WHY DO WE DO WHAT WE DO?

THE ATTENTION-READINESS-MOTIVATION FRAMEWORK

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ABSTRACT

According to many theories of motivation, the principal driver of human behavior is the valuation of actions. Actions are valued by computing the difference between stimulus value (the benefits and costs inherent in the stimulus outcome that is the expected result of a given action) and action costs (the effort required to perform that action). However, such accounts have difficulty explaining why individuals may act inconsistently in what appear to be comparable situations, and sometimes even act in ways that seem inconsistent with relevant action values. In this dissertation, I present the Attention-Readiness-Motivation (ARM) framework, according to which such behavioral anomalies occur because stimulus value and action costs are influenced by endogenous attention and action readiness – variables that are typically not considered as a part of the valuation calculus.

In Chapter 1, I introduce the ARM framework. In Chapter 2, I present a laboratory analogue of a common behavioral anomaly – medical non-compliance. Medical non-compliance includes behaviors in which patients fail to take simple actions (e.g. taking a pill beneficial to their health) even though the failure to take such actions could have highly adverse consequences. In a series of laboratory experiments, I simulated these adverse consequences using a personally salient and highly aversive electric shock. The laboratory equivalent of taking a pill was to press an easily accessible button that was likely to preclude shock-related adverse consequences. When doing nothing was the status quo, participants frequently did not press a button that would have, for example, enabled them to avoid experiencing the shocks. Contrastingly, when participants were required to make a choice, they nearly always chose outcomes that did

not lead to a shock. Yet, this apparent preference was not manifested in behavioral contexts in which a choice was not required.

In Chapter 3, I investigate behavioral anomalies in the context of emotion regulation. I created a laboratory decision context in which participants watched a series of negatively valenced images, and in each case had the option of electing to reappraise in order to decrease negative affect. Given the many benefits and few costs associated with reappraisal, I expected that most images would be reappraised. However, participants implemented reappraisals for a small minority of images. However when the default (of doing nothing) was removed, participants chose to reappraise in many more trials.

In Chapter 4, I sought to investigate the role of attention in explaining the types of behavioral anomalies described in Chapter 2 and Chapter 3. Specifically, I sought to test whether failures to act in valued ways are in some cases caused by insufficient levels of orienting attention. I first created a scalable laboratory analogue of a behavioral anomaly, one in which participants persisted in viewing lower-valenced images even though they could have, at no cost, viewed a higher-valenced image. When I experimentally increased their orienting attention towards a caption stating they had the option to switch, participants more frequently elected to view the higher valenced image. In real-world behavioral contexts, increasing attention, without an apparent change in valuation, also led to increased levels of approach motivation in behavioral contexts involving purchasing apples and electing to take the stairs instead of the escalator. These studies suggested that endogenous attention plays an important role in motivated behavior.

In Chapter 5, I investigated whether some behavioral anomalies may occur because action costs that objectively appear negligible may be consequential, and action costs that objectively appear identical may differently influence behavior. Such effects may occur because action costs are influenced by action readiness – the ease with which an action may be initiated given the pre-action-launch state of the individual. On our account, if action readiness levels are low, even action costs that appear to be negligible can strongly affect behavioral outcomes. Similarly, action costs that appear to be identical may affect behavior differently because their action readiness may differ. I developed this proposition using the image-viewing decision context of Chapter 4 and a computational model.

I conclude this dissertation by examining the elaborations and future directions related to the Attention-Readiness-Motivation (ARM) framework.

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CHAPTER 1: INTRODUCTION TO THE ATTENTION-READINESS-MOTIVATION (ARM) FRAMEWORK

1.0 Introduction

In this chapter, we begin with a consideration of the valuation calculus that underlies many theories of motivation and decision making. Next, we consider behavioral anomalies in which people appear to act counter to the valuation calculus. We then describe the Attention-Readiness-Motivation (ARM) framework that proposes that previously understudied variables – endogenous attention and action readiness – may impact the valuation calculus in unexpected ways, thereby producing apparent behavioral anomalies. Finally, we introduce the studies described in this dissertation.

1.1 The Valuation Calculus

According to many theories of motivation and decision making, a simple calculus governs our motivated behavior. This calculus hinges on our determining the subjective value of an action by computing the expected benefits and costs of performing that action in a particular context (Barron & Hulleman, 2015; Fehr & Rangel, 2011; Glimcher & Fehr, 2013; Rangel, Camerer & Montague, 2008). At any given moment, we are thought to initiate the action that has the highest subjective action value.

The computation of subjective action value involves an integration of *stimulus value* and *action costs*. Here, 'stimulus value' is defined as the difference between the expected benefits of the stimulus (that is the target of the action) and the inherent costs of the stimulus. For example, the stimulus value of a magazine is the difference between the anticipated benefit derived from the magazine and the price paid to obtain it. 'Action costs' are defined as the costs inherent to performing the action that is being valued. They may involve physical or mental effort (Kool, McGuire, Rosen & Botvinick, 2010). For example, the action cost of purchasing a magazine may include the effort of walking to a nearby store. The stimulus value and action costs are integrated into action values. This concept has been expressed as follows in the prior literature (Rangel & Hare, 2010):

Action Value = Stimulus Value - Action Costs (Equation 1.1),

where, *Stimulus Value = Stimulus Benefits – Stimulus Costs* (Equation 1.2).

1.2 Behavioral Anomalies

This intuitively plausible account of motivated behavior is supported by significant neural and behavioral evidence (Cisek, 2012; Daw, Niv & Dayan, 2005; Morris, Dezfouli, Griffiths & Balleine, 2014; Padoa-Schioppa, 2011). However, there is a growing list of consequential behaviors in which the action valuations appear to be positive (since the stimulus valuations are significant and the action costs appear to be small) and yet the associated actions are not initiated. For example, patients frequently do not take medications that are important to their well being (Harris et al, 2010; Morris & Schulz, 1992; Vermeire, Hearnshaw, ValRoyen, & Denekens, 2001), employees do not start beneficial retirement accounts crucial to their financial future (Beshears, Choi, Laibson, & Madrian, 2006), and individuals do not proactively act to obtain their preferred options in decision contexts involving organ donation (Johnson & Goldstein, 2015), electric utilities (Hartman, Doane & Woo, 1991) and insurance providers (Johnson, Hershey, Meszaros, & Kunreuther, 1993; Samuelson & Zeckhauser, 1988).

There is also a set of behaviors in which the action valuations appear to be zero or negative (and the action values of these actions seem to be less than the action values of other available actions) and yet these actions are nonetheless initiated. For example, people have a tendency to repeatedly sit at the same spot in a classroom (even if the seat is not differentiated from other seats) (Costa, 2012) and continue to snack well past satiation if the food-item remains within easy reach (Cohen, & Farley, 2008) as though the act of eating has "momentum" (Mehrabian & Riccioni, 1986).

Researchers have attempted to explain such puzzling behavior by analyzing their (potentially hidden) stimulus values or action costs. It has been suggested that factors such as loss aversion or implied recommendations may, often subtly, cause people to persist with apparently inferior current states (Mehrabian, A., & Riccioni, 1986). Loss aversion (Kahneman, Knetsch, & Thaler, 1991) refers to people's tendency to prefer avoiding losses to acquiring gains. It may disproportionately increase the outcome valuation associated with leaving the current state. Implied recommendation (McKenzie, Liersch, & Finkelstein, 2006) refers to implicit recommendations introduced into the decision architecture by an experimenter or policy maker. They may impact outcome valuations in the direction of the implied recommendation.

However, these factors do not seem to apply – at least in any obvious way -- to the behavioral puzzles considered above. For example consider the act of taking medicine that has been paid for, is easily accessible, and has no noticeable negative side effects. There is no loss aversion associated with taking such medicine since there are no losses to be avoided. The implied recommendation, if any, encourages patients to take the medicine and not avoid it. Similarly, loss aversion and implied recommendation do not adequately explain many of the other everyday behavioral puzzles described above.

In this dissertation, we propose that these and other behavioral anomalies occur because both Stimulus Value and Action Costs (the variables that, by Equation 1.1, determine the value of an action) are influenced by other psychological processes that are typically not considered as part of the valuation calculus.

1.3 The Attention-Readiness-Motivation (ARM) Framework

Our particular focus in what we call the Attention-Readiness-Motivation (ARM) framework is endogenous attention and action readiness.

According to the ARM framework, the calculus described in Equation 1.1, unlike the calculus pertaining to physical bodies, does not refer to a featureless space in which the same variables always result in the same outcomes. Instead, the valuation calculus occurs in the context of brain networks in which changes in internal variables (i.e. endogenous attention and action readiness) may result in different behavioral outcomes, even without apparent changes to Stimulus Values and/or Action Costs – the variables that determine the value of a given action.

The first claim made by the ARM framework is that Stimulus Value (Equation 1.1) is only relevant in the valuation calculus if *endogenous attention* (Lawrence & Klein, 2013) is directed towards that stimulus. When levels of endogenous attention towards a stimulus are high, stimulus values shape behavior; when levels of attention are low, stimulus values are less determinative of behavior. On this account, for example, patient non-compliance occurs not because there are subtle drivers of

Stimulus Value or Action Costs, but because patients do not always direct their attention towards the outcomes associated with taking the medicine (and/or the outcomes associated with not taking the medicine).

Why might people fail to direct their attention towards stimuli that are important to them? Research on mind wandering (Franklin, Mooneyham, Baird, & Schooler, 2014; Smallwood, & Schooler 2015) provides one potential explanation for this phenomenon. Stimulus independent thought (mental events that arise without external precedent) has been shown to interfere with the processing of online information – presumably including the outcome valuations of potential actions. Experience sampling studies suggest that up to 50% of waking thought is stimulus independent (McVay, J. C., Kane, M. J., & Kwapil, 2009). Thus, it is possible 'zoning out' (Smallwood, McSpadden & Schooler, 2008) prevents endogenous attention from being directed to important stimuli.

The second claim made by the ARM framework is that Action Costs are influenced by the level of *action readiness* – the ease with which an action may be initiated given the pre-action-launch state of the individual. When levels of readiness for an action are high, the Action Cost is lower than it would have been if levels of readiness for that action were low. Thus, by Equation 1.1, some actions with high readiness may be launched even though their stimulus valuations are not the highest of all potential actions (because their Action Costs are low); alternatively, some actions with low readiness may not be launched even though their stimulus valuations are the highest of all potential actions (because their Action Costs are high). On this account, for example, momentum eating occurs not because an additional bite has a greater

Stimulus Value than stopping, but because action readiness reduces the Action Costs of taking another bite (whereas the action of not taking another bite has low action readiness and relatively higher Action Costs).

Why might levels of action readiness for a particular action vary? Research on repetition priming has revealed that when an object is frequently and recently encountered, we become faster and more accurate at identifying it (Henson, Shallice & Dolan, 2000). We analogously propose that when an action is frequently and/or recently executed, its action readiness increases (and the costs of performing that action decrease). On our hypothesis, readiness for an action may also increase due to the following factors: watching someone perform that action; mentally rehearsing that action; and/or attending to affordances present in a stimulus that are related to that action (Gibson, 1977). Affordances are defined as the properties of a stimulus that suggest the possibility of an action (e.g. the handle of a suitcase may suggest lifting it).

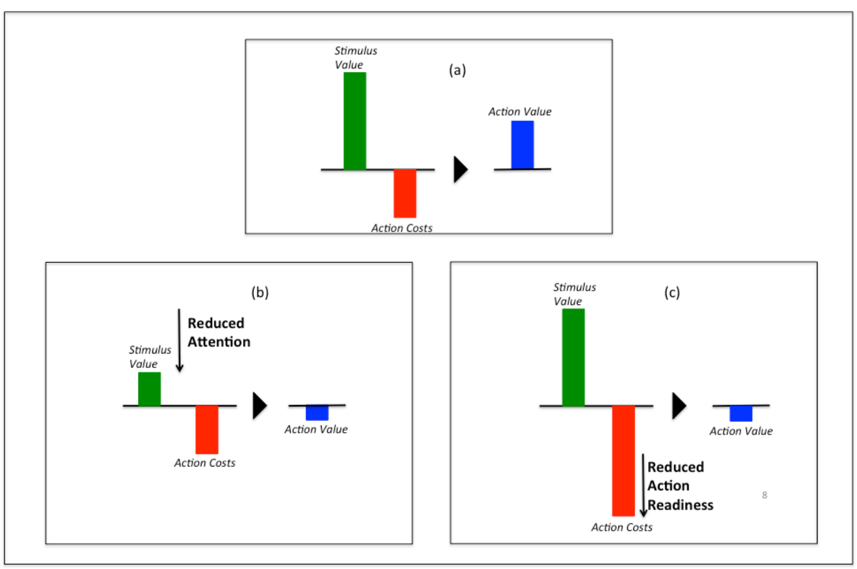

Figure 1.1, The ARM Framework: The three panels illustratively depict the valuation calculus (Equation 1.1) for the hypothetical action A. The baseline depicts zero values associated with the next best available alternative action to A. In panel (a) the Stimulus Value (depicted above the baseline) exceeds the Action Costs (below the baseline) and the resulting

Action Value is positive (above baseline) – implying that the action is likely to be occur. In panel (b) the attention directed towards the medication is reduced which in turn lowers the Stimulus Value below the Action Costs, resulting in negative Action Value – implying that the action is unlikely to occur. In panel (c), reduced action readiness increases Action Costs resulting in a negative Action Value.

The ARM framework suggests that in the context of executing an action 'A' with a positive stimulus valuation, a person may have high or low levels of endogenous attention and high or low levels of action readiness (See Figure 1.1). The action 'A' is most likely to occur when both endogenous attention and action readiness are high and least likely to occur when both variables are low. If one variable is high and the other is low, then the particular levels of each variable determine the probability of action execution. The above reasoning regarding an action with a positive stimulus value can be extended to an action with negative Stimulus Value (say B), since the act of *not* performing B has a positive stimulus value.

1.4 The Present Studies

The present studies systematically develop the ARM framework. Studies 1 and 2 are concerned with creating lab analogues of behavioral anomalies in which people do not appear to act in accordance to the operative stimulus valuation. We took particular care in both studies to avoid introducing hidden valuation drivers (e.g. loss aversion or implied recommendation). In Study 3 we demonstrated – using laboratory and field studies – that endogenous attention unlocks valuation processes and the lack of endogenous attention may produce behavioral anomalies in which people appear to diverge from executing valued actions. In Study 4, we used a computational model to demonstrate the role of action readiness in motivated behavior.

CHAPTER 2

PATIENT INERTIA AND THE STATUS QUO BIAS

Note to readers:

This chapter is adapted from a paper published in *Psychological Science*:

Suri, G., Sheppes, G., Schwartz, C., Gross, J. J. (2013) Patient inertia and the status quo bias: when an inferior option is preferred. *Psychological Science, 24*, 1763-1769.

2.0 Introduction

Many people fail to take active steps to protect their health. Diabetic patients frequently let years pass between their first diagnosis and insulin initiation (Harris et al., 2010); many high-risk heart patients resist initiating life-style changes despite physician recommendations (Van Steenkiste et al., 2004); and year after year, at-risk individuals fail to follow their doctors' recommendations to get a flu-shot (John & Cheney, 2008).

Medical non-compliance rates in developed countries are as high as 50% (Morris & Schulz, 1992). This results in a great deal of preventable human suffering and premature mortality. Medical non-compliance is estimated to increase health care costs in the US alone by $100 billion per year and is responsible for 10% of hospital admissions and 23% of nursing home admissions (Vermeire et al., 2001). Thus, patient non-compliance to prescribed medical interventions is a major public health problem.

Over four decades of research has shown that the causes of medical non-compliance are many. They include: quality of the doctor-patient relationship, number of medications prescribed, complexity of regimens, side-effects, social norms regarding compliance, a lack of medication/physician access, and unaffordable medical costs (Vermeire et al., 2001). One additional cause of medical non-compliance is *patient inertia* – which prevents patients from initiating and sustaining physician contact and/or adhering to recommended drug regimens (Joyner-Grantham et al., 2009).

One potential cause of patient inertia is hypothesized to be the Status Quo Bias (SQB) (Panidi, 2008), defined as the tendency to maintain a previous decision, either by actively choosing the default, or by doing nothing (Samuelson & Zeckhauser, 1988).

The SQB has often been said to underlie real-world decisions. One example concerns a choice between an expensive car insurance plan that protected a subscriber's rights to sue vs. a cheaper plan that restricted rights to sue. The expensive plan was offered as the default in Pennsylvania and the cheaper plan was offered as the default option in New Jersey. It was found that users in each case stuck with the default option (Johnson, Hershey, Meszaros & Kunreuther, 1993) – which, barring mysterious state-based preferences in suing others, suggests a role for the SQB. The SQB has also been cited as mechanism underlying other decision contexts, including choices involving electrical service providers (Hartman, Doane & Woo 1991), organ donation (Johnson & Goldstein, 2003), 401(K) plans (Beshears et al., 2006), investment portfolios (Ameriks & Zeldes 2001), and choices in health plans (Samuelson & Zeckhauser, 1988).

One difficulty in establishing the SQB as a potential cause of patient inertia is that prior demonstrations of the SQB have relied on decision contexts in which the outcomes are largely indistinguishable in value (e.g. Samuelson & Zeckhauser, 1988). This is not the case in patient inertia, where choice outcomes have significantly different values. For example, maintaining the default state of not commencing one's heart medication is much worse than the alternative (taking one's medication and lessening the risk of a heart attack). Thus most prior demonstrations of SQB do not readily apply to patient inertia.

To demonstrate the potential relevance of the SQB to patient inertia, one must first demonstrate decision contexts in which participants stay with the status quo even though it is unambiguously worse than the available alternatives. Such decision contexts go well beyond known instances of the SQB. We thus decided to attempt to create a laboratory context in which participants stuck with the status quo even though it was clearly not in their self-interest to do so. We reasoned that such a setting would serve as an analogue to decision contexts relevant to patient inertia.

When we embarked upon these studies, we believed that we were unlikely to find a decision context that fully satisfied these conditions. Much to our surprise, however, we found that it is indeed possible to demonstrate instances of the SQB where inertial forces prevail over 'better' outcomes (Study 2.1 and Study 2.2). In Study 2.3, we required participants to overcome inertia in a single early trial. This simple manipulation reduced SQB and suggested an approach that could be useful in decreasing patient inertia.

2.1 The SQB Extends to Decisions with Inferior Defaults (Study 2.1)

Is the SQB applicable to contexts in which the default option has a clearly inferior value to the alternative option? To answer this question, we used the threat of electric shock, which enables personally salient, differentiable decisions. Prior studies have shown that given the choice of waiting for a shock versus getting it over with quickly, most people choose the latter (Berns et al., 2006) because they consider the dread of waiting for the shock to be worse than the shock itself. In the present study, we tested whether forming the SQB would further the default state and thus prevent participants from pressing a button that would reduce their waiting time for a shock.

Our initial intuition was that most people would choose to proactively press a button to reduce their waiting time to getting shocked. To assess how widely shared this intuition was, we conducted pre-study surveys of non-psychologists (80 responders) and psychologists (25 responders, with a graduate degree in psychology). Both surveys indicated that most responders believed that given an option to do so, study participants would proactively opt to shorten the trials. In both groups, over 80% of respondents said that shortening the trial was not a difficult decision and there was no rational reason not to do it. Both groups expected over 80% of participants to choose to shorten the waiting period in at least 75% of the trials. This suggests that in the eyes of these external observers, there were no rational reasons to stay with the status quo.

In the laboratory component of the study, we contrasted choices made by participants who were forced to make a choice between reducing their waiting time or keeping it the same, and participants who had the same choices available – except that there was no forced choice. Reducing the waiting time required a proactive button press and not pressing this button resulted in the waiting time remaining unchanged.

2.1.1 Method

Forty-one students (20 women) were randomly assigned to either a forced-choice group (20 participants, 9 women) or a proactive-choice group (21 participants, 11 women).

All participants were calibrated on the maximum level of electrical shock that they could tolerate. The calibrated intensity caused high anxiety in all participants. Participants were instructed that trials would be of varying lengths and that a single shock (at the calibrated level) could be administered at any time during each trial. Participants were informed that a large majority, but not all, trials would contain a shock. Trials containing a shock would end with the administration of the shock.

Participants were instructed to monitor their subjective anxiety levels during the trial. At the end of every trial they were asked to record their anxiety levels (on a 1 – 7 scale). As a cover story, participants were told that their subjective evaluations of anxiety would be compared to their physiological responses (obtained via a finger pulse monitor). This comparison was not an actual objective of the study – rather, it was used as a vehicle for obscuring our real interest, namely participants' choice behavior. Participants were informed that experimenters were indifferent to whether they reduced their waiting times since our focus was the link between subjective anxiety and physiological responses, and that we were indifferent to the absolute level of anxiety.

At the start of every trial, participants in the forced choice group were presented with a choice of pressing two buttons. Pressing one would shorten the waiting time by ten seconds and pressing the other would keep the waiting time unchanged. Participants in the proactive-choice group had the option of pressing a trial-shortening button at any time in the trial. Pressing this button reduced the waiting time by 10s. If the participant elected not to press the button, the waiting time remained unchanged. In both conditions, if a participant pressed the trial-shortening button, a different-colored screen lasting 10s appeared at the end of trial informing the participant that had she not elected to shorten the trial, those 10s would have been a part of the trial. In this way, the total trial time was kept constant; however, the button press would mean that the shock anticipation period was lessened.

Four practice trials were conducted in which the actual shock was replaced with an audio beep. The audio beep was used (instead of the shock) so that participants' anxiety would not prevent them from fully understanding the task. Fourteen experimental trials were administered. All except 1 of these trials (trial 4) ended with the administration of a shock. The exact number of trials was not revealed to participants.

2.1.2 Results and Discussion

Participants in the forced-choice condition (where they had to select between shortening the trial or not on every trial) chose to shorten for 74.7% of the trials. Participants in the proactive-choice condition (where they had to actively press the trial-reducing button, or else accept the status-quo) chose to shorten for 40.7% of the trials (Figure 2.1). The difference is significant, $t(39) = 3.2$, $p = .003$, and demonstrates that on many trials subjects chose to keep the shock anticipation time unchanged when this was presented as the status quo option; however they often did not make the same selection when it was not the status-quo.

To better understand the effects of our manipulation, participants in each group were divided into low (0-4 button presses), medium (5-9 button presses), and high (10-14 button presses) button-pressers. The proactive-choice group had 52% low button pressers, 19% medium button pressers and 29% high button pressers. The forced-choice group had 25% low button pressers, 0% medium button pressers and 75% high button pressers. The difference is significant: χ^2=10.09, df = 2, p=0.006. Neither sex nor average levels of anxiety were predictive of a participant being in the low, medium, or high sub-group.

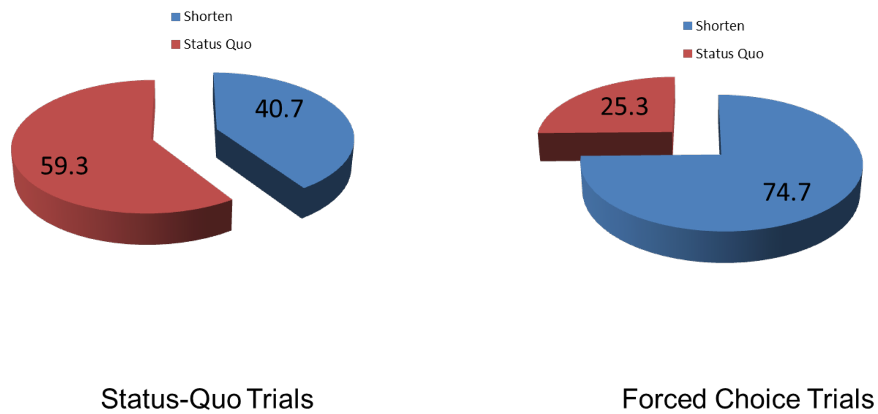

Figure 2.1, Study 2.1 Data: In Study 2.1, participants who were required to proactively press a trial-shortening button elected to shorten only 40.7% of the trials. Participants who were either required to press a button to shorten the trial, or press another button that would leave the waiting time unchanged, pressed it in 74.7% of the trials.

In post-experiment debriefings, 100% of participants indicated they did not feel that either the status quo option or its alternative were favored by the experimenters. Over 85% participants reported that the opportunity to shorten trials was personally salient to them, and over 78% of the participants indicated that they saw a clear difference between the status quo option and its alternative. Additionally, all participants reported that they understood that pressing the button could not hurt them – at worst their waiting time would remain unchanged.

Thus, contrary to our initial expectations – as well as those of our pre-study survey participants – these findings indicated that the SQB is evident even in decision contexts that involve an unfavorable default option (e.g. medical non-compliance). In

Study 2.2, we sought to "break" this effect by making the difference between the status quo option and its alternative even starker. Our intent was to find a condition that would be strong enough to eliminate the SQB.

2.2 Status-Quo Persistence Despite Strong Opposing Incentives (Study 2.2)

In Study 2.2, we tested whether the SQB would persist if participants were provided with an option that was even more obviously superior to the status quo. We reasoned that the SQB would be extinguished under these circumstances. Using the same paradigm as Study 2.1, we provided an option to press a button that would drastically reduce the probability of getting shocked. We were confident that the SQB would disappear; our plan was to gradually make the reward less salient, until the SQB reappeared once again.

As in Study 2.1, we conducted pre-study surveys with non-psychologists (100 responders) and psychologists (30 responders with graduate degrees in psychology). Findings from these surveys showed that both groups expected over 90% of participants to proactively press the shock-probability-reducing button for at least 75% of the trials. In both groups over 95% of respondents said that pressing the button was not a difficult decision and there was no rational reason not to press it.

In the laboratory component of the study, we contrasted choices made by participants who were forced to make a choice between reducing the probability of being shocked or keeping it the same, and participants who had the same choices available – except that there was no forced choice. In the latter group, reducing the probability of being shocked required a proactive button press and not pressing this button resulted in the probability of being shocked remaining unchanged.

2.2.1 Method

Forty students (22 women) participated in a study involving electrical stimulation. Twenty students (11 women) were randomly assigned to the proactive-choice group; the remaining 20 students (11 women) were randomly assigned to the forced-choice-group.

In procedures identical to those described in Study 2.1, participants in both groups were calibrated, informed about the trial structure, and asked to monitor and record their subjective anxiety so that it could be compared to physiological measures (which as in Study 2.1 was merely a cover to observe choice behavior). The number of practice trials (4) and experimental trials (14) was identical to Study 2.1 for both groups.

At the start of every trial, participants in the forced-choice group were required to make a choice between pressing two buttons. Pressing one would reduce the probability of being shocked in that trial by 90% (while keeping the magnitude of shock unchanged). Pressing the other button would keep the probability of getting shocked in that trial unchanged. Participants in the status-quo condition had the option of pressing an identical shock-probability-reducing button at any time in the trial. Pressing this button reduced the probability of being shocked in that trial by 90% (while keeping the magnitude of shock unchanged). If the participant elected not to press the button, the probability of getting shocked in that trial remained unchanged.

All participants were told that there was a small minority of trials during which they would not be shocked whether or not they pressed the shock-probability-reducing button. However, for most of the trials, pressing the button meant that shock would not be administered, whereas not pressing the button meant that they would be shocked. To ensure that participants fully understood, an explicit example was provided: “If you press the button every time in a set of ten trials, then on average you will not be shocked for nearly all of those trials. If you do not press the button in any of the ten trials, then on average you will be shocked for nearly all the trials.”

One concern was that participants might believe that choosing not to be shocked would have a detrimental effect on the study. To address this concern, participants were reminded that even if they pressed button every time, they would still have some anxiety in each trial since the probability of receiving the shock was reduced but not eliminated. They were told that this would be sufficient to compare their ratings with the physiological measure (which was the ostensible objective of the study). Thus, subjects were explicitly assured that whether or not they pressed the button was entirely up to them, and that the experimenters were completely indifferent to their choice.

Another concern was that participants would – despite being instructed to the contrary – believe that pressing the button would increase the probability of being shocked in shock-absent trials. However, none of the participants reported this belief in debriefings (they were specifically asked if this was the case). Further, as will be shown below participants in the comparison forced choice group pressed the button on most trials – even though this misunderstanding should have equally applied to them.

2.2.2 Results and Discussion

Participants in the forced-choice condition (where they actively selected one of two buttons on each trial) chose to reduce the probability of being shocked in 85.3%

of the trials. By contrast, participants in the proactive-choice condition (where they had to proactively press the shock-probability-reducing button, or else accept the status-quo) chose to reduce the probability of being shocked in only 52.1% of the trials (Figure 2.2). The difference is significant, t(38) = 3.3, *p* = 0.002.

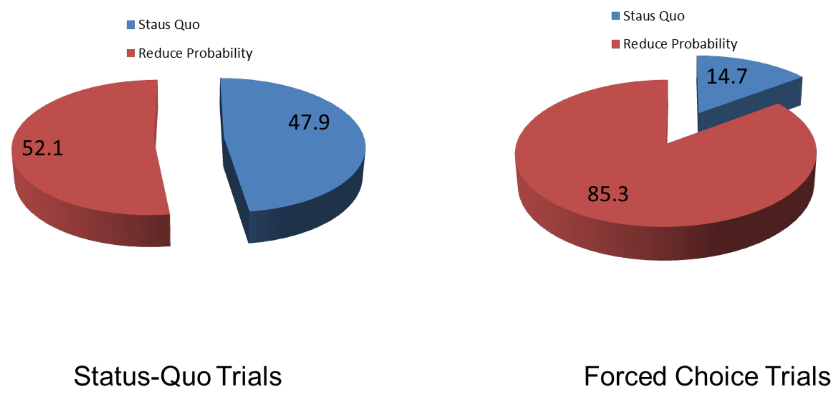

Figure 2.2, Study 2.2 Data: In Study 2.2, participants who were required to proactively press a shock-probability-reducing button elected to reduce the shock probability in only 52.1% of the trials. Participants who were either required to press a button to reduce the probability of being shocked, or press another button that would leave the probability of being shocked unchanged, pressed it in 85.3% of the trials.

To better understand the effects of our manipulation, participants in each group were divided into low (0-4 button presses), medium (5-9 button presses), and high (10-14 button presses) button-pressers. The proactive-choice group had 40% low button pressers, 10% medium button pressers and 50% high button pressers. The forced choice group had 0% low button pressers, 10% medium button pressers and 90% high button pressers. The difference was significant: χ^2=10.29, df = 2, p=0.005. Neither sex nor average levels of anxiety were predictive of a participant being in the low, medium, or high sub-group.

In post-experiment debriefings, 100% of the subjects acknowledged that they expected that almost everyone would frequently press the shock-probability-reducing button. They could not explain why they themselves had not used this option.

Like our participants, we too were puzzled at this unexpected result. It seemed that the SQB not only existed in cases with slightly inferior default options, but that it extended to options akin to those found in medical non-compliance where sticking to the default led to personally harmful outcomes. In Study 2.3, we sought to find ways to reduce the SQB.

2.3 An Intervention to Reduce the SQB (Study 2.3)

Studies 2.1 and 2.2 demonstrated that participants chose to stay with default options despite the fact they could have made themselves better off by proactively taking action (i.e. a button press). This parallel with patient inaction suggests that the SQB could underlie some instances of patient inertia. It is thus important to demonstrate manipulations that are successful in reducing SQB.

One such manipulation could be to *require* participants to press the shock-probability-reducing button early in the experiment. This would remove participants' resting state inertia thereby reducing their SQB. Support for the potential effectiveness of such a manipulation was found in the pattern of early button pressing in Study 2.1 and 2.2: participants who pressed the button 3 or 4 times in the first four trials were over 6 times more likely to press the button in subsequent trials compared to participants who pressed the button 0 or 1 times in the first 4 trials (74% vs. 12% respectively, $p<0.001$). Further, the likelihood of choosing the 'change' alternative increased with trial number: regressing the number of button presses for proactive-choice group participants against the trial number yields a positive slope of 0.39 (95% CI [0.07, 0.71]). These observations suggest that button presses in early trials facilitate a reduction in SQB.

2.3.1 Method

Forty-three students (22 women) participated in a study involving electrical stimulation. Twenty students (11 women) were randomly assigned to the 'replication' group, in which procedures identical to those used for the proactive-choice group in Study 2.2 were used. The remaining 23 students (11 women) were assigned to the 'mandatory' group.

The mandatory group followed procedures identical to those of the replication group with one important exception, namely that there were two additional mandatory trials administered before the 14 experimental trials. Participants were instructed to press the shock-probability-reducing button in one, but not both, of these two mandatory trials – they were free to choose the order. The mandatory trials were presented as routine training trials, serving only to clarify the experiment structure. Participants were not biased because the mandatory trials were balanced – one required button-pressing, and the other did not.

2.3.2 Results and Discussion

As expected, the results of the replication group were nearly identical to those of the proactive-choice group in Study 2.2. Participants pressed the shock-probability-reducing button 48.6% of the time. More importantly, in the mandatory group, participants pressed the shock-probability-reducing button 77.64% of time. This is significantly different from the replication group ($t(41) = 2.71$, $p = 0.009$), and indistinguishable from the forced-choice group in Study 2.2 ($t(41) = 1.09$, $p = 0.28$).

Study 2.3 suggests that demand characteristics are not playing a crucial role in determining participant behavior. If participants in Study 2.2 were avoiding reducing the probability of being shocked because they thought that was what the experimenters were hoping for, participants' behavior should have been unaffected by the two balanced mandatory trials in the mandatory group of Study 2.3. Similarly, Study 2.3 shows that participants were not acting with the misunderstanding that pressing the button would increase the probability of being shocked in shock-absent trials, or else the mandatory trials would not have affected participant behavior.

2.4 General Discussion

When doing nothing was the status quo, for most trials in Study 2.1 participants chose not to press a button that would have reduced their time waiting for a personally salient and highly aversive shock. When they were forced to make a choice, a large majority of participants preferred to reduce their waiting time. In Study 2.2, many participants persisted with the status quo despite having the option to eliminate the possibility of being shocked in nearly every trial. These findings were a surprise to us, and they demonstrated that the SQB exists even in decision contexts in which the status quo option was unambiguously less attractive than the alternatives. In Study 2.3 we showed that requiring the participants to make an early button-press could reduce their SQB in later trials.

One implication of our findings is that it may be necessary to re-examine traditional explanations for the SQB. Many behavioral economists favor loss aversion

– the tendency of people to prefer avoiding losses to acquiring gains – as an underlying mechanism for the SQB (Kahneman, Knetsch & Thaler, 1991). Yet loss aversion is not relevant to the decision contexts in Study 2.1 and Study 2.2 since there is nothing but gain associated with leaving the default option. Another mechanism hypothesized to underlie the SQB is omission predisposition (Ritov & Baron, 1992). The idea here is that people generally prefer inaction over action and thus choose options that are weighted towards inaction, which is often the default choice. However, the results of Study 2.3 are not consistent with the omission bias account since mandating a button press in the practice trials should not have affected the omission predisposition in later trials.

In our studies, it was the level of decision support provided to the participant that seemed to drive participant choice. The forced-choice group in Study 2.1 and 2.2 was presented with repeatedly marked choice points. No such support was provided to the proactive-choice group – and the results between the two groups were markedly different. In Study 2.3 a different kind of support was provided to the mandatory group – namely participants were given behavioral experience with pushing the shock-probability-reducing button. This seemingly minor support was enough to overcome the SQB.

In their influential book *Nudge,* Thaler and Sunstein (2008) identified several decisions in which individuals could be nudged to select more optimal options as long as these options were made to be the default options. However, this is frequently not possible. For example, it is difficult to mandate that people get flu vaccinations or get medical check-ups on a regular basis. In such cases, it is important to provide individuals with sufficient support to overcome their inaction inertia (or other default state). Our findings from Study 2.3 suggest an effective way to do this would be to focus resources to induce individuals to try the recommended option once. After they've completed the activity for the first time, their psychological inertia (Gal, 2006) would make it easier for them repeat the action. This suggests that efforts focusing on getting individuals to commence taking their medications as prescribed, or going for their first medical check-up, or going for a first run, may lead to the overcoming of patient inertia and the initiation of lasting compliance behavior.

It will be important for future studies to extend the present work by demonstrating the SQB in actual (rather than laboratory) decision contexts, particularly in the context of medical non-compliance. Second, future studies must also test whether the manipulation in Study 2.3 (mandating compliance outcomes in early trials) can impact patient compliance behavior.

CHAPTER 3

CHOOSING TO REAPPRAISE:

IT'S LESS COMMON THAN YOU MIGHT THINK

Note to readers:

This chapter is adapted from a paper published in *Emotion*:

Suri, G., Whittaker, K., & Gross, J.J. (2015). Choosing to reappraise: It's less common than you might think *Emotion, 15,* 73–77.

3.0 Introduction

Like any other motivated behavior, emotion regulation can be thought to occur as a joint function of its costs and its benefits. It stands to reason that people should regulate their emotions if they derive a clear hedonic benefit from doing so (e.g., decreasing negative feelings) and if the costs of regulation are low. There may be situations in which people are not aware that they can regulate their emotions or in which people have instrumental motives to leave their emotions as they are (e.g., instrumental motives to maintain negative emotions – Tamir, Mitchell, & Gross, 2008), but barring these, it seems plausible that most emotional episodes are regulated whenever it is helpful to do so. But do adults actually do this?

In the present work, we sought to empirically address this important question. As a test case, we focused on one of the best-researched forms of explicit emotion regulation – cognitive reappraisal. Cognitive reappraisal is a cognitive-linguistic strategy that alters the trajectory of emotional responses by reformulating the meaning of a situation (Gross, 2014). It seemed a good test case because compared to people who use reappraisal infrequently, frequent reappraisers show self-reported affective, cognitive, and social benefits (Gross & John, 2003).

We created a laboratory decision context in which participants were asked to watch negatively valenced affective images in a series of trials. On each viewing, they had the option of electing to reappraise in order to decrease negative affect and thus derive hedonic benefits. The cost of choosing to reappraise was a simple button press requiring negligible effort. Our task was designed to ensure that participants had no instrumental motives to maintain negative emotion. Further, participants were given detailed pre-experiment instructions on how to reappraise. Participants thus knew both that they could reappraise, and how to do so in this context.

We expected that participants would elect to reappraise for the vast majority of trials. A pre-study survey of 108 adult Mechanical Turk (MTurk) workers suggested that our intuition was generally shared. We asked survey-responders to predict participant behavior in the decision context outlined above. Reappraisals were predicted, on average, for over 70% of trials.

However, this shared intuition was incorrect. In Study 3.1, participants decided to proactively reappraise only 16.1% of all viewed images. In Study 3.2, we demonstrated that regulatory rates remained low for both low and high intensity images, even when another regulatory option (i.e. distraction) was available.

We reasoned that there were two potential factors that might have contributed to the puzzlingly low rate of reappraisal. First, participants might have found reappraisal difficult to execute. Thus the costs of reappraisal may have exceeded its benefits leading to a decision not to reappraise. Second, default factors may have played a role. Decision theorists (e.g. Samuelson & Zeckhauser, 1988) have documented that people are disproportionately likely to stick with a default option compared to equivalent decision contexts in which the same option is not designated to be the default. In Study 3.1 and Study 3.2, reappraisal difficulty was not manipulated and watching was designated to be the default option. In Study 3.3, we manipulated these two factors and measured impacts on regulation choice.

3.1 Proactive Reappraisals Occur in a Small Fraction of Emotional Events (Study 3.1)

We sought to measure the percentage of trials in which participants chose to reappraise (versus watch) while viewing a series of negatively valenced images.

3.1.1 Method

Forty undergraduates (24 women) were each given detailed reappraisal instructions, 6 practice trials, and 40 experimental trials. In each trial, participants were first briefly shown (0.5 seconds) a negatively valenced IAPS image. They were then shown a choice screen (for 15 seconds with a timer ticking off the seconds) informing them that if they did nothing, their instruction would be to "Watch" the image. The bottom half of the screen reminded the participants that they could elect press 'c' to change their instruction from "Watch" to "Reappraise."

Participants could press 'c' at any time during the choice screen. The choice screen was given a long duration to ensure that there was minimal possibility of participant inaction due to a shortage of time. After 15 seconds of the choice screen, an instruction screen appeared for 2 seconds. If the participant had pressed 'c' in the choice screen, their instruction screen asked them to "Reappraise", else their instruction was to "Watch".

Following the instruction screen, the image (that had been flashed previously) appeared for 5 seconds. During this time participants were asked to implement their instruction (i.e. "Watch" or "Reappraise"). Prior studies have shown that in emotion regulation choice contexts, participants do follow such instructions (Sheppes, Scheibe, Suri & Gross, 2011). The implementation time was limited to 5 seconds to reduce the possibility of participants following the instruction at the outset, but then changing their minds. Participants were explicitly asked not to distract or look away during the watch trials and not avoid attempting reappraisal during the reappraisal trials. Subsequently, participants were asked to rate their affect on a 0-10 scale.

Before the experiment started, participants were (mis)informed that the experimenters sought to measure the physiological effects of watching and reappraising, and a finger cuff was attached. This was to minimize demand characteristics (which could have provided participants with instrumental goals not to reappraise) by drawing participants' attention away from their choice as the DV of interest to the experimenters. No physiological data were actually collected.

In post-experiment debriefings, 100% of the participants in Study 3.1 (and also in Studies 3.2 and 3.3) stated that they believed the cover story about the purpose of the experiment. All participants also stated that they did not feel that the experimenters wanted them to choose any one option over the other. Further, all participants agreed with the statement that they had no difficulty in following instructions and complied with them fully. Finally, there were no data exclusions or additional manipulations in Study 3.1 (or Studies 3.2 and 3.3).

3.1.2 Results

On average, participants chose to reappraise during only 16.1% of trials (95% CI [9.6% - 22.4%]).

Participants felt less negative for reappraise trials than watch trials. The post trial affect rating (higher numbers represent less negative ratings) was 4.45 for trials that were reappraised and 3.95 for trials that were not reappraised (i.e. watched). The difference was significant (reappraise vs. watch, $t(1598) = 3.87$, $p < 0.001$, $d = 0.23$); comparison to a mixed-effects model showed that there were no (random) subject effects. This difference in reported affect was not driven by the normative valence ratings of the stimuli that were selected for reappraisal (mean valence 3.05) versus those that were not (mean valence 3.12). In the International Affective Picture System, lower scores on valence represent higher levels of negative emotion.

Despite the clear affective benefits associated with reappraisal, the percentage of trials for which proactive reappraisals were attempted was puzzlingly low and was counter to our pre-study expectations. In Study 3.2 we investigated whether this effect was driven by the absence of regulatory options that were best suited to the intensity of the displayed images.

3.2 Reappraisal Rates Are Not Affected by Image Intensity or Regulatory Options (Study 3.2)

Prior research has demonstrated that people tend to choose reappraisal as a regulation strategy for low intensity stimuli and distraction as a regulation strategy for high intensity stimuli (Sheppes, Scheibe, Suri & Gross, 2011). This led us to consider the possibility that participants in Study 3.1 might have avoided reappraising because they preferred to use distraction as a regulatory strategy (at least for the high-intensity images on offer). Since distraction was not an option, they might have settled for watching the images instead.

To test this possibility, we measured reappraisal rates for low and high intensity images in a decision context that included distraction as a regulatory option.

3.2.1 Method

Following Sheppes, Scheibe, Suri & Gross (2011), we created a set of 15 High Intensity (HI) (mean arousal = 6.12; mean valence = 1.99) and 15 Low Intensity (LI) images (mean arousal = 5.01; mean valence = 3.41). These image sets were identical to those used in prior research.

Twenty-five undergraduates (13 women) were asked to view a shuffled set of HI and LI images. They were given training and instructions identical to those used in Study 3.1 with the following exceptions. In addition to being taught to reappraise, participants were trained to distract by thinking of a neutral activity. As before, they were given a default instruction of “Watch” but a sign on the bottom half of the choice screen now reminded them that they could press ‘r’ to reappraise or ‘d’ to distract. If they elected to do neither, an instruction screen informed them that they were to watch the image; else they were asked to reappraise the image (if they had pressed ‘r’ in the choice screen) or distract from it (if they had pressed ‘d’ in the choice screen).

The other instructions, practice trials, and misdirection with the physiological monitor were identical to Study 3.1. As before no physiological data were recorded and as in Study 3.1, 100% of the participants stated that they believed the cover story about the purpose of the experiment and did not feel that the experimenters wanted them to choose any one option over the other.

3.2.2 Results

On average, participants chose to reappraise during only 12.4% of total trials (95% CI [9.9% - 14.8%]). The reappraisal rate for HI trials was 12% and for LI trials was 12.8%. The reappraisal rates were not moderated by intensity ($t(48) = 0.26$, $p = 0.79$).

Participants chose to distract during 10.6% of HI trials and 7.4% of LI trials (9.0% overall, 95% CI [6.7% - 11.43%]). These rates were not moderated by intensity

($t(48) = 1.07$, $p = 0.29$). Participants watched 77.3% of HI trials and 79.7% of LI trials (78.5% overall, 95% CI [74.7% - 82.3%]). These rates were also not moderated by intensity ($t(48) = 0.55$, $p = 0.58$).

These results do not contradict earlier studies that found that regulation choice was driven by intensity (Sheppes, Scheibe, Suri & Gross, 2011). In the current decision context, the vast majority of trials were watched, not regulated; there was thus inadequate power to test whether the type of regulation chosen (in the subset of trials that were regulated) was moderated by intensity.

Study 3.2 findings suggest that it is unlikely that the low rates of reappraisal in Study 3.1 can be explained by the high negative valence of the pictures. Further, compared to Study 3.1, reappraisal rates did not increase (for either HI or LI images) with the presence of attentional distraction as an alternative regulatory option. These results also demonstrate that low rates of regulation are not applicable to reappraisal only. When both reappraisal and distraction were available, participants chose to stay with the default "watch" option in 78.5% of all trials.

As in Study 3.1, reappraisal increased post-trial affect ratings. The post trial affect rating (higher numbers represent less negative ratings) was 4.42 for trials that were reappraised and 3.87 for trials that were watched – reappraise vs. watch, $t(748) = 2.89$, $p = 0.002$, $d=0.21$. This difference in reported affect was not driven by the normative valence ratings of the stimuli that were selected for reappraisal (mean valence 2.68) versus those that were watched (mean valence 2.77). As in Study 3.1 lower valence scores represent higher levels of negative emotion.

In Study 3.3, we continued to focus on reappraisal as a test case for investigating regulation frequency and sought to identify the drivers of the low rates of reappraisal.

3.3 Removing Defaults Increases Reappraisal (Study 3.3)

The benefits of reappraisal are numerous and profound (Gross, 2014). Despite these benefits, and despite higher affect ratings for reappraisal trials, reappraisal was attempted at low rates in Study 3.1 and Study 3.2. This suggests that there might be costs associated with reappraisal. We investigated two types of costs that we thought might contribute to these low rates: (1) Costs of overcoming default preferences and (2) Cognitive costs related to generating new reappraisals.

The first type of cost of reappraisal use is overcoming the bias shown by decision makers that leads them to prefer a default option (Dinner, Johnson, Goldstein & Liu, 2010). A default option is defined as the option that is chosen if the decision-maker does not act. Default preferences are seen as a bias because merely designating an option as the default increases the frequency of its selection even though its attractiveness remains unchanged. Such preferences have been observed in many decision domains including organ donation (Johnson & Goldstein, 2003) and retirement plans (Beshears et al., 2009). We reasoned that if participants in Study 3.1 and Study 3.2 had failed to choose reappraisal because they were biased toward the default (i.e. watching), then removing the default option would increase the percentage of trials on which they would choose to reappraise.

The second type of cost of reappraisal use is the cognitive effort associated with reappraisal. We reasoned that if participants in Study 3.1 and Study 3.2 had failed to choose reappraisal because of the effort associated with formulating an effective reappraisal, then providing them such reappraisals would increase the percentage of trials on which they would choose to reappraise.

To examine the simple and interactive effects of these two types of costs, we created (1) a proactive condition that contained a default instruction, and a marked choice condition in which participants had an explicit choice between watching or reappraising, but there was no default instruction; and (2) we created a support condition, in which participants were provided with a plausible reappraisal for each image in the experiment, and a no-support condition in which participants had to create their own reappraisals (if they chose to reappraise). We used a fully crossed 2x2 design. We expected to replicate the findings of Study 3.1 and Study 3.2 for the group in which defaults were present and no support was provided. We expected other groups to show higher rates of reappraisal use.

3.3.1 Method

Eighty-eight undergraduates were randomly assigned to the following 4 equal groups of 22 (11 women in each): 1) Default Choice, No Support 2) Default Choice With Support 3) No Default, No Support and 4) No Default With Support. Each group completed 40 trials.

The Default Choice, No Support group (Group 1) received instructions identical to those described in Study 3.1, with one exception – the default instruction was "Watch" for 50% of the trials and "Reappraise" for 50% of the trials. This allowed us to test whether participants had a preference for the default, or whether they simply had a preference for watching images. If participants did nothing, they were asked to stay with the default instruction. If they pressed ‘c’, their instruction was changed (from Watch to Reappraise or from Reappraise to Watch).

The Default choice With Support group (Group 2) was similar to Group 1 with one exception: both instructions (Watch and Reappraise) were accompanied with two distinct short comments that, in the Watch condition, described the image neutrally (e.g. “This image displays a wound.”) and, in the Reappraise condition, reinterpreted the meaning of the image in a less negative way (e.g. “This is an incision made by surgeon.”). These comments were shown just below the respective strategies on the slide on the choice screen described in Study 3.2.

The No Default, No Support group (Group 3) did not contain a default instruction and did not suggest reappraisal. In the choice screen the two choices (Watch and Reappraise) were presented side by side (counterbalanced) and separated by a vertical line. Subjects needed to press separate buttons in order to choose between the two alternatives.

The No Default With Support group (Group 4) was similar to Group 3 (two choices presented side by side on choice screen). However, as in Group 2, two distinct short comments either describing the image or suggesting an alternative appraisal accompanied the Watch and Reappraise instructions.

The instructions, practice trials, and misdirection with the physiological monitor were identical to Study 3.1 and 3.2 for all four groups. As before no data were recorded and 100% of the participants stated that they believed the cover story about the purpose of the experiment and did not feel that the experimenters wanted them to choose any one option over the other.

3.3.2 Results and Discussion

Reappraisal rates were compared using a 2 (default condition – "Watch" default vs. no default) x 2 (support condition – reappraisal support vs. no support) analysis of variance. This analysis revealed no interaction effect, $F(1, 84) = 0.005$, $p = 0.93$. However, the main effect of default was significant, $F(1, 84) = 49.28$, $p < 0.001$. Follow-up t-tests indicated that that removing the requirement to proactively override the default instruction, and instead providing the "Watch" and "Reappraise" choices without a default, increased the number of trials for which the reappraise option was chosen. There was no main effect of support, $F(1, 84) = 3.36$, $p = 0.07$. Providing reappraisal support did not significantly increase reappraisal choice (there was however a trend that suggested that providing reappraisal support may marginally increase the percentage trials reappraised). Mean reappraisal choice by condition is shown in Figure 3.1.

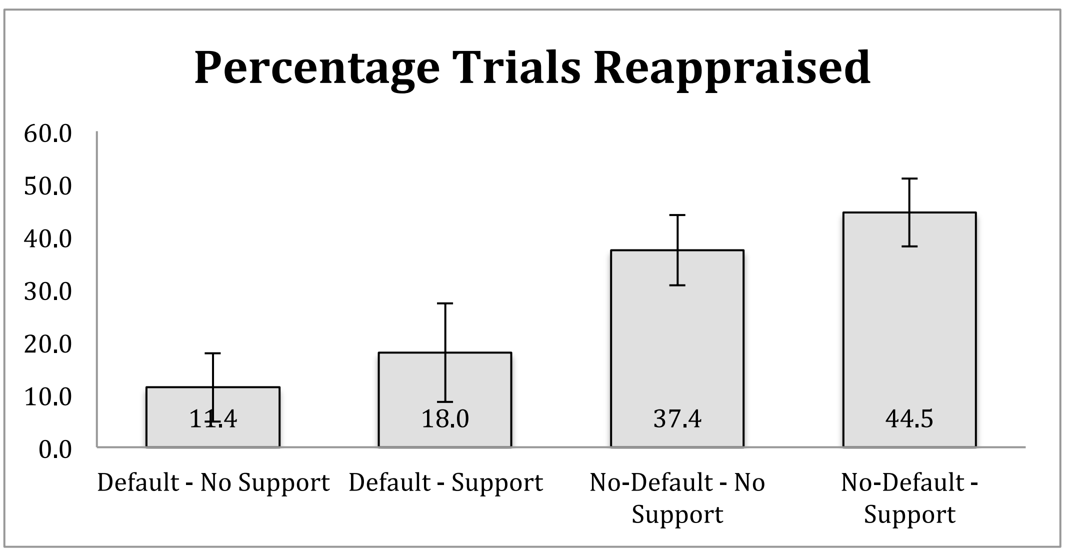

Figure 3.1, Study 3.3 Data: Mean reappraisal choice by condition. When watching was the default, the percentage of reappraisals was low. When the default was removed, the percentage of reappraisals significantly increased. Providing support did not yield a significant main effect. Error bars represent 95% confidence intervals.

The strong main effect created by removing the default instruction suggests that default preferences played a powerful role in influencing participant choices. To further examine this effect, we took advantage of the fact that in Group 1 and 2, participants received different default instructions. When the default instruction was “Watch”, participants elected to keep the default in 84.7% of trials (meaning that they reappraised in only 15.3% of trials, which nearly exactly replicates Study 3.2). When the default instruction was “Reappraise”, participants elected to keep the default in 78.3% of trials (meaning that they reappraised in 78.3% of trials). Responses in these two default conditions were significantly different $t(86) = 12.6$, $p < 0.001$.

Finally, as in Study 3.1 and 3.2, reappraisal increased post-trial affect ratings. The post trial affect rating (higher numbers represent less negative ratings) was 4.51 for trials that were reappraised and 3.72 for trials that were not reappraised (i.e. watched) – reappraise vs. watch, $t(3518) = 12.67$, $p < 0.001$, $d = 0.42$. This difference in reported affect was not driven by the normative valence ratings of the stimuli that were selected for reappraisal (mean valence 3.03) versus those that were not (mean valence 3.19).

3.4 General Discussion

It has often been assumed that healthy individuals reappraise whenever they need to – unless they are not aware of the benefits of reappraisal or they have instrumental goals that take priority. Collectively, this study series shows that even in decision contexts offering apparent hedonic benefits and requiring minimal costs, reappraisals are often not attempted due – at least in part -- to default preferences.

Specifically, these studies showed that in a laboratory decision context reappraisals were implemented for only 16% of the available opportunities (Study 3.1). Reappraisal rates were low for both high and low intensity images, and were not affected by the availability of distraction as an alternative regulatory option (Study 3.2). Default preferences seemed to drive decisions to not reappraise. When defaults were removed, reappraisal rates increased by 278% (Study 3.3).

Emotion regulation choice is a rapidly growing area of research (Gross, 2014). Several papers (e.g. Sheppes, Schiebe, Suri & Gross, 2011; Sheppes, Scheibe, Suri, Radu, Blechert, & Gross, 2014) have examined the factors underlying regulatory decisions. These studies have shown that selection often varies by contextual demand. For example, healthy participants have been shown to use reappraisal in low emotional intensity contexts and distraction in high emotional intensity contexts (we did not observe this in Study 3.2 due to the very large number of trials for which participants chose neither form of regulation). However, the primary focus of these studies has been on affective drivers of context (e.g. intensity of stimuli, nature of regulatory options). In the present studies, we found that non-affective contextual drivers – such as the designation of a default option – can play a large role in shaping regulatory choice.

In decision theory, defaults have been recognized to be important contextual variables that shape choice. Default behavior has been cited as mechanism underlying several decision contexts, including choices involving electrical service providers (Hartman, Doane & Woo 1991), organ donation (Johnson & Goldstein, 2003), 401(K) plans (Beshears et al., 2009), investment portfolios (Amerkis & Zeldes, 2001), patient inertia (Suri, Sheppes, Schwarz & Gross, 2013), and choices in health plans (Samuelson & Zeckhauser, 1988). It is thus not altogether surprising that defaults would also play a role in decisions involving emotion regulation.

What is surprising – at least to us – is the strength of the effect exerted by default designations. In Study 3.2, the default instruction to watch equally shaped choice for high and low intensity images. In Study 3.3, removing the default designation nearly tripled reappraisal rates.

In the context of everyday situations calling for explicit regulation, the default state – by definition – is to do nothing and experience the emotion. This study series suggests that many such everyday situations may go unregulated even though proactive regulation may have offered hedonic benefits.

Future studies must also evaluate the clinical implications of the above results. Prior work on emotion regulation and psychopathology has suggested that compared to healthy controls, people who suffer from psychological disorders tend to regulate their emotions inadequately (e.g. Kring & Sloan, 2009). The present work suggests that the absence of emotion regulation may sometimes be driven by contextual variables – such as the presence of defaults. It is possible that there are intrinsic or environmental factors that create more inflexible default behavior in clinical populations.

One notable feature of these results is that even when default preferences are removed and reappraisal facilitation is provided, reappraisal rates remained below 50%, countering the common assumption that reappraisal is ubiquitous (John & Gross, 2007). It is possible that this result is driven by either a hidden cost of reappraisal that we have not investigated in Study 3.3, or by a counterintuitive benefit of feeling affect even though it is negative. Future studies are needed to investigate this issue.

Our studies were designed to get initial purchase on an immensely complicated question. We thus made several scope simplifications: we focused on reappraisals as a test case (and not other forms of emotion regulation), healthy controls (not patients), affect induced by negative images in a laboratory context (not the different categories of naturalistic affect encountered in daily life), cued regulatory instructions (not spontaneous regulation (Aldao, 2013)) and conscious regulation (not automatic regulation (Gyurak, Gross & Etkin, 2011)). Further studies are required to examine these different contexts. Such studies may provide a deeper understanding of how and when people decide to regulate their emotions.

CHAPTER 4

THE ROLE OF ATTENTION IN MOTIVATED BEHAVIOR

Note to readers:

This chapter is adapted from a paper published in the Journal of Experimental Psychology: General:

Suri, G., & Gross, J.J. (in press). The role of attention in motivated behavior. *Journal of Experimental Psychology: General.* doi: http://dx.doi.org/10.1037/xge0000088

4.0 Introduction

Human behavior sometimes appears to defy explanation. For example, patients frequently do not take medications that are crucial to their well-being (Morris & Schulz, 1992) and employees do not start retirement accounts that are important to their financial future (Beshears, Choi, Laibson, & Madrian, 2006).

Researchers have attempted to explain such puzzling behavior by analyzing their (potentially hidden) motivational drivers. This quest is founded on the assumption that all instrumental behavior is *energized* and *directed* by motivational forces (Elliot & Covington, 2001). The energization of behavior refers to its activation, and the direction of behavior refers to whether the individual approaches or avoids objects, events, or internal representations (Elliot, 2006).

The motivational forces that give rise to motivated behavior are thought to be the result of *valuation*, which involves classifying things as 'good for me' or 'bad for me.' Ochsner and Gross (2014) suggest that multiple valuations often are computed for a given stimulus. These vary along a continuum of representational complexity, from core valuations representing relatively direct associations between a stimulus and an action (e.g. reaching for an apple) to conceptual valuations representing appraisals that are abstract and often verbalizable (e.g. I want to avoid the escalator and take the stairs because it is healthier).

If people are not taking their medications or signing up for favorable retirement accounts, then according to traditional motivational accounts, a lack-of-approach motivation, or an avoidance motivation (founded upon a 'bad for me' valuation) must be present. Such motivations may sometimes be based on contextual variables that obviously affect valuations (e.g. undesirable side effects of medication or retirement forms requiring a large time commitment); other times the contextual variables may be subtle and harder to detect. In their seminal paper, Samuelson and Zeckhauser (1988) hypothesized two potential sources of such relatively subtle avoidance motivation: choice difficulty and loss aversion. Choice difficulty refers to costly mental effort required to evaluate whether or not an action should be pursued (cf. Shah & Oppenheimer, 2008). Loss aversion (Kahneman, Knetsch & Thaler, 1991) refers to people's tendency to prefer avoiding losses to acquiring gains. It may cause (inferior) current-state preferences if the gains associated with leaving one's current state are valued to be less significant than the potential losses associated with leaving one's current state, even though objective valuations of rational decision theories would have valued the gains and losses equivalently (or even valued the gains to be higher than the losses).

Unfortunately for traditional motivational accounts, there seem to be cases in which neither obvious nor subtle contextual variables are evident – and yet the expected valuation-based behavior does not occur. For example, medical compliance rates are known to be low even when obvious contextual variables such as drug side effects or prescription costs are not a relevant factor (Joyner-Grantham et al., 2009). Subtle contextual variables also do not appear to apply in this context: the value of taking one's medicine is seldom in question and does not require costly analysis and there are few, if any, losses associated with leaving the current state of being unwell. Similarly, many company retirement plans do not entail a time-consuming application process, out of pocket costs, a high degree of choice difficulty, or potential losses upon leaving the current state (of an uncertain financial future) – and yet a large fraction of employees do not enroll in them (Madrian & Shea, 2000).

Building on work on attention and motivation (Carver, 1979; Carver, & Scheier, 1981; Pessoa, McKenna, Gutierrez, & Ungerleider, 2002), implementation intentions (Gollwitzer & Sheeran, 2006), and value-driven decision making (Rangel, Camerer, & Montague, 2008), we consider the possibility that valuation processes require attention in order to be translated into motivated behavior. Since valuation is the engine for motivation, this implies that motivated behavior is predicated upon attention (Figure 4.1). On this view, motivated behavior with respect to a given stimulus can only occur if that stimulus is attended to.

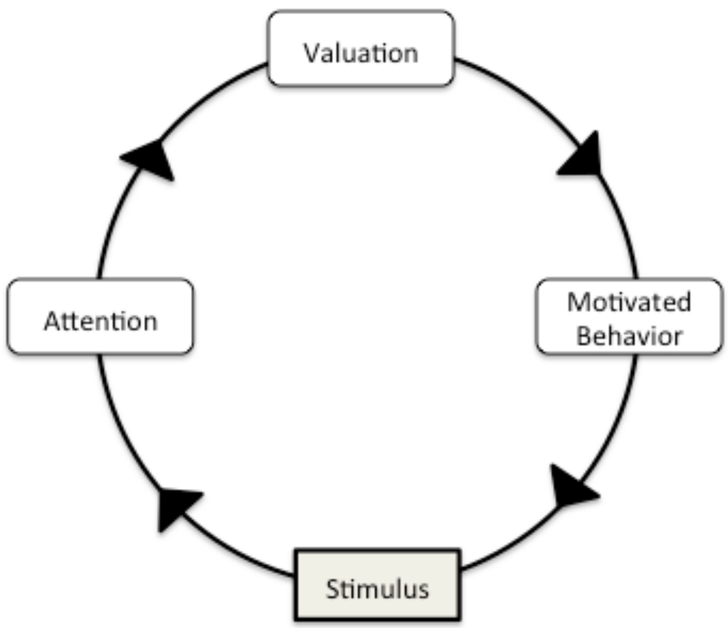

Figure 4.1, The Valuation Cycle: We propose that a stimulus must receive attention in order for it to be valued and for motivated behavior to occur (with respect to that stimulus). Motivated behavior may change the state of that stimulus which may cause the cycle to repeat.

Attention is not a unitary construct. Well-accepted frameworks of attention have described several types of attention. Here we are specifically focused on orienting attention, which enables the ability to prioritize input by selecting a modality or location (Petersen & Posner, 2012). On our account, valuation processes cannot be completed without a minimum level of orienting attention. This means that one reason that people don't take medications or start retirement accounts might be that during the course of their day such stimuli (the medicine bottle or the retirement forms) are not adequately prioritized and attended to.

The hypothesis that valuation (and therefore motivated behavior) requires attention may initially seem puzzling. We don't usually need to try to pay attention to a piece of cake before being motivated to eat it. Nor do we need to try to pay attention to a snake before being motivated to step back. However, the lack of effortful attention in these cases does not mean that attention is not required. This is because some affectively-laden states of the world are known to automatically capture attention (Carretié et al., 2004). Such stimuli are typically evolutionarily 'hard-wired' to elicit 'bottom-up' attentional and perceptual prioritization (Öhman, Flykt & Esteves, 2001). However, other stimuli – even stimuli whose effects are equally consequential – do not automatically capture attention. On our account, the valuation of such stimuli requires the implementation of 'top-down' attentional processes. For example, the act of choosing a healthy snack over an unhealthy one often requires attention towards one's eating behavior.

To test whether attention is necessary for valuation in such cases (and therefore for motivated behavior), we first developed a laboratory analog of the puzzling behaviors discussed above (Studies 4.1a-c). We created a context in which participants, according to motivational accounts, should always proactively leave their current state to approach positive stimuli or avoid negative stimuli. We then manipulated orienting attention. We hypothesized that motivated approach or avoidance behavior should be more evident in a high attention group compared to a low attention group. In Study 4.2 and Studies 4.3a-c, we sought to demonstrate the effects of increased attention on real-world behaviors. In Study 4.2 we tested whether a sign that read “APPLES” could increase apple sales in company cafeterias. This sign was not designed to affect the valuation of apples (a sign that read “SWEET APPLES” may have increased valuation); rather it was designed to increase attention towards the apples. In Study 4.3a we tested whether signs that read “Stairs” and “Stairs or Escalator?” would increase the number of pedestrians choosing to take the stairs at the stair/escalator choice point. These signs were not designed to affect the valuation of taking the stairs (a sign that read “TAKE THE STAIRS FOR YOUR HEALTH” may have increased valuation); rather they were designed to increase attention towards the stairs. Finding an increase in stair-climbing rates, we then tested whether this effect could be attributed to routine-disruption (Study 4.3b) or subtle demand characteristics (Study 4.3c).

4.1 Varying Levels of Attention Affects Motivated Behavior in the Laboratory (Study 4.1)

To investigate the role of attention in motivated behavior, we first sought to recreate in the laboratory the behavioral puzzles described above. In particular, we sought a context in which participants would frequently fail to act even though valuation processes implied that they would act. We then sought to test whether increasing attention levels would increase levels of proactive behavior.

These goals were best served by a behavioral context in which the valuation process is well understood. The viewing of affective images provides one such context (Lang, Bradley & Cuthbert, 1999). According to a standard hedonic account (Higgins, 1998), the valuation associated with viewing a higher-valenced (more pleasant or less negative) image is greater than the valuation associated with viewing a lower valenced image. Thus, barring error or idiosyncratic preferences, participants should act to view higher valenced images. Our experimental results were consistent with this prediction (Study 4.1a). As detailed below, when participants were asked to indicate their preferences by pressing one button to view a higher valenced image and another button to view a lower valenced image, they indicated a preference for viewing the higher valenced image in nearly every trial.

However, when participants' orienting attention was no longer forcibly directed to the buttons, and when they were instead required to *proactively* press a button to switch from viewing a lower-valenced image to a higher-valenced image in a series of trials, they did so infrequently. This provided an analogue to the behavioral puzzles described above, in that participants were not acting in accord with their preferences. This then enabled us to test whether increasing attention in this image-switching context would increase proactive behavior (Study 4.1b). Finding this to be the case, we then tested whether subtle demand effects could account for the observed results (Study 4.1c).

4.1.1 When Given a Choice, Participants Prefer Viewing Higher Valenced Images (Study 4.1a)

We sought to determine whether, when provided with an explicit, binary choice, participants would prefer viewing a higher valenced image over a lower valenced image.

4.1.1.1 Method

We created a series of forty trials in each of which 40 participants (24 women) (sample size based on effect sizes observed in pilot studies) were asked to indicate viewing preferences between a pair of affective images. Three types of images were used in the experiment: positive images depicted beautiful scenes from nature, neutral images depicted everyday items such as umbrellas, and negative images depicted images known to create disgust.

In each trial, a pair of images was sequentially presented for 1s. There were an equal number – twenty – of two types of trials: negative-to-neutral trials and neutral-to-positive trials. In negative-to-neutral trials the negative image was designated as the default image and in neutral-to-positive trials, the neutral image was designated as the default image.

After the 1-sec initial presentation of the default image, participants were presented with a 3-sec binary choice screen (without the image). In negative-to-neutral trials, the choice screen read "Press 's' to switch to a Neutral Image or Press 'c' to view Default Image." In neutral-to-positive trials, the choice screen read "Press 's' to switch to a Positive Image or Press 'c' to view Default Image." If no response was recorded, participants were shown the default image. Else, the chosen image was displayed for 15-sec. Attention to the choice was mandatory: Participants were instructed that they were required to make a choice in each trial.

To avoid perceptions of experimenter preferences in favor or against viewing the default image, participants were falsely told that experimenters were interested in measuring their autonomic responses to viewing any of the images included in the experiment (no such data were collected, although participants were hooked up to autonomic assessment devices). Post-experiment interviews suggested that 100% of participants believed this cover story and acted accordingly.

4.1.1.2 Results and Discussion

Participants responded in 100% of trials and elected to view the higher valenced image in 87.5% of trials (the higher valence image was selected, on average, in 36.4 out of 40 trials, 95% CI 34.9 – 37.8).

Across all groups there were no observed differences between default-image viewing for negative-to-neutral and neutral to positive trials suggesting that preferences for higher valenced images and attention effects generalize across the two different types of trials.

4.1.2 Attention Drives Actions Associated with Positively Valued Outcomes (Study 4.1b)

We sought to recreate the laboratory equivalent of a behavioral puzzle in which participants would continue viewing a lower-valenced image even though they had the option to view a higher valenced image with proactive action (i.e. electing to pressing a button). We tested whether increasing levels of attention would lead to increased proactive action.

4.1.2.1 Method

Fifty participants were randomly divided into two equal groups: a low-attention group (14 women, 11 men) and a high attention group (14 women, 11 men). Prior to the start of the experiment all images were sequentially displayed (500 ms/image) so that the participants knew the type of images they could expect in the positive, neutral, and negative category.

In the low attention group, participants were shown a default image for 1-sec. In negative-to-neutral trials, this default image was negatively valenced and the instruction caption under the initial negative image read "Press 's' to switch to a Neutral Image." In neutral-to-positive trials, this default image was neutrally valenced and the instruction caption under the initial neutral image read "Press 's' to switch to a Positive Image." Each trial lasted 15 sec. If a participant elected not to press 's' she would see the default image for the entire trial. Else, if a participant elected to press 's' at time t, the image would instantly switch, and the participant would view the higher valenced image for 15-t seconds.

An identical protocol was used for the high attention group with one important exception: if the participant had not switched in the first 5s of viewing the default image, a red border appeared around the caption under the default image (see Figure 4.2). If the participant had still not switched within 10s of watching the default image the caption flashed for 0.5s. Both these manipulations were designed to orient participant attention towards the caption that reminded them that they could view a higher-valenced image by pressing 's'.

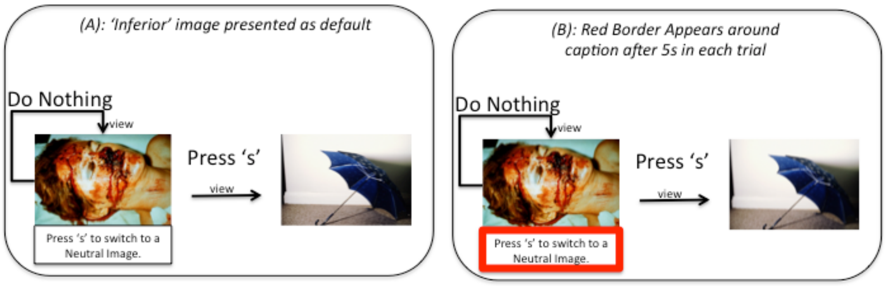

Figure 4.2, Study 4.1 Methods: As depicted in Panel A, participants frequently neglected to switch to the higher valenced image. In the high attention condition (Panel B), participants more frequently switched images after their attention was oriented towards the red-bordered caption.

As is Study 4.1a, participants were falsely told that experimenters were interested in their autonomic responses to image viewing and were indifferent to which specific images they viewed. Post-experiment interviews regarding beliefs about the purpose of the study suggested that 100% of the participants in both groups believed this cover story and acted accordingly.

4.1.2.2 Results and Discussion

In the low attention group, participants switched images in only 29.4% of trials (mean number of switches 11 out of 40, 95% CI 8.0 – 15.5). This low number was noteworthy since actions based on preference alone should have led to a switch in nearly every trial. These results suggested that we had successfully recreated a laboratory equivalent of the behavioral puzzles discussed above.

We had hypothesized that increasing attention would lead to increased levels of motivated behavior more consistent with image valuations (although not to the level of Study 4.1a, since proactive action away from a default was required). We found this to be the case: in the high attention group participants switched images in 50.3% of trials (mean number of switches 20.1 out of 40, 95% CI 15.6 – 24.5). The difference in the rate of switching from the default between the low attention and the high conditions is significant (*t*(23) = 2.80, p = 0.007, d = 0.79).

Notably, the rate of switching in the first 5-seconds (pre-red border) of the high attention group was undistinguishable from the low attention group: high attention group participants switched images for 16.9% (6.76 out of 40) trials within the first 5 seconds compared to 18.5% (7.4 out of 40) switches in the low-attention group within the first 5 seconds (*t*(23) = 0.39, p = 0.70). However, a large difference was observed in the second 5-second interval: participants in the high attention group switched images in an *additional* 26.5% of trials, whereas the low-attention group switched in an additional 8.5% of trials. The difference in the rate of switching in the second 5-second interval is significant (*t*(23) = 4.18, p < 0.001, d = 1.18). This (second) 5-second interval accounted for the bulk of the difference between the two groups.

Across all groups there were no observed differences between default-image viewing for negative-to-neutral and neutral to positive trials suggesting that preferences for higher valenced images and attention effects generalize across the two different types of trials.

These results are consistent with the hypothesis that orienting attention (recruited in this empirical context by the red-border) unlocks the valuation processes that lead to motivated action (i.e. a button press to switch images). However, it is also possible that the salient red border created a demand effect by making participants suspect that the experimenters *wanted* them to press a button and switch images (this could have occurred despite our attempts at creating a – reportedly credible – cover story about being interested in autonomic responses, not image-viewing behavior). We investigated this possibility in Study 4.1c.

4.1.3 Attention Does Not Drive Actions Associated with Negatively Valued Outcomes (Study 4.1c)

The goal of this study was to determine whether the image-switching behavior in the high-attention group of Study 4.1b was attributable to unlocking of the valuation process by orienting attention, or to experimenter-generated demand effects. To achieve this goal, we created trials in which we expected outcomes based on valuation to be different from outcomes based on demand effects.

As in Study 4.1b, we used trials in which a red-border/flash highlighted the option of pressing a button to switch from one image to another. As in Studies 4.1a and 4.1b, participants had the option of moving from a worse image (lower valenced) to a better (higher valenced) image. In Study 4.1c, we included trials in which participants had the option to move from a better image to a worse image.

Accounts based on attention-enabled valuation would suggest that participants would not, in general, press a button to move from a better to a worse image since doing so would result in a negatively valued outcome. In contrast, accounts based on demand-effects would suggest that participants would frequently press any highlighted button since experimenters' cues (i.e. displaying a red-border/flash) were giving rise to strong demand effects.

4.1.3.1 Method

Forty participants were randomly divided into two equal groups: a low-attention group (20 participants, 8 women) and a high attention group (20 participants, 7 women). Participants in both groups followed precisely the same procedures as participants in corresponding groups in Study 4.1b: participants in the low-attention group had the option to press a button and switch the default image to view an alternative image; participants in the high attention group additionally saw the red border and caption flash if they did not switch images early in each trial. The crucial difference from Study 4.1b was that in half the trials the default image was higher valence (i.e. more pleasant) than the alternative image.

There were a total of eighty trials: forty worse-to-better trials (50% negative-to-neutral and 50% neutral-to-positive) that were identical to the forty trials of Studies 4.1a and 4.1b, and forty better-to-worse trials (50% neutral-to-negative and 50% positive-to- neutral). No images were repeated between trials. As in Studies 4.1a and 4.1b, participants were falsely informed that experimenters were interested in their autonomic responses to image viewing and were indifferent to which specific images they viewed.

4.1.3.2 Results and Discussion

For both the low-attention group and the high-attention group, the worse-to-better trials replicated the pattern of results from Study 4.1b. The rate of switching for the high attention group was 61.4% (24.55 out of 40 trials) and the rate of switching for the low attention group was 40.2% (24.55 out of 40 trials). The difference in image switching between the high attention and low attention groups (for worse to better trials) was 21.2% ($t(78) = 3.08$, $p < 0.01$), which was virtually identical to the 20.9% difference observed in Study 4.1b.

Table 4.1: *Study 4.1, Average Rate of Image Switching % by Group*		
	Low Attention Group	High Attention Group
Worse-to-Better trials	40.2% (95% CI 33.4 – 46.9)	61.4% (95% CI 51.0 – 71.6)
Better-to-Worse trials	8.4% (95% CI 7.0 – 9.8)	11.2% (95% CI 9.3 – 13.0)

Importantly, for the better to worse picture trials, as shown in Table 4.1, there was no statistical difference between the low attention group and the high attention group (8.4% vs. 11.2%, $t(78) = 1.08$, $p = 0.39$).

Thus, participants did not appear to merely react to the cues and treat them as an indication of experimenter preferences. Rather, their behavior was more consistent with an account that featured the enabling of valuation processes via attention. When attention unlocked a valuation that was positive (e.g. switching from a worse to a better picture), the associated action was often undertaken. Conversely, when attention unlocked a valuation that was negative (e.g. switching from a better to a worse picture), the associated action was often not undertaken.

While the results of Studies 4.1a-4.1c support the hypothesis of attention enabling valuation and motivated action, this behavioral context was contrived and laboratory based. We next sought to determine whether similar attention-related effects were observable in more natural behavioral contexts.

4.2 Varying Levels of Attention Affects Real-World Purchasing Behavior (Study 4.2)

Can an attention-eliciting message – not designed to change valuation – increase approach motivation in a real-world behavioral setting? To answer this question, we placed a colored, attention-attracting sign reading "APPLES" on apple baskets (which themselves were manifestly visible) in company cafeterias. We hypothesized that the sign should increase orienting attention, which would enable valuation process and create increased motivation to purchase apples.

4.2.1 Method

We selected five company cafeterias with nearly identical layouts belonging to an enterprise located in the San Francisco Bay Area. Each cafeteria served several hundred employees every day. The main purpose of these cafeterias was to serve meals (primarily breakfast and lunch). However they also provide fruits, nutrition bars, and other refreshments that were consumed either as stand-alone snacks or as accompaniments to meals.

An apple basket was prominently displayed near the cash register of each cafeteria. Pre-experiment interviews with cafeteria employees (across all 5 cafeterias) suggested that many customers picked up an apple while waiting to pay for their meal. These customers were said to account for most apple sales. A small minority of customers were said to come to the cafeteria specifically to purchase an apple.

We constructed a colored sign on a folded A3 sized paper containing the word "APPLES" in upper case, 300-point Herculaneum font in bright blue ink with a red border. We placed these signs at the back of the apple baskets on alternate days over a two-week experimental period. The signs were placed on Monday, Wednesday and Friday of Week 1 and Tuesday and Thursday of Week 2. No signs were placed on the remaining days. Experimenters recorded the sales of apples at the end of every business day. This was accomplished by subtracting the apples remaining in the basket at the end of the business day from the total number of apples present in the basket at the start of the business day.

4.2.2 Results and Discussion

In the sign-present condition (across five days in five cafeterias) there were a total of 223 apples sold (8.92 apples per cafeteria per day). In the sign-absent condition (across five days in five cafeterias) a total of 99 apples were sold (3.96 apples per cafeteria per day). The difference was significant ($t(23) = 3.93$, $p < 0.001$, $d = 1.11$)). The five-day sign-present total sales were greater than the five-day sign-absent total sales in each of the five cafeterias.

Notably, the effects of the sign were strongest on Day 1 and Day 2 of the experiment and the weakest on Day 9 and Day 10 of the experiment. The average difference between the sign present and the sign absent conditions on these two days (Monday and Tuesday of Week 1) was 8 apples; the average difference on the last two days of the experiment (Thursday and Friday of Week 2) was only 2.4 apples. The average difference monotonically declined for each day-pair. One reason for this pattern of results may be that the novelty of the sign was highest on Day 1 and this novelty attracted maximum orienting attention.

The "APPLES" sign increased sales even though it did not directly seek to influence customers' valuation of the apples the way a sign reading "SWEET APPLES" might have. However this study did not preclude the possibility that it was not attention that enabled valuation processes; rather the presence of the sign may have directly increased valuation – because, for example, customers may have reasoned that the fact that someone went to the trouble of making a sign about the apples must mean that they are good. We investigated the source of effectiveness of similar signs in a different behavioral context in Study 4.3.

4.3 Attention Affects Behavior in a Real-World Health Related Behavioral Context (Study 4.3)

In Study 4.3, we sought to show that increased motivated behavior was due to unlocking of valuation processes by attention (and not because of direct change in the valuation). We used the context of pedestrians making the choice between taking the escalator or stairs at train stations in the San Francisco Bay Area. We tested whether attention-orienting signs could increase stair-climbing rates (Study 4.3a). Finding this to be the case, we tested whether these effects were induced, not by attention, but by a disruption to routine (Study 4.3b). Finally we tested if subtle demand effects could lead to increased stair-climbing (Study 4.3c).

4.3.1 Attention Orienting Sign Increased Rate of Stair Climbing (Study 4.3a)

The choice of whether to take the stairs or escalator is determined by a variety of factors. Prior studies have shown that only about 6% of pedestrians proactively choose to take the stairs (Suri, Sheppes, Leslie & Gross, 2014). Thus, taking the escalator is seen as a default and most pedestrians do not appear to view taking the stairs as an available option. We tested whether signs placed at the stair escalator choice point could increase rates of stair-climbing.

4.3.1.1 Method

Many prior studies have shown that signs highlighting the health benefits of taking the stairs, when placed at the stair-escalator point of choice are effective in increasing the number of pedestrians who elect to take the stairs (Suri, Sheppes, Leslie & Gross, 2014). A common assumption underlying these signs is that they are effective because they highlight the benefits of taking the stairs (e.g. weight loss, heart fitness) thereby directly impacting valuation.

We sought to determine if signs that did *not* refer to benefits of stair-climbing but only drew attention to the available option of taking the stairs could impact behavior. We chose two such signs: a sign that read “Stairs?” and a second sign that read “Stairs or Escalator?” We hypothesized that these signs would induce more pedestrians to take the stairs relative to cases in which no sign was in place. We used an A/B testing methodology (A = sign present; B = no sign) and recorded the percentage of pedestrians who took the stairs. Both signs were displayed on a 22” x 28” placard that was placed on a floor-standing sign stand. The signs used black lettering printed on white poster paper.

The choices of 1,369 pedestrians approaching (ascending) stair/escalator banks outside two train stations in the San Francisco Bay Area during the commute hours of 7am and 10am and 4pm to 6pm were observed and recorded. Measurements were made in two different stations on two consecutive weekdays over a duration of approximately 9 hours. This ensured that pedestrians were unlikely to have seen any sign more than once (this was later confirmed via interviews). Staircases in both stations had approximately 50 steps.

Pedestrians with items larger than a computer bag or a handbag were excluded because these items would influence their choice. We excluded individuals carrying a baby for similar reasons. Additionally, we counted groups of individuals larger than two as one choice, since people in these groups typically went along with the choice of the first pedestrian in the group.

Experimenters were positioned so that they could not be observed by pedestrians at the point of choice. Each experimenter was armed with two counters – one for the stairs and one for the escalator. A pedestrian was counted when she fully ascended the stairs or escalator. Experimenters were instructed to note any instance of a choice being driven by congestion on either the stairs or the escalator. No such instances were observed. One of the two signs was 'on' for a 15-minute interval that was then followed by no sign being present for the next 15-minute interval. A two-minute break between conditions provided experimenters time to place, or remove signs. The break also provided sufficient time to ensure that pedestrians who observed the experimenter handing the sign were not included in the study set. A stopwatch was used to mark 15-minute measurement intervals. Pedestrians in the process of ascending as the 15-minute measurement interval ended were not included.

We approached all pedestrians who had elected to take the stairs when there was a sign in place at the stair-escalator choice point and asked them to participate in a brief interview. People at the bottom of the stairs could not have observed pedestrians being approached after they had finished climbing the stairs. We sought to determine whether they had seen the sign below, and if so, to describe whether the sign had influenced their choice. If they indicated that the sign influenced them, they were asked to explain how (in an open ended form). They were then asked to indicate which item in a pre-written list was closest to their open-ended response. The list contained three items: (1) The sign convinced me that it is better to take the stairs or (2) The sign drew my attention towards considering what is better for me or (3) Other.

4.3.1.2 Results and Discussion

Both signs increased the percentage of pedestrians electing to take the stairs. The sign reading “Stairs?” outperformed the No Sign condition (11.9% vs. 5.8%; $\chi 2=7.09$, $df = 1$, $N = 625$, $p=0.008$, $V = 0.11$). The sign reading “Stairs or Escalator?” also outperformed the No Sign condition (13.2% vs. 6.4%; $\chi 2=9.44$, $df = 1$, $N = 744$, $p=0.002$, $V = 0.11$).

Out of the 91 pedestrians who took the stairs when a sign was present at the stair-escalator choice point (across both signs conditions), 63 provided complete interviews of which 26 either did not see the sign or indicated that the sign played no role in their decision. When asked to explain the source of the sign’s effectiveness, the remainder of the participants (37) provided open responses which were later coded by an experimenter who was blind to the hypothesis into the three items described in the Methods section above (i.e. ‘sign increased valuation’ or ‘sign attracted attention’ or ‘other’). A large majority of the open responses – 31 out of 37 (84%) – were coded as the ‘sign attracted attention’ item. After their open response when participants were asked to pick an item that most closely matched their open response, 33 out of 37 (89%) respondents self-coded their response as the ‘sign attracted attention’ item.

The increased rate of stair climbing after reading an attention-eliciting sign suggests that attention increases the impact of motivational forces on behavior. Results from participant interviews suggested that this increase is created because orienting attention enabled the valuation process to be translated into behavior.

However, there are two other possible explanations for pedestrian choices observed in Study 4.3a. First, choices may have been due to a disruption of typical routines due to the sign. Since the dominant majority of pedestrians routinely take the escalator, *any* disruption could have increased the rate of the stair climbing. We considered this possibility in Study 4.3b. Second, pedestrian choices could have been influenced by subtle demand effects that they did not (or could not) articulate in post-choice interviews. Despite the seeming neutrality of the signs used in Study 4.3a, they both prominently featured the word 'stairs'. This may have introduced a communicative intention that implied a preference for stair-taking by the creator of the sign (hence influencing valuation). We tested this possibility in Study 4.3c.

4.3.2 A Disruption in Routine Does Not Influence Stair/Escalator Choices (Study 4.3b)

In Study 4.3b, we sought to determine whether any disruption in typical routines (that almost always feature escalator-taking) could increase stair-taking behavior. We therefore constructed a sign that read "Have a good day!" We reasoned that such a sign would cause a disruption in routines like the signs used in Study 4.3a. However this sign would not draw attention to the stair-escalator decision and therefore would not unlock valuation processes related to stair-climbing.

A disruption-based account would predict that "Have a good day!" sign should increase the rate of stair taking. The attention-enabled valuation account proposed in this work would predict that there should be no increase in stair-climbing. We tested these contrasting predictions using methods identical to those described in Study 4.3a (contrasting a 'sign-on' condition with a no-sign condition).

The 'Have a good-day!' sign had no effect on stair-climbing rates. In the presence of the sign 5.1% of 638 pedestrians took the stairs and in its absence (a statistically equivalent) 5.3% of pedestrians took the stairs ($\chi2=0.01$, $df = 1$, $N = 638$, $p=0.92$). These results were consistent with the attention-valuation hypothesis, but not with the disruption hypothesis.

4.3.3 A Sign Not Featuring Stair-use Increased Stair Climbing Rates (Study 4.3c)

In Study 4.3c, we tested whether the increased stair-climbing rates observed in Study 4.3a were driven by the word 'stairs' that prominently featured in both signs that were tested. It is possible that the word 'stairs' introduced subtle demand characteristics that participants did not/could not report on in post-choice interviews in Study 4.3a. We therefore constructed a sign that read "Escalator?" We reasoned that such a sign would not directly create demand characteristics favoring stair climbing, but would draw attention to the stair-escalator decision and therefore would unlock valuation processes related to stair-climbing.

A demand-characteristic account would predict that "Escalator?" sign would not implicitly prime stair use and would therefore not increase the rate of stair taking. The attention-valuation account proposed in this work would predict that there should be an increase in stair-climbing since the "Escalator?" sign would draw attention to the stair-escalator choice. We tested these contrasting predictions using methods identical to those described in Study 4.3a (contrasting a 'sign-on' condition with a no-sign condition).

The ‘Escalator?’ sign increased stair-climbing rates. In the presence of the sign 9.8% of 719 pedestrians took the stairs and in its absence 5.8% of pedestrians took the stairs ($\chi 2=4.01$, df = 1, N = 719, p=0.04). These results are consistent with the attention-valuation hypothesis, but not with the demand-characteristics hypothesis.

4.4 General Discussion

It is difficult to understand why people often don’t do what is apparently in their best interest. Examples abound, such as patients not taking medications crucial to their well-being, and employees not signing up for retirement accounts crucial to their financial well-being. In this article, we have presented evidence that one cause of such behaviors may be a lack of orienting attention, which prevents the initiation of the valuation processes that are required to enable motivated behavior.

In Studies 4.1a-4.1c, participants frequently did not press a button that would have resulted in viewing a higher-valenced image instead of a lower-valenced default image. When their attention was trained towards a caption reminding them of their option to switch images, participants did so at much higher rates. In Study 4.2, company employees increased apple purchases after viewing a sign reading “APPLES” (which was designed to increase attention but not valuation). In Studies 4.3a-4.3c, pedestrians took the stairs at increased rates after they viewed signs reading “Stairs” or “Stairs Or Escalator?” compared to pedestrians who had not viewed these signs. They frequently attributed their behavior to increased attention.

To our knowledge, prior empirical work on motivational puzzles has not explicitly linked attention to motivated behavior. This link enables a common explanation for a broad spectrum of puzzling findings. For example, preferentially displaying healthy items over unhealthy ones in school cafeteria lines (Hanks et al., 2012) increased sales of healthier food items increased by 18% and decreased sales of less healthy food items by 28%. Presumably the students knew that they had access to the previously preferred non-healthy food items. However these items did not, on our account, receive enough attention to be valued and acted upon.

A similar example involved displaying tax-inclusive prices for products subject to sales tax for a three-week period. This tax-inclusive tax display reduced demand by roughly 8 percent relative to control products (Chetty, Looney & Kroft, 2007). Presumably, customers knew that they would have to pay sales tax on their purchases but a lack of orienting attention on that fact allowed them to purchase items that they otherwise would not have. When taxes were included in the display, the higher price was attended to at the time of the purchasing decision and relatively fewer sales occurred.

The present work makes clear contact with recent theories of decision making that feature the crucial role of valuation in choice. These theories point to multiple drivers of valuation (Lee, 2013; Ochsner & Gross, 2014;) that are integrated in the ventromedial prefrontal cortex (vmPFC)/orbitofrontal cortex (OFC)) (Levy & Glimcher, 2011; Rushworth, Kolling, Sallet, & Mars, 2012). This integration enables every-day decision making by allowing the comparison of the values of each available option and transmitting computed preferences to motor systems that give rise to action (Hare, Schulz, Camerer, O'Doherty & Rangel, 2011). Decision theorists have recognized the important role attention plays in the comparison of values of different options (Rolls, 2007; Hare, Malmaud & Rangel, 2011). This study series contributes to this rapidly developing literature by presenting behavioral evidence that creates a through-line from mechanistic lab-based accounts of attention and valuation to real world motivated behavior. Further, it provides a bridge connecting the (often separate) literatures featuring theories of motivation and theories of decision making.

Our findings demonstrate that orienting attention facilitates motivated behavior – both in the laboratory and in real-world behavioral contexts. However, important details of this attention-motivation link require further investigation. For example, we compared proactively taking an action (e.g. buying an apple) with persisting with a default option (e.g. doing nothing). It is unknown whether the valuation of default states is similar to the valuation of states requiring proactive action. Gathering evidence suggests that this may not be the case since leaving a default state may involve inertial costs (Suri, Sheppes, Schwartz & Gross, 2013) that are typically not relevant in contexts requiring a binary choice between two items, neither of which are associated with a default action. Additionally, it remains unknown whether a minimum amount of attention or a minimum duration of attention is required to enable motivated behavior. Furthermore, it is unknown whether a behavior that has been frequently performed before requires the same level of attention as a behavior that has not been performed before. Future studies are required to illuminate these issues. In clinical contexts, future studies are required to investigate whether deficits in orienting attention can help explain motivational deficits in disorders including schizophrenia (Barch 2005; Kring & Barch, 2014) and ADHD (Dovis, Van der Oord, Wiers, & Prins, 2012).

Our findings suggest that interventions aimed at changing behavior (e.g. smoking cessation, exercise initiation) could attempt to generate an increase in orienting attention and not just highlight the direct benefits/costs of the target activity. These results predict, for example, that a medicine bottle that beeped varying tones would decrease levels of medical non-compliance, as would personalized, attention-grabbing reminders from a cell phone. Such interventions may be most useful when behavior modification strategies that aim to change behavior via altering valuations (e.g. by instituting rewards or punishment or by providing more valuation related information) are not adequate. In many contexts, such attention-based interventions may be simple and inexpensive to deploy.

CHAPTER 5

THE ROLE OF ACTION READINESS IN MOTIVATED BEHAVIOR

Note to readers:

This chapter is adapted from a paper under review in the Journal of Experimental Psychology: General:

Suri, G., Sheepes, G. & Gross, J.J. (under review). The role of action readiness in motivated behavior. *Journal of Experimental Psychology: General.*

5.0 Introduction

Why do we do what we do? As outlined in Chapter 1, theories of motivation and decision making suggest that goal-directed behavior is governed by computations of the value of the potential actions relevant in a given context (Graham & Weiner, 1996; Glimcher & Fehr, 2013). Specifically, we are thought to choose actions that have the greatest subjective action value (Kruglanski et al., 2011; Rangel, Camerer & Montague, 2008).

The computation of subjective action value involves an integration of the stimulus value and the action costs. Here, the 'stimulus value' is defined as the difference between the anticipated benefits derived from the stimulus (that is the target of the action) and the inherent costs associated with the stimulus. For example, the stimulus value of a snack is the difference between the expected benefit derived from the snack and the price paid. 'Action costs' are defined as the costs inherent to performing the action that is being valued. They may involve physical or mental effort (Kool, McGuire, Rosen, & Botvinick, 2010). For example, the action cost of purchasing a snack may include the effort of walking to a nearby store. The stimulus value and action costs are integrated into action values. This concept has been expressed as follows in the prior literature (Rangel & Hare, 2010, also see Chapter 1):

Action Value = Stimulus Value - Action Costs

where, *Stimulus Value = Stimulus Benefits – Stimulus Costs*

Despite the equal weight given to stimulus value and action costs in Equation 1.1, most research in motivation and decision making has focused on the former and not the latter. Two factors have contributed to this imbalance in focus. First, in many commonly encountered behavioral contexts, action costs appear to be "negligible" relative to stimulus value. Specifically, action costs often do not require significant physical or mental effort (e.g. a pressing a button on a vending machine to obtain a snack). Second, in many behavioral contexts, action costs appear to be "identical" for all options under consideration (Rangel & Clithero, 2013, p. 126). Action costs may be identical for all options relevant to a behavioral context if they all require the same level of physical and/or mental effort (e.g. walking to an ice-cream store and choosing between two flavors of ice-cream).

In this work, we propose that action costs that objectively appear negligible may be consequential, and that action costs that objectively appear identical may differently influence Action Costs (see Equation 5.1). On our account, these effects occur because action costs are not fixed and unchanging; rather, they are influenced by a person's readiness to perform that action. We define *action readiness* as the ease with which an action may be initiated given the pre-action-launch state of the individual. An action that has been frequently or recently performed or rehearsed has a high level of action readiness. Correspondingly, actions that have not been frequently or recently performed or rehearsed have a low level of action readiness.

On our account, if action readiness levels are low, even action costs that appear to be negligible can strongly affect behavioral outcomes. Conversely, if action readiness levels are high, those same action costs can become irrelevant to the action valuation calculus (because they do not impact subjective perception of action costs).

Furthermore, options that appear to have identical action costs may have different levels of action readiness, and therefore differently affect behavior.

Constructs potentially related to action readiness have been tested in controlled laboratory environments – although not by manipulating action readiness and not in contexts that have included stimulus values or action costs. For example, previously encountered stimuli (e.g. words, faces, objects) have been shown to elicit increased accuracy and increased speed of response during retrieval compared to stimuli that have not been encountered before (Forster & Davis, 1984). Electrophysiological and fMRI findings suggest that such improvements are driven by "tuning" or "sharpening" of the residual representation of the repeated stimulus (Bargh, 2006; Wagner & Koutstaal, 2002). However these repetition-priming effects were observed in the context of multiple exposures to the same stimulus – and not due to repetition of the same action (which might have increased action readiness). Further, in these studies, participants were not required to make trial by trial value judgments – rather they were required to complete a single action (per trial) that involved perceiving, identifying, and/or categorizing stimuli as quickly as they could. It thus remains unknown whether action readiness influences goal-directed behavior that involves (the putatively more powerful) stimulus valuations.

In the present work, our aim was to determine whether it is possible for action readiness to influence actions when stimulus values are clearly present and action costs are apparently negligible. Concretely, we sought to determine whether a lack of action readiness (with respect to a specific action) could reduce completion rates of that action – even though that action was known to have a positive stimulus value.

Further, we sought to test whether holding stimulus values constant, but increasing action readiness (thereby reducing action costs), could increase completion rates of those actions.

To meet these goals, we created a decision context in which study participants had two action options available to them in every trial. Each action option had a clearly defined stimulus value and action costs. In this context, participants frequently failed to take highly valued actions (with positive Stimulus Value and apparently negligible Action Costs), which, according to motivational accounts, they should almost always have completed (Study 5.1). We next (Study 5.2) created a computational model to test whether action readiness could explain participant behavior in Study 5.1. The computational model suggested that frequent prior action and recent action increased the subsequent likelihood of the occurrence of that action (supporting the hypothesis that whether or not an action is completed is, in part, dependent on its action readiness). We next tested the predictions and the generalizability of the computational model using new stimuli and participants (that were not used in developing the computational model) (Study 5.3). Finally, the computational model suggested that *mandating* action early in the experiment should increase action readiness levels throughout experiment – thereby resulting in higher levels of action completion (even though there were no apparent changes to Stimulus Value and Action Costs). We found this to be the case; further, these increased levels of action completion were accurately predicted by the computational model (Study 5.4).

5.1 Creating a Behavioral Context in Which Action Readiness Effects Are Evident (Study 5.1)

To investigate the role of action readiness in decision making, we sought to create a context in which stimulus values and action costs were well understood, and could be independently manipulated. Further, we sought to measure participants' decisions over a series of trials – this would allow us to determine whether frequent actions and recent actions of a particular type make subsequent action of that type more likely.

One context that seemed promising given our goals was choosing between affective images (Lang, Bradley & Cuthbert, 1999). According to a standard hedonic account (Higgins, 1998), the stimulus value of a higher-valenced (more pleasant or less negative) image is reliably greater than the stimulus value of a lower-valenced image.

We measured participant behavior in two separate contexts. In the first context (required choice), we required participants to press one button to view a higher-valenced image, and another button to view a lower-valenced image. In this case, since the action costs for viewing the two images were equal, stimulus values would determine participant behavior. We therefore hypothesized that barring error or idiosyncratic preferences, participants would always press the button associated with the higher-valenced image.

In the second context (proactive choice), participants were presented a lower-valenced image and had the option to proactively press a button to view a higher-valenced image. The stimulus value of viewing a higher valenced image was thus pitted against the seemingly negligible cost of a button press. In this context, most theories of motivation and decision making would predict very frequent button presses nearly on par with the required choice context. However, based on our analysis of the role of action readiness, we hypothesized that participants -- who had low levels of action readiness for completing this action -- would choose to switch images much less frequently than they did in the required choice context.

5.1.1 Method

In Study 5.1, participants were randomly assigned to one of two groups, the required-choice group and the proactive-choice group. The purpose of the required-choice group was to determine participant preferences in a decision context in which only stimulus values (but not action costs) were relevant. The purpose of the proactive-choice group was to determine participant preferences in a context in which both stimulus values and action costs (including the potential effects of action readiness) were relevant.

In the required-choice group, in each trial, participants were required to either press 's' to switch away from a lower-valenced default image to view a higher-valenced image, or to press 'c' to continue viewing a default image. In this context all participants were asked to make a choice (i.e. press a button) in every trial. The action costs for both options were equal.

In the proactive-choice group, in each trial, participants continued to view the (inferior) default unless they proactively acted (by pressing the 's' key) to switch away from the default to view a higher-valenced image. In this context, participants had the option to act or to do nothing. No action costs were incurred if a participant elected to continue viewing the default image; the physical/mental effort associated with a button press was the action cost associated with viewing the higher-valenced image.

In order to maximize the effect of stimulus valuations, every effort was made to make the positive and negative stimuli as differentiated from neutral stimuli as possible. For negative stimuli, we chose images depicting intensely negative scenes that are reliably known to elicit avoidance behavior (Lang, 2010). For positive stimuli, we chose erotic images of the type reliably known to elicit approach behavior among heterosexual males (Knutson, Wimmer, Kuhnen, Winkielman, 2008).

Eighty participants were randomly assigned to either the proactive-choice condition (50 participants) or the required-choice condition (30 participants). All participants were heterosexual males between the ages of 18 and 50 who, in pre-experiment questionnaires, indicated that they enjoyed viewing erotic images. The sample-size in Study 5.1 and all subsequent studies was based on effect sizes observed in pilot studies. We pre-determined sample sizes that would give us adequate power in order to test our hypothesis, and stopped data collection when we had obtained this predetermined number of participants.

There were forty total trials (per participant) with an equal number – twenty – of two types of trials. The negative-to-neutral trials allowed the selection of a neutral image instead of a default negative image and the neutral-to-positive trials allowed the selection of a positive image over a default neutral image. Prior to the start of the experiment all images were sequentially displayed (500 ms/image) so that the participants knew the type of images they could expect in the positive, neutral, and negative category.

In the required-choice condition, participants were shown a default image for 1-sec. In negative-to-neutral trials, this default image was negatively valenced; in neutral-to-positive trials, this default image was neutrally valenced. After the 1-sec initial presentation, participants were presented with a 3-sec choice screen (without the image). In negative-to-neutral trials, the choice screen read "Press 's' to switch to a Neutral Image or Press 'c' to view Default Image." In neutral-to-positive trials, the choice screen read "Press 's' to switch to a Positive Image or Press 'c' to view Default Image." The choice screen lasted for 3-sec. If no response was recorded, participants were shown the default image. Else, the chosen image was displayed for 15-sec. Participants were instructed that they were required to make a choice in each trial.

In the proactive-choice condition, participants were shown a default image for 1-sec. In negative-to-neutral trials, this default image was negatively valenced and the instruction caption under the initial negative image read "Press 's' to switch to a Neutral Image." In neutral-to-positive trials, this default image was neutrally valenced and the instruction caption under the initial neutral image read "Press 's' to switch to a Positive Image." Each trial lasted 15 sec. If a participant elected not to press 's' she would see the default image for the entire trial. Else, if a participant elected to press 's' at time t, the image would instantly switch, and the participant would view the higher valenced image for 15-t sec (Figure 5.1).

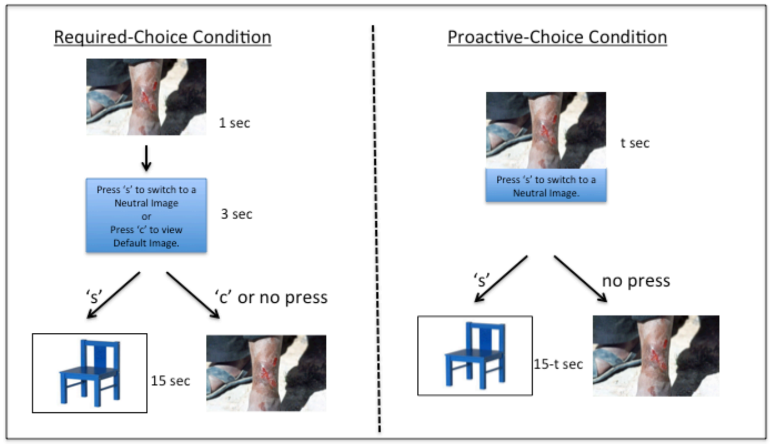

Figure 5.1, Study 5.1 Methods: Decision context for the Required-choice condition (left) and the Proactive-choice condition (right).

Every effort was made to ensure that there were no additional hidden stimulus valuations at work. For example, to avoid perceptions of experimenter preferences in favor of or against switching, participants were falsely told that experimenters were equally interested in measuring their autonomic responses to viewing any of the images included in the experiment (no such data were collected). Post-experiment interviews suggested that 100% of participants believed this cover story and acted accordingly. Additionally, we carefully examined the experimental area to remove potential (implicit) triggers that could increase the valuation of action or inaction (Hassin, Aarts, Eitam, Custers, & Kleiman, 2009).

5.1.2 Results and Discussion

Participants in the required-choice condition chose to switch the inferior image in 84% of the trials (mean number of switches 33.5 out of 40, 95% CI 29.5 – 37.5). Participants in the proactive-choice condition chose to switch the inferior image in 45% of the trials (mean number of switches 18.0 out of 40, 95% CI 14.9 – 21.2). The difference in the rate of switching from the default between the proactive-choice and the required-choice conditions is significant ($t(78) = 6.04$, $p < 0.001$, $d = 1.36$). There was no interaction between condition and trial type (negative-to-neutral versus neutral-to-positive trials), suggesting that these condition effects generalize across the two different types of trials.

Factors often used to explain inferior outcome preferences (Dinner, Johnson, Goldstein & Liu, 2011) – implied recommendations and loss aversion – were not applicable in this behavioral context. In post-experiment debriefings, all participants stated that they believed that the purpose of the experiment was to measure their physiological responses upon viewing different images and that the experimenters were indifferent to their viewing choices (thus implied recommendations were not applicable). Leaving the initially presented image always resulted in superior images (thus loss aversion was not a factor). Thus, traditional valuation variables did not appear to explain participant behavior in the proactive group of Study 5.1.

There were two alternative explanations for the seemingly puzzling participant behavior in the proactive choice group. First, it is possible that negligible as they seem to be, the action cost of the button press was often equal to (or exceeded) the stimulus value of the higher-valenced image. Alternatively, it is possible that action-readiness for button presses varied throughout the course of the experiment and in some cases the lack of action readiness increased the action cost of button pressing, frequently making it more than the stimulus value of the higher valenced image. We sought to test these two alternatives in Study 5.2.

5.2 Developing a Computational Model (Study 5.2)

If the action cost of a button press frequently exceeds the stimulus value of viewing a higher-valenced image, then barring noise, participant behavior on each trial should be independent of behavior in prior trials. Alternatively, if action readiness is influencing participant behavior, then the likelihood of button pressing should be influenced by what the participant did in previous trials. In particular, more frequent and more recent button presses should increase action readiness, thereby decreasing action costs (and increasing action values), with the effect that button presses should be more likely. In Study 5.2 we sought to test whether the two hypothesized drivers of action readiness – frequency and recency – could be used to predict participant action and inaction in the proactive-choice group of Study 5.1.

5.2.1 Method

We created a computational model to predict p(t), the probability of a button press at trial t, using two predictor variables – F(t) and R(t). F(t), a frequency variable, measured the *total* number of prior button presses (relative to non-presses) until trial t. R(t), a recency variable, measured the contribution of recent button presses, that is, those that took place just prior to trial t (Figure 5.2). We reasoned that if such a model fit the observed data well, confirmed the statistical significance of both theorized terms (F(t) and R(t)), predicted participant behavior well above chance, and made testable predictions that were empirically confirmed, then the case for the influence of action readiness would be strengthened.

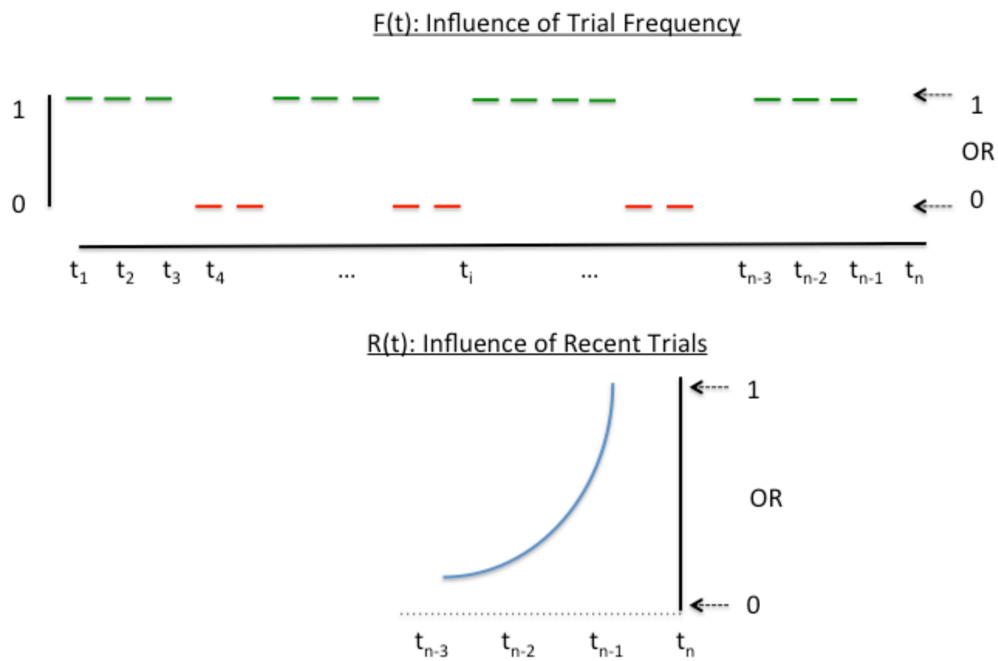

Figure 5.2, Action Readiness Variables: Two factors influence action readiness levels: frequency and recency. The frequency factor measures the influence of actions and inactions in all trials prior to t_n. In this illustrative example the trials in which an action occurred (top row of dashed lines), outnumber the trials is which the action did not occur (bottom row of dashed lines). The recency factor measures the influence of trials that immediately precede t_n. The influence of the trial immediately preceding t (i.e. t_{n-1}) is the highest; it decays exponentially for prior trials.

F(t) represents participant behavior in all prior trials (prior to t) and is the difference between button presses ('1') and non-button presses ('0') before trial t. For each button press, F(t) was increased by 1, and for a non-press, it was decreased by 1 (to model that non-presses created action readiness for non-presses, not just a lack of readiness for presses). To mark that the first press is especially important with respect to initiating executing-readiness, it was given twice the weight (=2) as other button presses (=1) (in pilot data there were twice as many participants with zero button presses than with any other number of presses). Thus for the first seven trials represented by the string 1110011, F(8) = 2+1+1-1-1+1+1 = 4. For the seven trials represented by the string 0000011, F(8) = -1-1-1-1-1+2+1 = -2.

R(t) captures the influence of three immediately prior trails and is equal to $i_{t-1}e^{-1} + i_{t-2}e^{-2} + i_{t-3}e^{-3}$ where i_{t-1}, i_{t-2} and i_{t-3} are equal to 1 if the t-1st, t-2nd and t-3rd trial (respectively) had a button press, else they equal 0. Here 'e' represents the natural exponential. For example, if the three trials prior to trial t were all 1s (i.e. button presses), then $R(t) = e^{-1} + e^{-2} + e^{-3}$. On the other hand if the three trials prior to trial t were all 0s (non-button presses) then R(t) = 0. The logic behind this formulation of R(t) is that the influence of a button press decays exponentially.

We then used the following logistic regression:

$$\ln[p(t)/(1-p(t))] = \alpha F(t) + \beta R(t) + e$$

Here ln[p(t)/(1-p(t))] is the logit of the probability (log odds ratio) that a button press occurs at trial t, and α , β and e are constants.

5.2.3 Results and Discussion

Both F(t) and R(t) were significant in predicting presses/non-presses (McFadden $R^2 = 0.26$ representing an "excellent fit" in the context of a logistic regression (McFadden, 1973)). Overall, the model correctly predicted participant choice in 75.1% of trials (well above chance). Models using both F(t) and R(t) were superior to models that used F(t) alone or R(t) alone (Deviance = -6.9, $p<0.01$). The results of the logistic regression are summarized in Table 5.1.

Table 5.1 **Study 5.1 logistic regression**

	Estimate	Standard Error	z-	Pr(>|z|)

			Value	
Intercept	-0.26	0.08	-3.0	0.002
F(t)	0.10	0.006	15.86	< 2e-16
R(t)	0.74	0.28	2.64	0.008

The predictive power of the variables in the computational model suggested two testable predictions related to action readiness. First, the term R(t) implied that participant behavior should be 'clumpy' – i.e. button presses and non-presses should tend to occur together. Second, since action readiness patterns shaped in the early trials should cascade to later trials, participant choices in very early trials (i.e. the first two trials) should predict participant behavior in subsequent trials.

To test the clumpiness prediction, we represented a button press in a trial by 1 and a non-press by 0. The string 1100001111000011100 has more 'clumpiness' than 1101001011010010100, though they both have the same number of 1s and 0s. We used the total number of switches (1 to 0 or 0 to 1) to create a metric c, for clumpiness (defined as the total number of switches divided by the total possible switches; lower numbers represent greater clumpiness). For example, c(101011) = 4/5=0.8 and c(111100) is 1/5 = 0.2.

To measure the clumpiness of a randomly generated binary string in which the probability of a 1 is 45% (equal to the overall probability of a button press in the proactive-choice group of Study 5.1) we used the bootstrapping method. We randomly generated randomly 10,000 such strings and computed the average clumpiness scores of these string to be 0.49. The clumpiness scores of Study 5.1 proactive-choice group strings was 0.29 – a much higher level than would occur if 1s were randomly distributed ($p < 0.01$).

To test the early trials prediction we measured the downstream trial-switches of participants had elected not to switch images in either of the first two trials ('00' participants) and compared this to participants who had pressed the button on both the first two trials ('11' participants) and participants who had pressed the button in exactly one of the first two trials ('10' or '01' participants). As predicted by the model, the total button presses in proactive-choice group of Study 5.1 were a function of the action readiness produced in the first two trials (i.e. 11 (74%)>01 (52%) ≈10 (51%) >00(28%), $F(3, 46) = 9.66$. $p < 0.01$).

Both the clumpiness and the early-trial property provide evidence in support of the hypothesis that action readiness influenced participant behavior in the proactive-choice group of Study 5.1.

5.3 Generalizability of Our Computational Model (Study 5.3)

We next sought to test the generalizability of the computational model derived in Study 5.2 by using stimuli and participants that were different from those used to create the model. This is a necessary step since while Study 5.2 provided support for the influence of action readiness, it did not prove the general validity of the model derived in Study 5.2. To do this, it is necessary to test the predictions of the model in the context of a new data set.

In addition to providing general evidence for the computational model, we sought to address an important limitation of Study 5.1. Specifically, in an attempt to make positive stimuli as motivationally salient as possible, we had limited our stimuli to erotic images and our participants to heterosexual males who acknowledged that they enjoyed viewed erotic images. We had derived our computational model for this particular demographic and stimulus set. However, we reasoned that since the computational model was derived purely using action readiness related features, it should also predict responses for a sample that included males and females and used a different set of stimuli.

5.3.1 Method

Ninety-three participants (fifty-eight females) between the ages of 18 and 50 were asked to complete procedures identical to those used for the proactive-choice group in Study 5.1. However, the stimuli used in Study 5.3 were different from those used in Study 5.1. Unlike in Study 5.1, the positive images depicted aesthetically pleasing scenes of nature.

As in Study 5.1, participants were (mis)informed that experimenters wished to measure their affective responses to images and were indifferent to whether they switched or not. A finger pulse monitor was attached to the non-dominant hand of each participant. As in previous studies, post-experiment interviews suggested that 100% of participants believed this cover story and acted accordingly.

5.3.2 Results and Discussion

Participants switched images in 42.2% of trials (mean number of switches 16.9, 95% CI 14.3 – 19.4). This was statistically equivalent to the rate observed in the proactive-choice group in Study 5.1. The computational model correctly predicted participant choices in 71.2% of trials (similar to 75.1% for Study 5.1 participants).

5.4 Action Initiation or Rehearsal Increases Action Readiness (Study 5.4)

In Study 5.4, we tested a crucial implication of our computational model, namely that *mandating* action (or action rehearsal) early in the experiment should increase action readiness levels throughout experiment – thereby resulting in higher levels of proactive action.

5.4.1 Method

In Study 5.1 (proactive-choice group), 38% of the participants had elected not to switch images in either of the first two trials ('00' participants). Only 14% of participants had elected to switch in both trials ('11' participants). The rest of the participants switched images in exactly one of the first two trials.

Using the computational model created in Study 5.2, we calculated that if all the participants were *required* to switch images in exactly one of the first two trials, then the total number of switches (across all participants, across all trials) would increase to 57% (compared to 45% in Study 5.1). This would occur because the large number of '00' participants would become '01' or '10' participants. Having an early button press would have cascading effects resulting in a higher level of button presses.

To calculate the prediction for this higher level, we used the computational model of Study 5.2 and assumed that 50% of participants would start with press/no-press ('10') and 50% of participants would start with no-press/press ('01') in the first two trials. We calculated F(t) and R(t) after the first two trials and used these values to calculate the probability of a button press at the next trial (i.e. the first non-mandatory trial). We used the computed probability (say P for a given trial) and randomly picked a probability, r, from a normal probability curve centered at P. If r>50% we assumed a button press, otherwise we assumed a non-button press. We then calculated the button press probability for the next trial until we generated a press/no-press profile for all 40 trials. Using bootstrapping we calculated that the average number of button presses (after the mandatory trials) should be 57%.

This prediction is at odds with accounts prior motivational and decision making accounts and consistent with accounts featuring action readiness. If only stimulus values and action costs were operational, then required 'pre-experiment trials' should not affect action values in later trials.

In Study 5.4, we selected three levels of action readiness: 1) one-trial practice which involved physically performing the action required to overcome a default; 2) one-trial practice which involved mentally rehearsing the action required to overcome a default; and 3) no practice or rehearsal. We hypothesized that physically performing the action would cause greater action readiness (and therefore greater proactive action in switching away from the default) than mental rehearsal, which in turn would cause greater action readiness than no practice.

Sixty participants (selected using the same criteria as in Study 5.1) were randomly assigned to one of three conditions: Physical practice, Mental practice, or No practice. Participants in all three conditions were given the proactive-choice instructions described in Study 5.1. Physical practice participants were asked to complete two additional pre-experiment trials. In one, and only one, of these trials they were required to press the 's' key to switch the default image for another image. In the other trial they were required to not press the 's' key. The order of these trials was left up to the participants. Mental practice participants were asked to mentally rehearse pressing the 's' key in one (and only one) of two pre-experiment trials. The were instructed to imagine – as vividly as possible – their hand coming onto the keyboard, and a finger pressing the ‘s’ key. In the other pre-experiment trial, participants were asked to imagine – as vividly as possible – their hand staying in its current spot (and not coming to keyboard and pressing the ‘s’ key). As in the Physical practice group, the order of these two trials was left up to the participants. In the No practice condition participants were allowed to complete the two pre-experiment trials in any way they chose (i.e. they were not required to either press or not press ‘s’).

As in Study 5.1, participants were (mis)informed that experimenters wished to measure their affective responses to images and were indifferent to whether they switched or not. A finger pulse monitor was attached to the non-dominant hand of each participant. As in previous studies, post-experiment interviews suggested that 100% of participants believed this cover story and acted accordingly.

5.4.2 Results and Discussion

Participants in the Physical practice condition pressed ‘s’ in 62.6% (mean number of switches 25.1, 95% CI 19.8 – 30.2) of the trials. Participants in the Mental practice condition pressed ‘s’ in 52.2% (mean number of switches 21.1, 95% CI 15.5 – 26.7) of the trials. Participants in the No practice condition pressed ‘s’ in 32.5% (mean number of switches 12.8, 95% CI 7.7 – 18.0) of the trials (replicating results from the proactive-choice group in Study 5.1, $t(68)=1.5$, $p=0.12$).

The action costs in each of the groups (i.e. the effort of a button press) apparently appeared to be identical in all groups. Yet, consistent with our account, varying levels of action readiness produced different behaviors.

Both conditions in which action readiness was hypothesized to increase (Physical practice and Mental practice) produced significantly greater switching than the non-readiness increasing condition (No practice): Physical practice vs. No practice ($t(38) = 3.33$, $p = 0.002$, $d = 1.02$); Mental practice vs. No practice ($t(38) = 2.19$, $p = 0.03$, $d = 0.66$). The difference between the physical practice and mental practice conditions was not significant ($t(38) = 1.02$, $p = 0.31$) suggesting that the action readiness produced by mental rehearsal was not significantly weaker than the action readiness produced by physical action completion.

The value of 57% predicted by the computational model was within the 95% CI of 50%-76% for the Physical practice group. Participants in the No practice group switched images in 33% of all trials – statistically equivalent to the proactive-choice group of Study 5.1.

5.5 General Discussion

We hypothesized that action readiness –the ease with which a new action can be initiated, given the pre-action-launch state of the individual – influences subjective perceptions of action costs. On our account, holding stimulus valuations constant, a person is more likely to perform a behavior with high levels of action readiness than low levels of action readiness. We further hypothesized that action readiness would increase with the frequency and recency of the performance of that action.

To test these hypotheses, we created a picture-viewing decision context. In Study 5.1, participants persisted with an inferior option (a lower-valence default image) even though their valuation preference – measured in a forced choice context not involving action-readiness factors – was for the non-default image. In Study 5.2, we used a computational model to show that the prior frequency and recency of an action (here, a button press) predicted the probability of occurrence of that action. This provided evidence that action readiness drove the observed behavior in Study 5.1. In Study 5.3, we showed that the computational model predicted behavior for a data-set not used to generate the model. Finally, in Study 5.4, we further confirmed the role of action readiness by examining the effects of requiring participants to act in one of two early trials. As predicted by accounts featuring action readiness, but not by other accounts, the effects of initial actions cascaded throughout the experiment.

We have conceptualized action readiness in terms of increasing or decreasing action costs. Performing an action that has not been frequently or recently performed often requires greater effort than performing actions that have been frequently or recently performed. Such effort may involve cognitive costs (Shenhav, Botvinick & Cohen, 2013), and these costs may become manifest in several behavioral contexts including those we have examined in this work.

A second – but we think less apt – way to conceptualize action readiness is in terms of habit. Habit is defined a pre-existing association, strengthened by long-standing repetition, between cue and action (Neal, Wood & Quinn, 2006). Action readiness, on the other hand, is related to the facilitation of activated action, even when such an action does not result from a long-standing association with a cue. In Study 5.4, we detected action readiness effects after participants pressed a key just once – which could hardly be labeled as a habit. However, we recognize the possibility that action readiness processes may lead to habit formation over time.

Action readiness may provide a new way to analyze behavior in which people persist with an action even though the stimulus value of that action is zero or negative. For example, people have a tendency to repeatedly sit at the same spot in a classroom (even if the seat is not differentiated from other seats) (Costa, 2012). Similarly people continue to snack well past satiation because the food-item remains within easy reach (Cohen & Farley, 2008) as though the act of eating has "momentum" (Mehrabian & Riccioni, 1986). On our account, such behaviors occur because the chosen actions (sitting in the same chair or having another bite) have a high level of action readiness. This enables the initiation of actions associated with stimulus values that are zero or negative.

Action readiness may be a contributing factor in explaining why people persist with inaction when seemingly low-cost actions could have resulted in large gains. For example, patients frequently do not take medicines crucial to their health (Suri, Schwartz, Sheppes & Gross, 2013), employees do not spend a few minutes to start beneficial retirement accounts crucial to their financial future (Beshears, Choi, Laibson, Madrian, 2006), and individuals do not proactively perform simple actions to obtain their preferred options in decision contexts involving organ donation (Johnson & Goldstein, 2003), electric utilities (Hartman, Doane & Woo, 1991), and insurance providers (Johnson, Hershey, Meszaros & Kunreuther, 1993; Samuelson and Zeckhauser, 1988).

Prior analyses have drawn attention to subtle factors that might underlie such behavior, such as implied recommendation and loss aversion (Dinner, Johnson, Goldstein, & Liu, 2011; Kahneman, Knetsch, & Thaler, 1991). However, these factors do not always appear to apply to these behavioral contexts. On our account, these behaviors occur because they require unfamiliar actions with low action readiness. If such actions had been performed even once before, they would be much more likely to be performed again.

Action readiness may also explain why individuals act inconsistently in what appear to be comparable situations. The valuation calculus described in Equation 1.1 suggests that if the valuation of a stimulus exceeds associated action costs, then barring noise, the relevant action should always occur. Similarly, if the valuation of a stimulus is less than the associated action costs, then barring noise, the relevant action should never occur. The present work suggests the intriguing possibility that seeming inconsistencies from the valuation calculus are not products of random noise, but may be attributable to varying levels of readiness to act.

The effects of action readiness can be observed in a wide array of disciplines. For example, the task-switching literature (Rubinstein, Meyer, & Evans, 2001) discriminates between two types of trials: an nth trial is a switch trial if it involves a different task from the n-1st trial and it is a repeat trial if it involves the same task as the n-1st trial. A large body of evidence has demonstrated that across various types of tasks, performance on switch trials is worse than performance trials (Monsell, 2003). While there are several ways to understand this phenomenon, a prominent model suggests that repeated trials are more efficient because of transient carry-over of task-set "activation" from trial to trial (Gilbert & Shallice, 2002). This activation based proposal is consistent with the action readiness account described here. Similarly, a propensity for repeating familiar actions may lead individuals and organizations to stay with existing processes and conduct searches (e.g. for new processes) much less than is optimal (Schotter & Braunstein, 1981), even though these new processes may offer significant efficiencies. Action readiness effects are also abundant in the public policy domain. Many such effects were discussed at length in the influential book Nudge in which Thaler and Sunstein (2008) identified several decisions in which individuals could be nudged to select more optimal options as long as these options were made to be the default options. While several of their examples involved changing underlying stimulus value, some examples leveraged the role of action readiness in financial and health-related domains. Finally, action readiness effects may play an important role in developing effective self-control interventions. Baumeister and Heatherton (1996) propose that the longer a response is repeated, the more difficult it becomes to override. They therefore suggest that self-control attempts are most likely to be successful *before* action-readiness increases the costs of stopping an undesirable action. This implies that self-control interventions should ideally target the earliest manifestation of an undesirable behavior and not be limited to contexts in which a behavior has 'hardened' and assumed troubling proportions. In this study series, we have demonstrated that action readiness effects can be influential even in contexts that include stimulus valuations (Equation 5.2) that suggest contrary actions. However, action readiness may not always appreciably affect behavior. Many behaviors are largely or entirely shaped by stimulus valuations. Future studies must investigate the circumstances in which action readiness influences behavior to a greater or lesser extent.

CHAPTER 6: EXTENSIONS AND FUTURE DIRECTIONS

6.0 Introduction

In the above chapters we have, in 17 studies, motivated and developed the ARM framework. In this chapter we begin by describing extensions and elaborations of the ARM framework. Next, we consider the relationship of the ARM framework to other behavioral constructs including habits, psychological inertia, and implementation instructions. We close by elaborating future research directions related to the ARM framework.

6.1 Extensions and Elaborations of the ARM framework

Equation 1.1 describes the valuation a single action pertaining to a single stimulus. In this sense, it may be thought of as an atomic element of action valuation. However, everyday behaviors often involve the integration of several such action valuations. Here, we consider some of the complexities attendant to such integrations. First, calculating the stimulus value of even a single stimulus often involves the separate valuation of the different attributes of that stimulus. For example, the benefit of eating an orange may involve separate valuations of its taste, texture, and smell (Rangel & Hare, 2010). Second, even simple behaviors often involve the integration of many separate stimuli. Obtaining an orange may involve shopping for other needed items (a different stimulus from the apple). Third, a cost with respect to one stimulus may be perceived as a benefit with respect to another stimulus. For example walking to the store to obtain an apple is a cost with respect to the stimulus of obtaining the apple, but may be perceived as a benefit with respect to the stimulus of obtaining exercise. Finally, there is inherent uncertainty inherent in obtaining a benefit and/or incurring a cost. Equation 1.1 may be updated to reflect the stochastic nature of the valuation calculus:

Action Value = E[Discounted Stimulus Benefit | Action] – E[Discounted Stimulus Cost | Action] – E[Discounted Action Cost |Action], where E[ ] denotes the expectation operator (Equation 6.1)

The probabilistic features of Equation 6.1 are resonant with the expectancy-value theories of motivation (Barron & Hulleman, 2015) that also feature probabilistic benefits and costs. An important difference between the two formulations is that action costs are not separated from stimulus-associated costs in expectancy-value theories. Rather these theories postulate that more than one behavior is possible, the behavior chosen will be the one with the largest combination of expected success and value (Kuhl, & Beckmann, 2012).

The ARM framework may be described using either formulation. We have chosen the valuation calculus approach of Equation 1.1 because of its focus on the 'atomic' base case and because of gathering neural evidence in its favor (discussed below).

6.2 Relationship of the ARM framework to Other Behavioral Constructs

The ARM framework connects to several behavioral constructs. Here, we discuss its connections to habits, psychological inertia, and implementation intentions.

6.2.1 ARM and Habits

A habit is defined a pre-existing association, strengthened by long-standing repetition, between cue and action (Neal, Wood & Quinn, 2006). Action readiness, on the other hand, is the ease with which an action may be initiated given the pre-action-launch state of the individual. This readiness may be increased by prior (and recent) repetition, mentally rehearsing an action, watching someone else do the action or by environmental affordances. Action readiness effects have been observed after participants pressed an unfamiliar key a handful of times – which could hardly be labeled as a habit (Suri, Sheppes, Schwartz & Gross, 2013). We recognize the possibility that sustained action readiness associated with a cue may lead to habit formation over time.

6.2.2 ARM and Psychological Inertia

Prior work has defined Psychological Inertia as the tendency to maintain the status-quo (Gal, 2006). Individuals may have more or less psychological inertia relative to each other. We propose that psychological inertia is an emergent property of action readiness. In particular, a tendency to have high levels of action readiness across many action contexts would result in high inertia. This readiness may apply to actions or to inactions; thus, individuals with high psychological inertia may persist with the same actions in a given context (e.g. ordering the same meal at a restaurant) or with the same inactions (e.g. not taking medicine beneficial to their health).

6.2.3 ARM and Implementation Intentions

Gollwitzer and his colleagues (e.g. Gollwitzer, 1999) have defined implementation intentions as the furnishing of the goal intention with an if–then plan specifying when, where, and how the person will instigate responses that promote goal realization. An implementation intention adds action specificity to goals that may only be represented at a high level. Explicitly formulating implementation intentions has been shown to increase goal-directed behavior in a variety of contexts (Gollwitzer, 1999). We propose that the effectiveness of implementation intentions stems, in part, from an increase endogenous attention toward the goal and in part from an increase in the action readiness of the various sub-steps required to reach a desired goal (via mentally rehearsing the actions they plan to take to achieve their goal). If proven correct, this proposal may provide a potential mechanism underlying implantation intentions.

6.3 Future Directions

We highlight important outstanding questions related to the neural bases of the endogenous attention and action readiness, the influence of endogenous attention on valuation, the nature of action readiness, and the possibility of interaction between endogenous attention and action readiness.

6.3.1 Neural Bases of Endogenous Attention and Action Readiness

Studies have shown that the lack of endogenous attention (and the presence of zoning out) may be associated with activation in the Default Mode Network (DMN) (Anticevic, 2012; Raichle, in press; Schooler et al., 2011). The DMN is a set of brain networks whose activity has been shown to be high when the mind is not engaged in specific behavioral tasks and low during focused attention on the external environment (Smallwood, Brown, Baird & Schooler, 2012). Strong evidence suggests that the DMN enables self-reflective thinking (Qin & Northoff, 2011) and as a result DMN activity may conflict with activity requiring endogenous attention on external events. DMN activity is known to be anti-correlated with an External Attention System (EAS), and with the fronto-parietal-control network (FCPN) – a set of brain regions most typically implicated in executive, top-down cognitive control processes (Fornito, Harrison, Zalesky & Simons, 2012).

Action readiness has been infrequently studied and its neural bases are not directly known. However, some inferences are possible from the repetition priming literature which has revealed that while behavioral performance improves with stimulus repetition, neural activity in humans (BOLD fMRI) as well as single cell firing rates monkey's tend to decrease. This phenomenon is known as repetition suppression (Larsson, J., & Smith, 2012). There are several hypotheses on how a decrease in neural activity produces an increase in behavioral efficiency but gathering consensus (Gotts, Chow, & Martin, 2012) centers around the proposal that as cells are firing at reduced rates, they are firing more synchronously with one another, leading to more efficient neural processing. This suggests that action readiness effects will result in reduced activations, and increased synchrony (measured, for example, by functional fMRI) in regions/networks that were involved with the initiation of that action for the first time (or when its action readiness effects were low).

6.3.2 Attention's influence on Valuation

Many models assume that decision makers compute option values by making several – potentially noisy – estimates of the option value and then integrating these values over time. These models predict that the option value assigned to an item depends on the amount of time spent attending to that option

(Armel, Beaumel, & Rangel, 2008). Subsequent experiments based on these models (Rangel, 2013) showed that, in some cases, appetitive items were more likely to be chosen and aversive items were less likely to be chosen if the time interval of visual attention directed towards those items was increased. The ARM framework proposes that endogenous attention unlocks valuation processes (i.e. enables them to occur) – which is different from the above view. This suggests the intriguing possibility that attention may influence valuation via different routes. Future studies are required to determine whether and how these two routes relate to each other.

6.3.3 The Nature of Action Readiness Effects

Action readiness may have semantic components (i.e. readiness shaped by the outcome/meaning of the action) and/or motor components (i.e. readiness shaped by the action without regard to associated outcomes). We have described evidence in which either semantic or motor effects (or both) may have affected action costs. Future studies are required to determine whether one or both components drive action readiness effects.

6.3.4 Interaction Between Exogenous Attention and Action Readiness

One implication of the ARM framework is that endogenous attention and action readiness may interact to influence the probability of occurrence of a motivated action. For example, at a high level of action readiness, a relatively low level of endogenous attention may be enough to initiate action readiness. The same level of attention may not result in action if levels of action readiness are low. Future studies are required to empirically determine the nature of this potential interaction.

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