presented during the transfer test signal that multiple outcomes are available on each trial.

The current results are most consistent with the propositional EU account of PIT, where participants make a *controlled choice* to pursue the cued outcome because it is seen as much more available than the alternative outcome. There are some discrepancies in the results, however, particularly with respect to Experiment 3 (where *insensitivity* to devaluation was observed in Chapter 3). These issues will be elaborated on further in Chapter 5 (General Discussion).

Chapter 5: General Discussion

5.1 Introduction

The experiments in this thesis aimed to further current knowledge of the psychological mechanisms that underlie human PIT effects. Chapter 1 predominately focused on two psychological theories of PIT: S-O-R theory and a recently developed propositional EU theory (which is an extension of hierarchical S: R-O theory). The dominant model of PIT, S-O-R theory, advocates an automatic associative link mechanism. It specifically suggests that Pavlovian stimuli presented during the transfer test activate the sensory properties of the associated outcomes, which then trigger associated instrumental responses (thereby circumventing the current incentive value of the outcome). The finding that PIT is often insensitive to devaluation clearly accords with this account very well (Hogarth, 2012; Hogarth & Chase, 2011; Holland, 2004; Rescorla, 1994b; Watson et al., 2014).

Chapter 1 contrasted S-O-R theory to the propositional EU account, which proposes that PIT effects are mediated by explicit contingency knowledge that gives rise to effortful inferential reasoning processes. It also makes the key prediction that PIT effects are mediated by judgements about the probability (Op) and value (Ov) of each outcome. The primary empirical support in the existing literature for the propositional EU account came from two findings. Firstly, PIT effects are usually only observed in participants who can report explicit contingency knowledge (Bezzina et al., 2016; Hogarth et al., 2007; Lovibond et al., 2015; Talmi et al., 2008). Secondly, PIT effects are sensitive to post-training verbal instructions (Hogarth et al., 2014). These findings are both regarded as evidence for the propositional EU account, because they suggest that PIT effects reflect controlled, cognitive processes. However, it was clear that there were several key aspects of the propositional EU account that were untested in the existing literature. A core aim of the current research, therefore, was to test the propositional EU account of PIT more thoroughly. Another important aim was to further test the instructional and devaluation effects, with the aim of providing a cohesive account of PIT that reconciled these seemingly paradoxical results.

5.2 Summary of results

Chapter 2 extended the work of Hogarth et al. (2014) and Seabrooke et al. (2016), by testing whether PIT would be sensitive to a post-training reversal instruction. The experiments also tested whether the PIT effects would be influenced by time pressure (Experiment 1) or concurrent load (Experiment 2). Standard PIT effects were observed in the Non-Reversal groups; the reward cues selectively increased choice of the instrumental response that was paired with the common outcome. Interestingly, these non-reversal PIT effects were observed even when participants responded very quickly, or when they completed the concurrent load task during the transfer test. These data therefore provide unique evidence of an ‘automatic’ PIT effect, at least in the sense that the PIT effects demonstrated in these experiments do not appear to require a great deal of time or controlled processing.

Experiment 2 also demonstrated a reversed PIT effect by instruction; the stimuli biased response choice not towards the response that was paired with the cued outcome, but towards the response that was more likely to be reinforced according to the instruction. A reversed PIT effect was not observed in the Reversal Load group. Crucially, in this reversal instruction group, the concurrent load procedure did not reveal any evidence of an underlying automatic S-O-R mechanism; response choice was at chance regardless of the stimulus present. This result seems most naturally interpreted within the propositional model of PIT, because automaticity was not revealed when the conditions favoured the discovery of an underlying S-O-R mechanism.

Chapter 3 used a different approach, in that it tested the effect of a novel, very strong devaluation procedure on PIT. In Experiment 3, a typical PIT procedure produced a PIT effect that was insensitive to this devaluation procedure. That is, the Pavlovian stimuli selectively increased choice of the response that was paired with the cued outcome, and the size of the PIT effects for the valued and devalued outcomes did not significantly differ. This insensitivity to devaluation is usually regarded as evidence of an automatic S-O-R mechanism. However, Experiments 4-6 provided examples of insensitivity to devaluation even when an S-O-R mechanism should not have been able to readily operate. Together, the data were therefore interpreted as evidence to suggest that insensitivity to devaluation may, at least sometimes, reflect a controlled decision-making process.

The experiments in Chapter 4 developed an alternative method to examine the effect of outcome devaluation on PIT. In each experiment, every transfer test trial signalled two outcomes[8], so that both instrumental responses were cued on every trial. Crucially, only one of the signalled outcomes was valued, because the other had been devalued. Cue-elicited response choice was highly sensitive to devaluation under these circumstances; participants selectively chose the response that was associated with the cued, high-value outcome. Interestingly, this *sensitivity* to devaluation was not observed when participants received a concurrent load throughout the transfer test (Experiment 11). Under these circumstances, participants responded at chance. Crucially, they did not show any evidence of an automatic PIT effect that was insensitive to devaluation. Taken together, the results reported in Chapter 4 support the suggestion that PIT is

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[8] With the exception of the Single-Cue group in Experiment 9.

highly sensitive to outcome devaluation when multiple outcomes and responses are cued on every transfer test trial.

5.3 Theoretical implications

The current research provides strong evidence to suggest that PIT effects are goal-directed, at least when multiple outcomes and responses are cued. In the past, an automatic S-O-R link mechanism has been favoured, in which the stimulus activates only the sensory properties of the outcome (Alarcón & Bonardi, 2016; Balleine & Ostlund, 2007; de Wit & Dickinson, 2015; Hogarth, 2012; Hogarth & Chase, 2011; Holland, 2004; Watson et al., 2014, 2016). This explanation has been favoured largely because PIT procedures usually produce insensitivity to devaluation. Experiment 3 confirmed this result, by demonstrating a PIT effect that was insensitive to a very strong devaluation manipulation. This insensitivity to devaluation is seemingly inconsistent with the *sensitivity* to devaluation that was observed in Chapter 4; a stimulus that signalled a devalued outcome increased responding for that outcome in Experiment 3, but *decreased* responding for that same devalued outcome in Experiment 10 (and related experiments). The experiments used the same devaluation technique, which suggests that it is not simply the method of devaluation that is crucial (Eder & Dignath, 2016). Clearly, there is a crucial difference between PIT effects in which multiple outcomes/responses are primed (Experiment 10), and the standard value-insensitive case in which only one outcome/response is primed (Experiment 3). In light of these discrepant results, much of the discussion below concerns the extent to which the dominant psychological theories of PIT are able to account for both of these results. Of course, the effects demonstrated in Experiments 3 and 10 were not observed within a single experiment, so cross-experimental comparisons must be made with caution. However, the effects were both significant in opposite directions, and they were also both demonstrated in multiple experiments. It seems clear, therefore, that both effects are robust and replicable. For the sake of simplicity, the discussion in this section focuses predominately on Experiment 3 versus Experiment 10. It is also worth noting, however, that the other experiments reported in Chapter 4 are also relevant to the discussion because they complement the conclusions of Experiment 10 very well.

The insensitivity to devaluation observed in Experiment 3 is clearly most consistent with S-O-R theory, where the stimulus activates the identity but not the value of the associated outcome, which then triggers the associated instrumental response (Hogarth & Chase, 2011; Martinovic et al., 2014; Rescorla, 1994b). The value-based response choice pattern in Experiment 10, by contrast, is less readily reconciled with current accounts of S-O-R theory. The results of Experiment 10 provide clear evidence to suggest that the stimuli retrieve both the sensory properties and the current incentive value of the outcomes. Current versions of S-O-R theory are therefore able to explain the insensitivity to devaluation observed in Experiment 3, at the expense of not being able to explain the sensitivity to devaluation that was observed in Experiment 10. There are, however, other ways in which an amended S-O-R model could potentially account for the goal-directed PIT effects that were reported in Chapter 4. These possibilities will be discussed after the section immediately below, which considers whether the propositional EU theory can reconcile the results of Experiment 3 with the results of Experiment 10.

Propositional EU theory proposes that PIT effects are mediated by cue-elicited increases in perceived outcome probability (Op). That is, participants infer that the stimulus presented during the transfer test signals that the associated response is more likely to be reinforced. They then deliberately choose that response to obtain the cued outcome. From the propositional EU perspective, it is clear that typical PIT designs, in which only one high- or low-value outcome is cued per trial, confound cue-elicited outcome probability with outcome devaluation. When a single stimulus that predicts a devalued outcome is presented, it signals a high probability outcome that is of low value (high Op, low Ov). The other outcome retains a high Ov but has a low Op (because it is not cued). Under these circumstances, participants demonstrate a bias towards the cued, devalued outcome (compared to a neutral stimulus), perhaps because it is considered to be much more available than the non-cued outcome. The cue-elicited increase in perceived outcome probability (Op) for the devalued outcome may lend itself to a decision-making process that is akin to a *Hobson's choice*; although participants are free to choose either response, they believe that *only the cued outcome is available* and so they respond for it, even if that outcome has been devalued. When multiple outcomes and responses are cued on every transfer test trial, however, PIT effects are highly sensitive to outcome devaluation. Participants may infer that both responses are likely to be reinforced during the transfer test, and so they can choose between the cued outcomes on the basis of their value. This account may reconcile the sensitivity to devaluation observed in Experiment 10 with the insensitivity observed in Experiment 3; in both cases, response choice is a reflection of a 'high-level', decision-making process.

This analysis does, however, highlight something of a paradox. On the one hand, participants seem to prefer devalued outcomes to no outcome in the standard PIT procedure (as seen in Experiment 3), but seek to *avoid* the same devalued outcomes in Experiment 10. The latter result suggests that the devaluation procedure rendered the value of the devalued outcomes negative. The EU of these outcomes should also, therefore, be negative in the standard PIT design (Experiment 3). The EU for the non-cued but valued outcome should be zero, because the outcome is considered to be unavailable. Hence, when the devalued outcome is cued, the EU of the valued outcome should be zero, and the EU of the devalued outcome should be negative. The instrumental response that predicts the valued outcome should therefore be preferentially performed, even when the devalued outcome is cued. Of course, this result is not usually observed - participants often show a bias towards the devalued outcome when it cued (relative to a neutral stimulus).

Why then, according to propositional EU theory, did participants often choose a cued aversive outcome in Experiment 3? One possibility is that the insensitivity to devaluation observed in Experiment 3 is entirely an artefact of the measurement technique. As noted in Chapter 3, measuring PIT effects relative to baseline response choice in typical PIT procedures is a flawed approach, because baseline response choice is biased towards the still-valued outcome after devaluation. This issue is discussed in more detail below.

Another way to reconcile the results of Experiment 3 with propositional EU theory would be to suggest that there is value in simply earning some kind of outcome on each trial. This leads participants to respond for the devalued but available outcome over the valued but (perceived to be) unavailable outcome. A limitation of typical PIT designs is that they often confound the perceived availability of the cued outcome with the absence of the other (non-cued) outcome. As a consequence, participants may infer that the signalled outcome is available on a given trial, and that all non-cued outcomes are not. This makes it difficult to determine whether PIT effects are driven by an expectancy of the cued outcome, an expected omission of the non-cued outcome, or both. The experiments reported in Chapter 4 offer a unique advantage in that they help to isolate the role of outcome value by signalling multiple outcomes that differ in their value.

5.3.1. Other theories

Of the theories of PIT discussed so far, the experiments reported in this thesis appear to be most consistent with the propositional EU model of PIT. The section below now discusses the results with reference to three other theories of PIT, namely an amended S-O-R model, hierarchical S: R-O theory and a mediated S-R theory that was recently proposed by Cohen-Hatton et al. (2013).

5.3.1.1. An amended S-O-R model

One way in which an amended S-O-R model could account for the sensitivity to devaluation observed in Chapter 4 would be to propose that PIT effects are still mediated by an S-O-R associative chain, but that chain encodes both the sensory properties and the current incentive value of the outcome. There is no inherent reason for S-O-R theory to assume that the stimulus activates only the identity of the associated outcome rather than its value. Indeed, this was merely an auxiliary assumption that enabled S-O-R theory to explain the insensitivity to devaluation that is usually observed in PIT experiments. It is also not necessary to assume that goal-directed PIT effects are propositional in nature; automatic associative link processes have, in the past, been purported to explain other goal-directed learning phenomena (de Wit & Dickinson, 2009, 2015). S-O-R theorists could therefore reconcile the results reported in Chapter 4 with S-O-R theory very simply, by proposing that the stimulus activates both the sensory properties and the value of the outcomes. The model could, for instance, take a stance similar to that of the associative-cybernetic (AC) model of instrumental action (Balleine & Ostlund, 2007; de Wit & Dickinson, 2009, 2015, Dickinson, 1994, 2012, 2016). The core idea here is that, once an outcome representation has been activated, its value is assessed in an *incentive system*. Information is then fed back to the motor program. This feedback loop allows modulation (i.e. activation or inhibition) of the instrumental response depending on the current incentive value of the outcome.

To make this analysis more concrete, consider the example of Experiment 10. Here, two instrumental responses (R1 and R2) were trained to predict different food outcomes (O1 and O2) as well as a common outcome O3 (R1-O1, O3; R2-O2, O3). The unique outcomes O1 and O2 were then devalued. Instrumental response choice was then tested in the presence of stimulus compounds that signalled the common, valued O3 and either the devalued O1 or O2 (S1+S3, S2+S3). Response choice was strongly biased *away* from the response that predicted the cued, devalued outcome. The AC model (or something similar) could explain this result by perhaps suggesting that the stimulus compounds activated the mental representations of the associated outcomes. The S1+S3 compound, for example, would have excited the mental representations of outcomes O1 and O3. Then, the value of the outcomes would have been assessed in the incentive system, where O1 would have been recognised as devalued (and O3 as valued). This information would then be fed back to the motor programs through the feedback loop. This feedback loop would result in O3 priming R1 and R2 indiscriminately (because both responses were equally associated with O3). O1 would also inhibit the performance of R1, because O1 was devalued. Hence, the priming effect of O3 on R1 would be offset by the inhibition of R1 by O1. R2 would therefore be activated more strongly than R1, and so should be preferentially performed. Clearly, this is one way in which an amended S-O-R model could account for the goal-directed PIT effects observed in Chapter 4.

It is not clear, however, that the model described above can account for the *insensitivity* to devaluation that is usually seen in typical PIT experiments (such as Experiment 3). If outcome value is assessed in the incentive system, then the response that predicts the cued, devalued outcome should not be performed (because the incentive system should inhibit the response just as in Experiment10). Thus, the adapted S-O-R model can account for the goal-directed effect observed in Experiment 10 (as well as other related effects in Chapter 4), at the expense of not being able to explain the insensitivity to devaluation observed in Experiment 3 (as well as other previous demonstrations of insensitivity to devaluation observed in the PIT literature).

By contrast, the propositional EU theory of PIT can account for both results. From this perspective, participants show a small bias towards the devalued outcome when it is the only outcome cued, because it is perceived to be much more available than the alternative, non-cued outcome. There may also be value in earning *something* rather than nothing at all. When multiple responses are cued, however, then participants selectively choose the response that produces a high-probability (i.e. cued), high value outcome.

5.3.1.2. Hierarchical S: R-O theory

The results reported in this thesis also have interesting implications for the hierarchical S: R-O theory that was first introduced in Chapter 1. Hierarchical S: R-O theory suggests that PIT effects arise because the Pavlovian stimulus signals an increase in the strength of the associated instrumental relationship (Balleine & Ostlund, 2007; Cartoni et al., 2015; Colwill & Rescorla, 1990b; de Wit & Dickinson, 2009; Hogarth et al., 2014; Rescorla, 1991). The key point to note about hierarchical S: R-O theory is that it proposes that instrumental responses (including PIT effects) are mediated by *forward R-O* associations (in contrast to the backwards O-R association that is advocated by S-O-R theorists). It therefore assumes that instrumental responses are evaluated and performed on the basis of their consequences (de Wit & Dickinson, 2009). Intuitively, this account appears to be a goal-directed model of PIT, and is therefore consistent with the goal-directed effects that were observed in Chapter 4.

There are at least two possible ways to interpret the psychological nature of the hierarchical S: R-O mechanism. In the animal literature, hierarchical S: R-O theory is often interpreted within an associative link framework, where activation of the response representation transmits excitation to the associated outcome representation (e.g., Colwill & Rescorla, 1990b; Rescorla, 1991). From this perspective, the mechanism that allows hierarchical theory to generate goal-directed behaviour is unclear. The forward R-O link allows the response to activate the associated outcome representation, but it is not clear how the appraisal of the outcome’s value then modulates the instrumental response (Dickinson, 1994; Mackintosh & Dickinson, 1979). For this reason, the hierarchical S: R-O mechanism has been suggested to be integrated with the AC model (de Wit & Dickinson, 2009; Dickinson, 1994). In this way, the evaluation of the outcome representation can be fed back to the motor system through the feedback loop, thereby modulating the extent to which the instrumental response is performed based on the current incentive value of the associated outcome. This approach has the advantage of providing an associative mechanism of goal-directed behaviour (Dickinson, 1994). However, it is unclear whether this mechanism can account for the finding that PIT effects are profoundly influenced by verbal instructions (Hogarth et al., 2014). Hence, the link-based view of hierarchical S: R-O theory can (when integrated with the AC model) provide a mechanism for goal-directed behaviour, but not necessarily provide a full account of the complexities of human behaviour.

An alternative view is that the hierarchical mechanism is propositional in nature. Per this account, participants infer that the stimulus signals which response will be reinforced during the transfer test, and they then *deliberately choose* that response. From this perspective, the propositional EU and hierarchical accounts of PIT are essentially the same theories that use different terminologies. This interpretation has the advantage of allowing hierarchical theory to explain the instructional sensitivity reported in the current thesis and by Hogarth et al. (2014), at the expense of not providing a mechanistic view of human goal-directed behaviour.

5.3.1.3. Mediated S-R theory

The results reported in Chapter 4 also have interesting implications for another link-based theory of PIT, called *mediated S-R theory*, that was recently put forward by Cohen-Hatton et al. (2013). Mediated S-R theory has not been discussed so far because the experiments in this thesis were designed to test the dominant S-O-R account of PIT against the propositional EU model. However, mediated S-R theory was proposed largely to explain the counterintuitive finding that PIT can be both outcome-selective and insensitive to devaluation. The sensitivity to devaluation observed in Chapter 4 is therefore clearly relevant for the mediated S-R theory of PIT.

Similar to S-O-R theory, mediated S-R theory assumes that instrumental training produces bidirectional instrumental R-O/O-R links. These links allow thoughts about the outcome O (i.e. activation of the outcome representation) to automatically activate the associated instrumental response R representation. Crucially, when the stimulus S is subsequently paired with the outcome O during Pavlovian conditioning, the outcome representation will also activate the representation of the associated instrumental response R. This means that the representations for both the Pavlovian stimulus S and the instrumental response R will be concurrently activated, which is suggested to produce a direct stimulus-response (S-R) associative link. A similar argument can be made when Pavlovian conditioning precedes instrumental training. Here, Pavlovian conditioning fosters a link between the mental representations of the stimulus S and the outcome O. When the instrumental response R is subsequently paired with the outcome O, the outcome will activate the associated stimulus S. It is worth mentioning here that activation of the response R would precede activation of the stimulus representation S under these circumstances (Alarcón & Bonardi, 2016). Nevertheless, the authors suggest that the concurrent activation of the stimulus and response would allow a direct S-R link to form (Cohen-Hatton et al., 2013).

S-R associative links do not incorporate a representation of the outcome, and so PIT would not be expected to be sensitive to devaluation under these circumstances. Cohen-Hatton et al.'s (2013) mediated S-R account therefore successfully predicts the insensitivity to devaluation that is usually reported in PIT experiments. However, the goal-directed PIT effects reported in Chapter 4 seem to lie beyond the scope of mediated S-R theory. Consider the example of Experiment 10 again. Here, Pavlovian training (presumably[9]) precedes instrumental training, because life experience prior to the experiment would have allowed the food pictures to become associated with the outcomes (de Wit & Dickinson, 2015). In the transfer test, the stimulus compounds (e.g. S1+S3) should have therefore automatically triggered the response that was most strongly associated with the stimuli (R1), irrespective of the value of the mediating outcomes. The fact that the *opposite* result was observed speaks against the mediated S-R account of PIT.

To conclude this section, the results reported in this thesis appear to be most in line with the hierarchical and propositional EU theories of PIT. This is because, when only one low-value outcome is cued, participants show a bias towards that outcome relative to a neutral stimulus. This suggests that the stimulus signals which response is

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[9] To test mediated S-R theory more thoroughly, future research should use the experimental design of Experiment 10 but incorporate a formal Pavlovian conditioning procedure.

more likely to be rewarded, and is therefore akin to the hierarchical mechanism that has been advocated previously (Balleine & Ostlund, 2007; Cartoni et al., 2015; Colwill & Rescorla, 1990b; de Wit & Dickinson, 2009; Hogarth et al., 2014; Rescorla, 1991). In light of the instructional effects reported in this thesis and by Hogarth et al. (2014), it seems possible that this hierarchical mechanism is encoded propositionally. Furthermore, when stimuli signal multiple responses and outcomes that differ in their values, participants select the response that is associated with the cued outcome that is of the highest value. This latter result suggests that outcome value also plays an important role in human PIT effects. Together, these results therefore seem to be best accounted for by the propositional EU theory of PIT. The section below now details a broader implication for the interpretation of outcome devaluation effects.

5.3.2. Other theoretical implications

Another important theoretical implication of the current results concerns the interpretation of demonstrations of insensitivity to devaluation more generally. Insensitivity to devaluation is usually regarded as a key criterion for diagnosing habitual or automatic control (de Wit & Dickinson, 2009; Dickinson, 1985). Experiments 4 and 5, however, demonstrated insensitivity to devaluation even when the instrumental responses were merely instructed rather than established through trial-by-trial conditioning. These experiments suggest that insensitivity to devaluation may, at least sometimes, reflect the operation of a controlled reasoning process rather than an automatic associative link mechanism. It might be argued that the instructions alone could have fostered an instrumental link, which would then explain why the instructed PIT effect was insensitive to devaluation. As noted in Chapter 3, the mechanisms that would allow an instruction to produce an instrumental link is far from clear (Mitchell et al., 2009). At the very least, formal instrumental training would be expected to produce a *stronger* link than an instructed contingency. This prediction was not supported by the results of Experiment 5; if anything, the PIT effect for the devalued outcome was numerically larger for the instructed responses than the trained responses in Experiment 5. Thus, the data were consistent with the suggestion that instrumental responses can be insensitive to devaluation, even when an inflexible link mechanism should not readily operate.

The instructed insensitivity discussed above is consistent with the suggestion that insensitivity to devaluation may sometimes reflect a controlled decision-making process. As noted above, typical PIT procedures (e.g. Experiment 3) might produce an apparent insensitivity to devaluation because they confound cue-elicited outcome probability with outcome devaluation. When only one outcome is cued per trial during the PIT transfer test, participants may choose that outcome not because it is an involuntary response, but because it is perceived to be the only available outcome. Thus, the apparent insensitivity to devaluation may in fact reflect a *response strategy* rather than an inflexible stimulus-elicited response (Robinson & Berridge, 2008). Future research will determine whether this account also stands up to scrutiny with respect to other instrumental learning phenomena.

Another interesting finding comes from the concurrent load tasks used in Experiments 2 and 11. In Experiment 2, the standard (non-reversal) PIT effect was insensitive to the load manipulation. The sensitivity to devaluation observed in the No Load group of Experiment 11, by contrast, was completely eliminated in the Load group. These results suggest that the standard PIT effect observed in Experiment 2 might be more automatic than the sensitivity to devaluation observed in Experiment 11, because it was more resistant to manipulations that aimed to reduce participants' ability to use controlled reasoning processes. From the propositional EU theory perspective, the standard PIT observed in Experiment 2 relies largely on judgements of perceived outcome probability. The sensitivity to devaluation observed in Experiment 11, by contrast, is more complex because it depends on a recollection of the relevant contingencies, judgements of perceived probability, *and* the current incentive value of the outcomes. Hence, from the propositional EU theory perspective, it makes sense that the sensitivity to devaluation observed in Experiment 11 would be less automatic (i.e. more influenced by the load manipulation) than the standard PIT effect observed in Experiment 2. Of course, it is possible that different processes underlie the two effects. The standard PIT effect in Experiment 2 may be relatively automatic (and hence immune to the load manipulation), while the sensitivity to devaluation observed in Experiment 11 may be more controlled (which would explain why it was eliminated through concurrent load).

5.4 Methodological implications

The primary methodological implication of the current results comes from the argument made in Chapter 3 regarding the use of a moving baseline to measure devaluation effects in PIT experiments. It was suggested that typical PIT devaluation procedures, in which PIT effects are measured relative to a neutral stimulus, may underestimate the size of the PIT effect for the valued outcome, and overestimate the size of the PIT effect for the devalued outcome. This is because, after outcome devaluation, response choice in the neutral stimulus condition is usually biased towards the still-valued outcome. As a consequence, there is less room to observe a PIT effect for the valued outcome (due to the ceiling effect on response choice), and there is relatively greater opportunity to detect a PIT effect for the devalued outcome. It is possible (although not currently confirmed) that the insensitivity to devaluation observed in Chapter 3 is *entirely* due to this artefact. If this is the case, it would have profound implications for the theories of PIT, because it would allow researchers to say with more certainty that PIT effects are goal-directed. Notably, the amended S-O-R model would no longer have a problem explaining the insensitivity to devaluation observed in Experiment 3, because the effect could simply be attributed to a flaw in the experimental design. The propositional EU model would also not need to rely on the suggestion that participants choose to respond for the cued, devalued outcome because it is better to earn *something* than nothing. Of course, this possibility remains speculative at present. It is certainly still possible that PIT can be truly insensitive to devaluation in other circumstances, even when the size of the effect is not assessed relative to baseline response choice. Some experimental designs that aim to test this idea are described below (Section 5.6, Future research).

The ceiling effect issue described above was discussed in the context of outcome devaluation, but it also has potentially important implications for experiments exploring the relationship between PIT and drug dependency. Notably, several recent experiments have reported that baseline response choice is correlated with dependence, but PIT effects are not (Hogarth & Chase, 2011, 2012; Martinovic et al., 2014). However, these experiments all used typical PIT procedures in which one response was trained to predict a drug reward (either tobacco; Hogarth & Chase, 2011, 2012, or alcohol; Martinovic et al., 2014) and another response that was trained to predict a non-drug reward (chocolate). Instrumental response choice is then tested in the presence of drug (tobacco or alcohol), chocolate and neutral stimuli. Under these circumstances, baseline (non-cued) instrumental choice typically correlates with dependence. That is, highly dependent drug users show a preference for the drug response in the absence of any Pavlovian stimuli. This means that, as dependency increases, there is less room to observe a PIT effect for the drug reward (in much the same way as there is less opportunity to observe a PIT effect for the still-valued outcome in typical PIT devaluation experiments). It is therefore not surprising that these experiments failed to find a relationship between PIT effects and dependency. Indeed, one might even expect a *negative* correlation under these circumstances, because the size of the observable PIT effect for the drug reward should be inversely related to dependency. In sum, to test whether PIT effects are truly correlated with dependence, future research needs to measure the PIT effect in a way that is not contingent on baseline response choice in the presence of a neutral stimulus. The design used in Experiments 8-11, for example, could be adapted to measure the relationship between tobacco dependency and PIT. In this design, tobacco points would serve as the common outcome (O3) that both responses produce. The responses would also produce two other outcomes O1 and O2 (e.g. crisps and popcorn). As in Experiments 8-11, instrumental response choice would then be assessed in the presence of stimulus compounds that signal tobacco and one of the other outcomes (S1+S3, S2+S3). If PIT is related to tobacco dependence, then a positive correlation would be expected between the size of the reversed PIT effect (such as that seen in Experiments 8-11) and tobacco dependency. This is because highly dependent tobacco smokers might attempt to avoid the cued food outcome in order to obtain the cued tobacco outcome.

5.5 Applications

The current results suggest that controlled, propositional processes can play an important role in generating PIT effects. This is in contrast to the usual interpretation of PIT, and cue reactivity more generally. Cue-elicited reward seeking may have evolved, at least partially, as a decision-making heuristic to maximise success when searching for natural rewards such as food and water. Cues may signal the availability of specific rewards, and therefore the viability of the responses that earn those rewards. When only one reward is perceived to be available because it is the only cued outcome, resources may be channelled into obtaining that reward, even if it is of low value (as seen in the standard PIT effect). More research is needed to determine whether this is also true of cue-elicited reward-seeking outside of the laboratory, or whether it is simply an artefact of procedures that measure PIT devaluation effects against a biased baseline. If it is also applicable in real-world contexts, then the insensitivity to devaluation seen in Experiment 3 (as well as other previous demonstrations) provides a clear way in which reward cues may facilitate dysfunctional behaviour. The results may, therefore, have important implications for clinical treatments. PIT processes are thought to be involved in a range of problematic behaviours, including drug-seeking (Hogarth et al., 2010) and overeating (Colagiuri & Lovibond, 2015; Watson et al., 2014). These behaviours are often not moderated by outcome devaluation – for example satiety, health warnings or taste aversion (Hogarth & Chase, 2011; Hogarth, 2012; Watson et al., 2014). That is, people seek rewards even when those rewards lead to unpleasant, unwanted and potentially damaging consequences. The current research suggests that these behaviours can sometimes reflect strategic processes.

The current results suggest that interventions for overeating and drug abuse should focus especially on controlled decision-making biases. This conclusion is consistent with the finding that extinction of cues through exposure therapy does little to reduce cue reactivity (e.g., Conklin & Tiffany, 2002). This failure is also mirrored in laboratory PIT experiments, which have demonstrated that Pavlovian extinction procedures do not eliminate PIT in either rodents (Delamater, 1996; Rescorla, 1992a) or human participants (Hogarth et al., 2014; Lovibond et al., 2015; Rosas et al., 2010). The aim of these procedures is to degrade Pavlovian S-O associations by repeatedly pairing a reward-predictive stimulus with non-reinforcement. Through the lens of associative theory, exposure therapy (extinction) should reduce cue reactivity by weakening the link between the stimulus S and the outcome O. The failure of these manipulations to influence cue reactivity is therefore troubling from an S-O-R link perspective (Cohen-Hatton et al., 2013). From a propositional perspective, however, it makes sense that extinction treatments would not dampen cue reactivity in the real world. This is because cue exposure therapies are often incongruent with knowledge about the real world. For example, individuals will continue to believe that a chocolate bar wrapper signals chocolate, regardless of whether that wrapper has been repeatedly presented in the absence of a chocolate reward in the clinic.

Note that this analysis of appetitive extinction contrasts with phobia exposure treatments, where beliefs are not (typically) congruent with reality. A patient may have a phobia of spiders, for example, even though spiders are not (usually) harmful. Repeatedly presenting spider stimuli in the absence of an aversive outcome should, therefore, reinforce the (correct) belief that spiders are not generally harmful. Of course, there are also some examples of reward-predictive stimuli where extinction treatments would not undermine propositional beliefs. Consider the example of an individual who eats chocolate in front of the television every evening. Here, the television is associated with chocolate, but it does not actually *produce* chocolate. Hence, extinguishing the television-chocolate association should not undermine propositional beliefs about the "signalling" role of the television. It is possible that extinction treatments using these types of stimuli would be more responsive to cue exposure treatments, because such therapies would not undermine propositional beliefs with respect to these stimuli.

5.6 Future research

The results reported in this thesis have been interpreted as evidence for the role of controlled, propositional processes in PIT. However, the immunity of the basic PIT effect to speed and load in Chapter 2 suggests that PIT effects do have an automatic quality. These experiments also indicate potential to observe further automaticity in other PIT experiments. This would be an exciting and worthwhile line of research, because it would provide clear evidence for a dual-process account of PIT. It would therefore have profound implications for the strategies that are recommended to target problematic PIT processes outside of the laboratory. The demonstration of a standard PIT effect despite a reversal instruction would provide especially strong evidence of automaticity, because this would be completely incongruent with the instructed contingencies. Hence, it could demonstrate a behavioural dissociation between propositional beliefs (perhaps measured via expectancies) and performance.

One way to look for further automaticity would be to increase the demands of the PIT task. Increasing the task complexity should reduce participants' ability to use explicit and controlled strategies, and might consequently reveal evidence of an underlying automatic mechanism. More Pavlovian and instrumental contingencies could be trained, for example, to increase the working memory demand (e.g., Le Pelley et al., 2005). If a standard PIT effect is observed under time pressure or concurrent load, even when there are many contingencies to remember, it would lend credence to the suggestion that PIT effects can be generated automatically (and would hence support a dual-process account of PIT). The demonstration of a PIT effect in the absence of explicit contingency knowledge would provide especially compelling evidence of automaticity.

Another exciting avenue for future research applies to the experiments reported in Chapter 4. Those experiments found clear evidence of sensitivity to devaluation, which suggests that PIT effects are, at least in those designs, goal-directed. It does not follow that *all* PIT effects will necessarily be goal-directed though. It is certainly still possible that automaticity (i.e. insensitivity to devaluation) will be revealed when participants are unable to use controlled reasoning processes. Experiment 11 provided an initial test of this possibility, by testing whether an “automatic” PIT effect would be revealed when participants completed a concurrent load task during the transfer test. Recall that in that experiment, two responses were trained to predict a common outcome O3, as well as either O1 or O2 (R1 – O1, O3; R2 – O2, O3). The unique outcomes O1 and O2 were then devalued, before response choice (R1 versus R2) was tested in the presence of stimulus compounds that signalled the common, valued outcome with one of the unique, devalued outcomes (S1+S3; S2+S3). The goal-directed effect (where S2+S3 increased R1 responses more than S1+S3) that was observed in the No Load group was not observed in the Load group who were engaged in a concurrent load task during the transfer test. The Load group did not demonstrate any evidence of automaticity either though; participants responded at chance throughout the transfer test, irrespective of the stimulus compound present. It is of course possible that more demanding concurrent load tasks would be more effective in producing evidence of automaticity. It is difficult to draw firm conclusions from null results, but it is fair to say that Experiment 11 provided no clear evidence of automaticity under concurrent load. However, it is still possible that other manipulations (described below) will be more effective in producing evidence of automaticity in the procedures used in Chapter 4.

It has been suggested that instrumental learning can be mediated by two distinct controllers: a goal-directed process that is sensitive to outcome devaluation, and an S-R “habit” process that is insensitive to devaluation (Dickinson, 2016). Various manipulations have been shown to produce a shift from goal-directed instrumental responding to habitual control. These manipulations include overtraining of the instrumental response (Adams, 1982; Tricomi et al., 2009), stress induction (Schwabe & Wolf, 2009), acute alcohol administration (Hogarth, Attwood, Bate, & Munafò, 2012), and negative mood induction (Hogarth, He, et al., 2015). It would be worth testing whether these manipulations are also effective in producing evidence of automaticity (i.e. insensitivity to devaluation) in the PIT designs employed in Chapter 4. It is possible, for example, that the relatively modest instrumental training in Experiment 10 favoured a goal-directed mechanism over an automatic mechanism. A longer training period might be more successful in producing an automatic PIT effect that is insensitive to outcome devaluation.

It is also important for future research to examine the extent to which the results reported in Chapter 4 replicate in non-human subjects. Notably, Rescorla (1994b) observed *insensitivity* to devaluation in rats in a study that was conceptually very similar to Experiment 7 of the current thesis. In both experiments, rats/humans were trained to perform two instrumental responses to each earn two different outcomes. One outcome associated with each instrumental response was then devalued by either pairing the outcome with lithium-chloride to induce sickness (Rescorla, 1994b), or by coating the food with ground cloves and olive oil to make it taste unpleasant (Experiment 7 of the current work). Instrumental responding was then assessed in the presence of stimuli that signalled outcomes associated with both instrumental responses. Crucially, one of the cued outcomes was always valued, and the other was always devalued. Under these circumstances, Rescorla's (1994b) rats performed both responses indiscriminately during the transfer test; response choice was *insensitive* to devaluation. Experiment 7 of the current thesis, by contrast, produced clear evidence of sensitivity to devaluation in humans; participants showed a strong bias towards the instrumental response that predicted the cued, still-valued outcome.

One possibility is that the differential results in rats and humans arose from procedural differences between the two experiments. As Rescorla (1994b) noted, it is very difficult to ensure that outcome devaluation is complete. Although Rescorla’s rats rejected at least some of the devalued outcomes during the devaluation procedure, it is possible that the outcomes were still somewhat valued. This residual value may have produced the observed indifference between the valued and devalued outcomes; there may have been no difference in value in the memory of the two outcomes. It is also possible that PIT effects are mediated by fundamentally different processes in rats and humans; that human PIT effects are mediated by a goal-directed, propositional process, and that rodent PIT effects are mediated by an automatic S-O-R process. This analysis would have a profound influence on our interpretation of rodent PIT experiments, because it would suggest that rodent studies might translate poorly to human experiments. To progress this debate, it seems sensible to first replicate both Rescorla's (1994b) experiment in rats, and Experiment 7 of the current thesis in humans. The severity of the devaluation method could also be varied in each design. It would also be a worthwhile endeavour to translate the procedure used in Experiments 8 and 10 for use in non-human subjects. These latter experiments are particularly useful because the automatic and goal-directed accounts predict *opposite* results. The experiments therefore directly set the two theories against one another.

5.7 Conclusion

The experiments in this thesis explored the psychological mechanisms that underlie human outcome-selective PIT effects. The research confirmed that PIT effects are, at least sometimes, sensitive to verbal instructions. However, they appear to be robust against speeded reaction time tasks and concurrent load tasks (Chapter 2). The latter results provide preliminary evidence to suggest that PIT effects may have an underlying automatic quality to them. In Chapter 3, a typical PIT procedure was shown to produce insensitivity to devaluation using a very strong devaluation procedure. This insensitivity was interpreted within a propositional framework, because it was apparent even when the instrumental relationship was merely instructed, which should not encourage the formation of an instrumental associative link. Finally, Chapter 4 demonstrated that PIT is highly sensitive to devaluation when multiple outcomes and responses are cued on every transfer test trial.

Overall, the results provide support for the propositional EU account of PIT. In particular, the PIT effects reported here appear to reflect a goal-directed decision-making process that is based on perceived outcome probability and outcome value. The results consequently have profound implications for our theoretical and applied understanding of PIT, in that they suggest that controlled decision-making processes can play an important role in PIT. Future work will confirm whether automatic processes also mediate PIT effects in other circumstances.

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Appendices

Appendix 1: Food devaluation measurements

Due to the different surface area and weight of the food outcomes, different amounts of the clove paste was used to devalue the foods. For each food, 11 grams (g) of oil were used per 5g of ground cloves. The table below shows the quantities of the cloves paste required to devalue 100g of each food.

Crisps	**Nachos**	**Popcorn**	**Cashews**
100g	100g	460g	50g

Appendix 2: Exclusion data

Experiment 3:

Mean percent choice of the valued key in three stimulus conditions of the transfer test in participants who failed the contingency knowledge tests (N = 10).

Valued	**Neutral**	**Devalued**
66.25 (9.37)	51.25 (11.10)	41.25 (11.98)

Note: Numbers in parentheses denote SEMs.

Experiment 5:

Mean percent choice of the valued key during the trained and instructed transfer test in excluded participants (N = 6).

Trained transfer test			**Instructed transfer test**		
Valued	**Neutral**	**Devalued**	**Valued**	**Neutral**	**Devalued**
43.75 (9.13)	39.58 (5.02)	58.33 (8.64)	83.33 (9.36)	63.54 (6.73)	37.50 (14.16)

Note: Numbers in parentheses denote SEMs.

Experiment 6:

Mean percent choice of the valued key in the three stimulus conditions of the transfer test in participants who failed the contingency knowledge tests (N = 18).

Valued	**Neutral**	**Devalued**
62.89 (7.50)	66.67 (7.09)	72.92 (5.42)

Note: Numbers in parentheses denote SEMs.

Experiment 7:

Mean percent choice of R1 in the two stimulus conditions of the transfer test in participants who failed the contingency knowledge tests (N = 7).

S1+S4	**S2+S3**
54.46 (13.76)	45.54 (10.97)

Note: Numbers in parentheses denote SEMs.

Experiment 9:

Mean percent choice of R1 during the transfer test in participants who failed the contingency knowledge tests.

Group	**S1+S3**	**S2+S3**
Single-cue (*N* = 9)	40.28 (12.29)	50.00 (14.01)
Compound-cue (*N* = 5)	58.75 (4.24)	51.25 (7.76)

Note: Numbers in parentheses denote SEMs.

Experiment 11:

Mean percent choice of R1 during the transfer test in participants who failed the contingency knowledge tests.

Group	**S1+S3**	**S2+S3**
No Load (*N* = 10)	55.00 (6.64)	53.75 (6.86)
Load (*N* = 6)	30.21 (12.12)	39.58 (11.93)

Note: Numbers in parentheses denote SEMs.

Appendix 3: Experiment 4 descriptive data

Mean percent choice of the valued key during the trained and instructed transfer tests in each instruction group of Experiment 4.

	Trained transfer test			**Instructed transfer test**		
Instruction	**Valued**	**Neutral**	**Devalued**	**Valued**	**Neutral**	**Devalued**
O-R	95.83	75.00	27.08	100.00	93.75	75.52
	(2.22)	(5.81)	(11.68)	(0.00)	(3.17)	(11.93)
R-O	92.61	60.23	17.05	97.16	91.48	62.50
	(6.78)	(10.16)	(8.52)	(2.29)	(3.70)	(13.40)

Note: Numbers in parentheses denote SEMs.