

Two Essays on Escalation of Commitment

by

Abhijit Guha

Department of Business Administration
Duke University

Date: _____

Approved:

William Boulding, Co-Supervisor

Richard Staelin, Co-Supervisor

Julie Edell Britton

Wendy Wood

Dissertation submitted in partial fulfillment of
the requirements for the degree of Doctor
of Philosophy in the Department of
Business Administration in the Graduate School
of Duke University

2009

ABSTRACT

Two Essays on Escalation of Commitment

by

Abhijit Guha

Department of Business Administration
Duke University

Date: _____

Approved:

William Boulding, Co-Supervisor

Richard Staelin, Co-Supervisor

Julie Edell Britton

Wendy Wood

An abstract of a dissertation submitted in partial
fulfillment of the requirements for the degree
of Doctor of Philosophy in the Department of
Business Administration in the Graduate School
of Duke University

2009

Copyright by
Abhijit Guha
2009

Abstract

This dissertation focuses on managerial decision making, and specifically explores conditions wherein managers may increase their propensity to escalate commitment towards a failing project. Escalation researchers (e.g. Schmidt and Calantone, 2002) have listed four classes of factors that may impact a manager's propensity to escalate commitment towards a failing project, and have called for research into how exactly these factors impact escalation. In this dissertation, we explore two such factors. The first factor relates to the characteristics of the decision process used by firms to evaluate the project. Here, for example, researchers have looked at whether the manager was also involved in making decisions about the project in a prior period, and Boulding, Morgan and Staelin (1997) have shown that such manager's positive beliefs about the project (formed in a prior period) make a manager more likely to escalate commitment. The second factor relates to project characteristics. Here, for example, researchers have looked at whether or not the project relates to a product that is perceived as new, and Schmidt and Calantone (2002) have shown that managers are more likely to escalate commitment towards a failing project relating to a new product.

The first dissertation essay uses three experiments to examine how a hitherto unexplored characteristic of the decision process might lead to increasing escalation of commitment. Specifically, building off research into the illusion of control, we examine

whether the opportunity to use managerial skill during the decision process makes a manager more willing to escalate commitment towards a failing project. We find that whenever managers act on cues that cause them to think they can use their managerial skill to control some outside factor (even though in reality they cannot), managers overestimate their ability to “control the odds” related to this outside factor. Such beliefs feed forward and lead managers to make suboptimal decisions about the overall project.

The second dissertation essay looks at how project characteristics might make a manager more (or less) likely to escalate commitment towards a failing project. We explore this issue in the hitherto unexplored real options setting. Real options have emerged as an important part of marketing strategy, and have been used to structure new product alliances, value customers etc. We run a controlled experiment and we examine whether differences in option-structure (which is a project characteristic) impact the propensity to make suboptimal option-exercise decisions. We find that managers are more likely to make suboptimal option-exercise decisions in the case of put options (vis. call options), and – as predicted by the endowment effect literature - this increased propensity to make a suboptimal decision is mediated by/ explained by the psychological ownership construct.

Contents

| | |
|---|----|
| Abstract | iv |
| List of Tables | x |
| Acknowledgements | xi |
| 1. Introduction | 1 |
| 1.1 Overview | 1 |
| 1.2 Essay 1 (Controlling the Odds: Sometimes it is Just an Illusion)..... | 1 |
| 1.3 Essay 2 (Real Options: A Gold Mine, or Fool’s Gold?) | 3 |
| 2. Controlling the Odds: Sometimes it is Just an Illusion..... | 5 |
| 2.1 Introduction..... | 5 |
| 2.2 Model Development and Hypotheses..... | 8 |
| 2.2.1 The Illusion of Control..... | 8 |
| 2.2.2 Link between the Illusion of Control and Managerial Conceit..... | 12 |
| 2.2.3 Modeling Managerial Illusion of Control | 14 |
| 2.2.3.1 The Baseline Situation | 14 |
| 2.2.3.2 Effect of Control Cues | 15 |
| 2.3 Overview of Studies..... | 17 |
| 2.3.1 The Stimulus | 17 |
| 2.3.2 Participants..... | 21 |
| 2.4 Study 1 | 21 |
| 2.4.1 Pretest..... | 21 |

| | |
|---|----|
| 2.4.2 Main Study | 22 |
| 2.4.2.1 Independent and Dependent Variables..... | 23 |
| 2.4.2.2 Contrasting the Two Experimental Subgroups | 24 |
| 2.4.2.3 Testing for Differences between John and Peter | 24 |
| 2.4.2.4 Contrasting the Baseline Group Against the Experimental Group | 25 |
| 2.4.2.5 Examining Other Variables | 27 |
| 2.4.3 Discussion..... | 28 |
| 2.5 Study 2 | 31 |
| 2.5.1 Pretest..... | 31 |
| 2.5.2 Main Study | 31 |
| 2.5.2.1 Subgroup 1 of the Baseline Group..... | 31 |
| 2.5.2.2 Subgroup 2 of the Baseline Group..... | 32 |
| 2.5.2.3 Combining Subgroups in the Baseline Group | 33 |
| 2.5.2.4 Contrasting the Baseline Group Against the Experimental Group | 33 |
| 2.5.3 Discussion..... | 35 |
| 2.6 Study 3 | 36 |
| 2.6.1 Main Study | 36 |
| 2.6.1.1 Setting up the Groups..... | 36 |
| 2.6.1.2 Contrasting Experimental Group 1 with Experimental Group 2..... | 37 |
| 2.6.2 Discussion..... | 39 |
| 2.7 Conclusions and Discussion | 40 |
| 2.7.1 Relevance for Theory | 41 |

| | |
|---|----|
| 2.7.1.1 Theory Contribution..... | 41 |
| 2.7.1.2 Avenues for Future Research | 44 |
| 2.7.2 Relevance for Practice | 45 |
| 2.7.2.1 Removing Control Cues from the Decision Environment | 45 |
| 2.7.2.2 Dampening the Impact of Control Cues..... | 47 |
| 2.7.2.3 Other Applications..... | 47 |
| 2.7.3 Is Managerial Illusion of Control, or more generally Managerial Conceit, all Bad? | 48 |
| 2.7.4 Closing Discussion | 49 |
| 3. Real Options: A Gold Mine, or Fool’s Gold?..... | 50 |
| 3.1 Introduction..... | 50 |
| 3.2 Theory Development | 54 |
| 3.3 A Controlled Experiment | 61 |
| 3.3.1 Setting up the Cells | 61 |
| 3.3.2 Stimuli Equivalence | 62 |
| 3.3.3 The Stimulus | 63 |
| 3.3.3.1 Stage 1 – The Option-Purchase Decision..... | 64 |
| 3.3.3.2 Call Option Stimulus (Stage 1 – The Option-Purchase Decision) | 65 |
| 3.3.3.3 Call Option Stimulus (Stage 2 – The Option-Exercise Decision)..... | 66 |
| 3.3.3.4 Put Option Stimulus | 67 |
| 3.3.4 Key Measures..... | 67 |
| 3.3.5 Results | 69 |

| | |
|---|----|
| 3.3.5.1 Hypothesis 1 | 69 |
| 3.3.5.2 Hypothesis 2 | 71 |
| 3.3.5.3 Hypothesis 3 | 72 |
| 3.3.5.4 Increase in Explanatory Power | 75 |
| 3.4 Conclusion and Discussion | 75 |
| 3.4.1 Marketing Strategy and Real Options | 76 |
| 3.4.2 Escalation | 77 |
| 3.4.3 Endowment Effect | 78 |
| 3.4.4 Implications for Practice | 79 |
| 3.4.5 Closing Discussion | 79 |
| References | 81 |
| Biography | 85 |

List of Tables

| | |
|------------------------------------|----|
| Table 1: Overview Of Studies..... | 23 |
| Table 2: Study 1 Results | 26 |
| Table 3: Study 2 Results | 34 |
| Table 4: Study 3 Results. | 38 |
| Table 5: Measures..... | 69 |
| Table 6: Testing Hypothesis 1..... | 70 |
| Table 7: Testing Hypothesis 2..... | 72 |
| Table 8: Testing Hypothesis 3..... | 73 |

Acknowledgements

Many have helped me in this dissertation journey, and more generally speaking, during my PhD journey. William Boulding and Richard Staelin have helped me develop a taste for managerial decision making, taken me through a research project from idea to execution and – more generally speaking – have shaped my research and teaching philosophy. Julie Edell Britton and Wendy Wood patiently listened to me present many versions of my dissertation essays, and their comments have greatly improved the final product. Kurt Carlson introduced me to consumer behavior research, and has helped me develop a second, wholly new research agenda. Kathy Joiner, Kerrie Hillman, Sue Yager and Anita Tapp have helped above and beyond the call of duty (e.g., moral support, coffee, even helped with proofreading!).

In addition, there are various faculty / PhD colleagues / staff (at Duke and elsewhere) who have helped me – professionally and personally - along the way.

I thank every one of those listed above. I could not have done this without you.

1. Introduction

1.1 Overview

This dissertation includes two essays on escalation of commitment. Researchers have defined escalation of commitment as continuation in a failing course of action (Schmidt and Calantone 2002). These researchers then call for future research into the various factors that may influence a manager's propensity to escalate commitment. In this dissertation, we look at two of these factors. Specifically, we examine how (1) certain characteristics of the decision process, and (2) certain characteristics of the project might lead managers to be more likely to escalate commitment to a failing project.

1.2 Essay 1 (*Controlling the Odds: Sometimes it is Just an Illusion*)

Here we explore how a certain characteristic of the decision process might lead to increased escalation. Specifically, we examine if managers overestimate the impact of their managerial skill. If so, then if managers have the opportunity to use their managerial skill in the course of a decision process, then such managers will overestimate the impact of their skill, and hence will be more likely to escalate commitment towards a failing project.

To motivate our examination, we first posit that the success of a marketing project (e.g., the launch of a new product) depends both on factors directly controlled by the firm and on factors outside the firm's control. Yet marketing managers believe they

can use their managerial skill in the course of a decision process to control, or at least minimize, any negative impact of these uncontrollable outside factors. March and Shapira (1987) call this belief “managerial conceit”. In this paper, we relate managerial conceit to the illusion of control (Langer, 1975). We show how the illusion of control may operate in managerial settings, and how such managerial illusion of control might lead to a version of managerial conceit.

We develop, and test, an enriched process model of managerial illusion of control in three laboratory experiments across 250+ participants with managerial experience. We find that, in the course of a decision process, whenever managers act on cues that cause them to think they can use their managerial skill to control some outside factor (even though in reality they cannot), managers overestimate their ability to “control the odds” related to this outside factor. In turn, these overestimates feed forward and lead these managers to make suboptimal decisions about the overall project, i.e. escalate commitment towards a failing project.

This research contributes to the marketing strategy literature by identifying an additional, and new, causal mechanism underlying managerial conceit. It contributes to the illusion of control literature by developing and validating an enriched process model for this phenomenon, and also by answering some open questions in the illusion of control literature. It contributes to marketing practice by first showing how the managerial illusion of control leads to suboptimal marketing decisions, and then by

illustrating (1) organizational systems, and (2) debiasing mechanisms that limit the negative consequences of managerial illusion of control.

1.3 Essay 2 (*Real Options: A Gold Mine, or Fool's Gold?*)

Here we explore how a certain characteristic of the project might lead to increased escalation. Specifically, we examine escalation of commitment in the (hitherto unexplored) context of real options. By way of background, real options are an integral part of marketing strategy. Real options have been used to structure new product alliances, to set up marketing alliances, to value customers and to design R&D strategy. Much of extant research on real options has focused on the upfront structuring of real options. There is much less research into whether or not managers, having bought a real option, make appropriate decisions when the time comes to exercise a previously-purchased real option.

Real options may be structured as either call options or as put options. In the case of call options, the firm does not own the underlying asset, but has the right to buy the asset for a given price in the future. In the case of put options, the firm owns the underlying asset, but has the right to sell the asset for a given price in the future. We hypothesize that managers are more likely to make suboptimal option-exercise decisions in the case of put options (vis. call options). Further, we suggest that this difference in behavior relates to the endowment effect. We note that, in endowment effect examinations (e.g., Shu and Peck 2008), the psychological ownership construct mediates/

explain endowment effect results. Hence we propose that a suitably modified version of the psychological ownership construct will mediate/ explain differences in the propensity to make suboptimal option-exercise decisions. We run a controlled laboratory experiment, using 90+ participants with managerial experience, and we find support for the hypotheses proposed above.

This research contributes to the literatures on (1) marketing strategy, and (2) real options by demonstrating that managers are more likely to make suboptimal option exercise decisions in the case of put options (vis. call options). More important, this research identifies the endowment effect (and particularly, the psychological ownership construct) as the underlying driver of this finding. More broadly, this research also contributes to the endowment effect literature. It shows that endowment effect extends to contingent ownership, and then it validates an enriched process model for this phenomenon. Finally, this research has multi-faceted implications for practice. It has implications for (1) how marketers should structure real options, (2) how marketers should manage real options after the option has been purchased, and (3) how analysts should value real options.

2. Controlling the Odds: Sometimes it is Just an Illusion

Could it be that it's just an illusion putting me in all this confusion?

Borrowed from "Just an Illusion" by Imagination

2.1 Introduction

Most marketing decisions are made in environments where the final outcome is uncertain and not completely determined by factors under the firm's control. For example, the launch of a new product is affected not only by decisions made by marketing managers, but also by factors such as channel acceptance and competitor reactions, neither of which is under the direct control of marketing managers. Similar uncontrollable factors are present in most strategic marketing decisions, e.g., the introduction of a new advertising campaign, or the launch of a new marketing channel. Hence, marketing managers should realize that getting a positive outcome is due not only to their managerial skill, but also to exogenous factors, hereafter referred to as chance. Empirically, however, managers often (1) attribute successes to their own managerial skill (Vosgerau, Anderson and Ross 2008), and (2) ignore the impact of chance. March and Shapira (1987) label such attributions as "managerial conceit" and note that this tendency may lead to suboptimal decisions¹. Camerer and Lovallo (1999) show that such tendencies can lead to excessive entry into a market.

¹ The word "conceit" has different, often very negative, connotations in daily use. However, here we use the term "managerial conceit" since it is a well accepted term in the Management Science literature.

The observation that individuals attribute success to their own personal characteristics is not confined to managers. This tendency has been noted to occur at the individual level in numerous psychology studies (e.g., Heider, 1958; Miller and Ross, 1975; Alba and Hutchinson, 2000). Referred to as self-serving bias, ego-defensive or ego-protective behavior, or ego-biased attributions, this literature provides theories about why individuals believe they can cause some positive outcome to occur. Although there are some differences among these theories, all assume that this belief is an individual level characteristic. March and Shapira (1987) state that managers acquire this individual-level conceit from post-hoc stories of managerial successes that emphasize the role of skill and remove chance from the explanatory narrative. Hence, when making a strategic marketing decision, managers assume they can “manage the odds”, thereby obtaining a favorable outcome from among the wide range of possible (favorable and unfavorable) outcomes.

In contrast to the work described above, which is based on the premise that managerial conceit is an individual-level characteristic, our research centers on an additional, but very different, causal mechanism for managerial conceit. This mechanism is important because it points to an altogether different approach for reducing managerial conceit. In developing this mechanism we borrow from the work on illusion of control (e.g., Langer, 1975; Koehler, Gibbs and Hogarth 1994). Their work suggests that when individuals are provided with an opportunity to use their skill, i.e.

when individuals are provided with *control cues*, they overestimate their ability to “control the odds”. For example, when individuals get to choose a lottery ticket (choice = *control cue*), vis-à-vis those individuals who are merely assigned a lottery ticket, then such individuals are more likely to overvalue such lottery ticket (Langer, 1975).

This suggests two conceptualizations of managerial conceit. The first, which has been explored in prior Management Science research, is that managerial conceit is an individual-level characteristic, which is present in all managers to varying degrees. The second, which has yet to be explored, is that managerial conceit may be a state variable. That is, managers who get *control cues* may have increased levels of managerial conceit. This conjecture leads us to examine how the illusion of control may evidence in managerial settings.

In the process of examining managerial illusion of control, we develop an enriched process model of illusion of control. In past work related to the illusion of control, there is little or no use of process measures that track exactly how a *control cue* leads to the illusion of control. Developing such a process model is important for two reasons. First, from a theory standpoint, it provides us with a more detailed understanding of the illusion of control phenomenon. Second, from a practice standpoint, if we are able to isolate a variable that mediates/ explains the illusion of control results, then we can dampen managerial illusion of control by introducing debiasing mechanisms that suppress the value of this mediating variable.

In sum, this paper has the following goals. First, we connect two very disparate literatures, i.e. we show how the illusion of control literature (in Psychology) connects to the managerial conceit literature (in Management Science). Second, we examine how illusion of control operates in managerial settings. Third, we develop an enriched process model for managerial illusion of control, and this greatly contributes to the theory underlying the illusion of control phenomenon. Fourth, we test our process model across a series of experiments. We examine how the managerial illusion of control may lead to suboptimal marketing decisions, and then we isolate those specific mediating variables that explain these suboptimal marketing decisions. Fifth and final, we use our process model to develop and test debiasing mechanisms that reduce the negative impact of the managerial illusion of control.

2.2 Model Development and Hypotheses

We start our model development by first discussing the concept of illusion of control, and then focusing on the link between the illusion of control paradigm and managerial conceit.

2.2.1 The Illusion of Control

The illusion of control literature typically uses gambling contexts where individuals cannot use their skill to affect gamble outcomes. Nevertheless, if individuals get to act on a cue which (generally speaking) suggests that they can use their skill to impact outcomes, then such individuals are more likely to bet in a manner inconsistent

with the underlying (true) odds of success. Langer (1975) proposes (but does not test) the idea that individuals use cues, such as the opportunity to choose an option, to determine if they can positively influence either the probability of, or the actual level of, a desired outcome. She supports this proposal by noting that the opportunity to choose an option normally implies the individual can actually influence the expected value associated with the possible task outcomes. However, using this simplifying heuristic to infer an ability to control the odds can lead to problems if the context only involves random chance (e.g., in gambles like lotteries and die rolls). Langer (1975) labels this tendency of individuals, who when given the opportunity to act on a *control cue*, overestimate their ability to influence the outcome of a gamble as the “illusion of control.” To the best of our knowledge, there is no research that provides a path model for this process (although Bouts and Van Avermaet (1992, pgs 334-5) have specifically called for such a path model). Ideally, such a path model would show two things. First, this path model would show exactly which components of the gamble are impacted by the *control cue*, and (also important) which components of a gamble are not impacted by the *control cue*. Second, the path model would show exactly how the impacted components impact, in turn, the overall gamble odds.

Since we base our experimental studies on Langer’s (1975) lottery ticket study, we briefly describe this study. Participants were randomly split into an experimental group where each participant *chose* a lottery ticket, and a baseline group where each

participant was *allotted* a lottery ticket. Subsequently, all participants were asked if they would exchange their lottery ticket for a new lottery ticket that had better odds of winning. Normatively speaking, all lottery ticket holders should have exchanged their tickets. However, a significantly larger proportion of experimental group participants decided not to exchange their original ticket. It appears that experimental group participants, who got the opportunity to act on the *control cue* (lottery ticket choice = *control cue*), were more likely to perceive that they could select a ticket with a better-than-normative probability of winning. It thus appears that if individuals get to act on a *control cue*, then such individuals overestimate their ability to control gamble outcomes, i.e., overestimate their ability to control the odds.

Some researchers however have questioned the above result (see Koehler, Gibbs and Hogarth 1994, pg 189), as it is possible Langer's results were driven by a "matching" explanation. To elaborate, all experimental group participants were able to pick out a favorite/ preferred number (or, were matched with their favorite number), while at least some of the baseline group participants were not assigned/ matched with their preferred number. It is quite possible that this dichotomy led to the result wherein the experimental group had a higher valuation for their lottery ticket. In support of this alternate explanation, Goodman and Irwin (2006) recently showed that, even in gambles wherein there are no *control cues*, individuals have certain preferred numbers, and individuals who are assigned such numbers give these numbers a higher probability of

winning vis. other numbers. Given the presence of such “preferred numbers”, an open question is whether, in Langer’s lottery ticket experiment, one would still see evidence of the illusion of control if (1) experimental group participants can choose a number, but (2) baseline group participants are “matched” with their preferred number. We further explore this question in our experimental studies.

Researchers have also examined how to dampen the illusion of control. Langer (1975) proposed that introducing an “intrusion of reality” into the situation will reduce the extent of illusion of control. Bouts and Van Avermaet (1992) dampened the impact of the illusion of control by asking experimental group participants to write down the objective odds of winning before acting on a *control cue*. Koehler, Gibbs and Hogarth (1994) dampened the impact of the illusion of control by focusing attention on base rates before experimental group participants acted on a *control cue*. However, given the absence of an explicit process model, it is difficult to exactly pin down exactly how this dampening mechanism works. Ideally, the debiasing mechanism should be shown as having impacted certain process variables (and not impact others), and in turn these impacted process variables should feed forward and lead to dampened levels of illusion of control.

To sum up, there are some key gaps in the illusion of control literature. First, there is no explicit process model that identifies how exactly a *control cue* manipulation leads to the illusion of control. Second, although researchers have found ways to debias

the illusion of control, there is no process explanation for how exactly such debiasing mechanisms work. Put another way, if a suitable process model were developed, and suitable mediating variables were found, it would be possible to create (new) debiasing mechanisms by using manipulations that suitably impact the mediating variable.

Finally, there are (yet unexamined) alternate explanations around the illusion of control studies that involve choice, e.g. Langer's (1975) lottery ticket study.

2.2.2 Link between the Illusion of Control and Managerial Conceit

We see considerable similarity between (1) the illusion of control results, where it appears that individuals who get the opportunity to act on a *control cue* (falsely) perceive they can use their skill to change the underlying odds of success in random chance gambles, and (2) a conjecture that managers, who get to act on a *control cue*, may have an increased tendency to overestimate their ability to control random chance factors in a business setting. Further, we see the possibility of applying the same general methodology used in the illusion of control experiments to situations involving complex marketing projects. Since this illusion of control methodology ensures that the *control cue* does not (normatively speaking) effect project outcomes, any differences in managerial choices between managers who get a *control cue*, relative to managerial choices by those who do not get a *control cue*, would evidence managerial illusion of control.

As noted earlier, managerial conceit has hitherto been conceptualized as an individual-level characteristic. It has its foundations in novice managers listening to post-hoc stories of managerial successes that emphasize the role of skill, and remove chance, from the explanatory narrative (March and Shapira, 1987). This learning mechanism leads managers to downplay the role of chance. Managers assume they can control the odds, thereby obtaining one of the more favorable outcomes from among the wider range of possible (favorable and unfavorable) outcomes. This conceptualization of managerial conceit imbues all managers with some managerial conceit, albeit to differing degrees.

In contrast, if we find evidence of managerial illusion of control, then managerial conceit may also be conceptualized as a state variable. That is, if managers can act on a *control cue*, then this leads managers to overestimate their ability to get a favorable outcome. To examine this new conceptualization, we apply the illusion of control methodology within the context of managers making a re-evaluation decision related to a previously launched new product. Such re-evaluation contexts exist in many marketing domains, such as new product launches, new distribution channel introductions, etc., where marketing managers initiate a project in the face of business uncertainty and subsequently must assess whether or not to persist with the project in face of negative post-launch information. We supplement the standard manipulation-response experimental approach found in the illusion of control literature by (also)

taking multiple process measures. This allows us to gain a better understanding of how the managers' belief structure is impacted/ changed by the *control cue*, and how this changed belief structure ultimately results in managers making suboptimal decisions.

2.2.3 Modeling Managerial Illusion of Control

2.2.3.1 The Baseline Situation

We use a decision context where managers integrate information across multiple project components (e.g., the project's market share, the competitor's response to the project launch) in order to form an overall impression, or summary belief, about a marketing project. As is true in most marketing decision contexts, the information signals provided about individual project components are somewhat ambiguous. Hence, managers must extract what they perceive as truth from these signals to assess the level of each of these components. Specifically, our context involves a new product launch setting. In period 1, all participants make the decision to launch a project. In period 2, participants re-evaluate this prior-launched project. During this re-evaluation situation the manager receives project feedback that is, on average, negative. In addition, the manager is specifically asked to consider an exit option that (normatively) dominates the option to persist with the current project. The manager must decide whether the firm should persist with the current (failing) project, or should the firm switch to the (dominating) exit strategy. Since this benchmark situation provides no *control cues* which may lead managers to believe they can "control the odds" of the project's future

outcome, any tendency to stick with a losing course of action in this benchmark situation would be attributed to factors such as improper use of prior beliefs (Bolton 2003; Biyalogorsky, Boulding and Staelin 2006), sunk cost fallacy (Staw 1981) and increased personal responsibility (Schmidt and Calantone 2002).

2.2.3.2 Effect of Control Cues

We start this discussion by acknowledging that (1) when a manager gets to act on a *control cue*, such action normally results in managers using their managerial skill to make some positive impact, and (2) some managers have more skill than others. The open question is whether managers overestimate the impact of acting on such *control cues*. We propose that, after controlling for individual differences in skill, managers who act on a *control cue* are more likely to overestimate their ability to control uncontrollable factors, i.e., exhibit managerial conceit. Moreover, this tendency will be associated with those specific aspects of the project which are most associated with the *control cue*. This leads to our first hypothesis:

H1: After controlling for managerial skill, managers who act on a control cue (vis. managers who do not act on a control cue) will have an inappropriately higher valuation for project components linked to the control cue

Hypothesis 1 links a *control cue* to a manager's valuation (belief) of project component outcomes most closely linked to the *control cue*. We next consider the link between a *control cue* and its impact on overall project evaluation. Consider a project

whose value is influenced by many components. In such an instance, a manager's summary belief about the project is a function of the manager's beliefs about each project component (Biyalogorsky, Boulding and Staelin, 2006). As just discussed, the presence of a *control cue* leads to managerial illusion of control, and thus leads to an upwardly biased valuation of the project components most closely linked to the *control cue*. In turn, this biased valuation leads to higher summary beliefs about the project, and thus leads to managers making overall suboptimal decisions at the project level. With this in mind, we state Hypothesis 2:

H2: Managers who act on a control cue (vis. managers who do not act on a control cue) will have a higher likelihood of making suboptimal decisions at the project level, i.e., are more likely to exhibit downstream consequences of having inappropriately higher valuations of project components linked to the control cue.

Hypotheses 1 and 2 imply a very particular process in terms of how a *control cue* leads to suboptimal downstream marketing decisions. That is, the inappropriately high evaluation of the component(s) of the decision associated with the *control cue* leads to the increased tendency to make an inappropriate project-level decision. However, this should not be true for components not associated with the *control cue*. If the above narrative is correct, then the component level valuations associated with the *control cue* should explain (mediate) the effect of a *control cue* on the project-level decision. With this in mind, we propose Hypothesis 3:

H3: The increased valuation of the project component linked to the control cue (Hypotheses 1) will explain (mediate) the result that managers who act on a control cue have an increased likelihood of making suboptimal decisions at the project level (Hypothesis 2).

2.3 Overview of Studies

We examine managerial illusion of control across three studies. In Study 1, we illustrate managerial illusion of control and we provide support for our proposed process model about managerial illusion of control. In Study 2, we rule out an alternate explanation for Study 1 results, and we provide further support for our proposed process model. In Study 3, we illustrate a debiasing mechanism to reduce the impact of managerial illusion of control, and we link this debiasing mechanism to our proposed process model.

2.3.1 The Stimulus

We use variants of a common base case across all studies². This case reflects a two-period, new product, setting where managers integrate project information across multiple project components (e.g., market share, market growth, competitive entry) to make decisions about the new product project. In period 1, managers make the decision about whether or not to launch the new product. Thereafter, in period 2, managers

² Versions of this case have been extensively used in managerial research; e.g. Boulding, Morgan and Staelin (1997); Biyalogorsky, Boulding and Staelin (2006)

decide whether or not to persist with the project. In period 2, we insert our manipulation: some managers get to act on a *control cue*, while others do not.

The general setup is as follows. Participants are asked to complete the Quality Valve Company (QVC) case. This case states that they (participants) are part of a new product committee at QVC; a committee that (1) is wholly separate from QVC's operating divisions, and (2) approves new product launches. In period 1, the decision problem is whether QVC should launch a new automotive valve - RXT 1. The success of RXT 1 is explained as depending primarily on three project components: (1) the market share of RXT 1 after launch, (2) the market growth rate, and (3) the competitive entry response of QVC's major competitor (Great Lakes Valve Company, or GLVC). To make the uncertainties about project components explicit, participants are provided with analyst's report that includes probabilistic forecasts of all the uncertainties and, based on these projections, a distribution of (estimated) NPVs. Participants are specifically told that, after incorporating all relevant cash flows, the project has a mean NPV of \$3.4M. Then, participants are asked to make a launch/no-launch decision, and the overwhelming majority of participants choose to launch the project³.

Participants are then told that two years have passed. They are now given the analyst's updated report, which documents RXT 1's post-launch performance. The

³ As in Boulding, Morgan, Staelin (1997), a small number (< 10%) of participants do not proceed with the project, and we drop these participants from the analyses

report includes the realized values of two of the three project components. It indicates positive news, i.e. market growth is more than anticipated. It indicates some negative news, i.e. market share is less than anticipated. There is no information about GLVC's plans to enter the market with a competitive product. The underlying message, however, is that the project is not meeting NPV expectations. Participants are now told that the firm is planning to hire a salesperson with the stated goal of improving the current situation. There are two salespersons being considered, and both salespersons will definitely join QVC if made an offer. Therefore, the value of the (to-be-hired) salesperson - to QVC - depends solely on the quality of the (to-be-hired) salesperson.

All this new/updated information is summarized in the analyst's revised report, and this report explicitly takes into account the impact of the hiring of one of these two stellar salespersons. The updated/ revised distribution of NPVs, after incorporating all relevant future cash flows, indicates a mean NPV of \$2M. Importantly, this revised NPV forecast is lower than the certain cash-out value of \$2.5M associated with an immediately available exit option, i.e., discontinuing RXT 1 and selling off the capital equipment. The report makes the NPV values and the benefits from the exit option very explicit, and consequently, when asked to decide between continuing with the RXT 1 project, or terminating the RXT 1 project and getting the certain cash-out value of \$2.5M, the appropriate (normative) decision is to terminate the project and sell the capital

equipment (see Boulding, Morgan and Staelin (1997) for a discussion of the appropriateness of this decision).

As in prior studies which examined the illusion of control, participants are randomly split into a baseline group and an experimental group, with only the latter group able to act on a *control cue*. The *control cue* is designed so that an impartial judge would say it cannot influence project outcomes. This enables us to attribute differences in managerial choices across groups as being due to managerial illusion of control.

All the studies are based on the lottery ticket study (Langer 1975). Specifically, participants are told that QVC is considering hiring either John Richards or Peter Jones to improve the performance of the RXT 1 project. The case states that both John and Peter are stellar GLVC salespersons, and that both salespersons will definitely join QVC if offered a position. The senior management in QVC is split over whether to hire John or Peter; half prefer John, and the balance prefers Peter. In the baseline group, participants do not control which salesperson is hired by QVC (participants are told that, if QVC persists with the RXT 1 project, then QVC will hire one of John or Peter)⁴. In the experimental group, participants actually select one of the two salesperson candidates. In all conditions, participants subsequently decide on whether or not to continue with the RXT 1 project. Consistent with Langer (1975), we expect that

⁴ Note we do not assign a specific salesperson to the baseline group. This is consistent with Gawronski, Bodenhausen and Becker (2006; Experiment 4), who show that mere assignment can create preference

participants who get to select/ choose a salesperson will be more likely to persist with the project. Also, consistent with Langer (1975), we are less interested in whether or not participants make a normatively inappropriate decision to persist with the RXT 1 project; rather we are more interested in how the propensity to persist with the RXT 1 varies across conditions.

2.3.2 Participants

Participants are post-graduate business students (e.g., Executive MBA participants, MBA participants). Such students are good proxies for mid-level industry managers (e.g., participants averaged six years of prior managerial experience). Further, prior research has shown the decision quality of this type of participant is similar the decision quality of more senior managers (Remus 1996; Montgomery, Moore and Urbany 2005; Biyalogorsky, Boulding and Staelin 2006). Participants are paid \$15 to participate in a 35-minute study.

2.4 Study 1

2.4.1 Pretest

We use 23 participants in this pre-test. After participants make a period 1 decision, and see all relevant period 2 information (including seeing the CVs of the two salespersons), the study is terminated and the participants are asked to compare John

and Peter. We use a nine-item measure⁵ ($\alpha = 0.94$), which we term as QUALITY OF SALESMAN, to assess participants' valuation of John and Peter. The scale used is a 1-9 scale, with [1] implying that John is better than Peter, [5] indicating that both salespersons are equivalent, and [9] implying that Peter is better than John. Pretest results ($M=5.29$, $SD = 1.34$) indicate that, on average, participants perceive John and Peter to be equal (the average rating is not significantly different from [5]; $t(22)=1.02$, $p>0.3$). However, only a subset of participants (21.3% participants) rated the salespersons as exactly equal, i.e. provided a rating of exactly [5] for all nine measures. In contrast, other participants rated one or the other salesman as overall better (albeit, on average, neither salesman was seen to be superior). This suggests that we have two kinds of participants, i.e., (1) those who perceive that the two salespersons are exactly equal, and (2) those who perceive that the two salespersons are unequal (note: in aggregate, this inequality is not significant). This point of inequality is important, and we later re-address this issue.

2.4.2 Main Study

We recruit 123 participants, of which 67 are randomly assigned to the experimental group and 56 are randomly assigned to the baseline group. This is shown in Table 1 (which overviews all three studies).

⁵ Examples of the items used: (1) Who is the better salesperson, given QVC's requirements?, (2) How would you rate the capabilities of Peter vis. the capabilities of John?

Table 1: Overview of Studies

| | | | |
|----------------|---|------------|--|
| Study 1 | Baseline Group (<i>n</i>=56) | VS. | Experimental Group# (<i>n</i>=67) |
| | Ps are assigned [one of two "equal" salespersons] | | Ps choose preferred salesperson |
| Study 2 | Baseline Group (<i>n</i>=60) | VS. | Experimental Group# (<i>n</i>=67) |
| | Ps are assigned preferred salesperson | | Ps choose preferred salesperson |
| Study 3 | | | Experimental Group 1# (<i>n</i>=67) |
| | | | Ps choose preferred salesperson |
| | | VS. | Experimental Group 2 (<i>n</i>=33) |
| | | | Ps choose preferred salesperson + write down counterfactuals |
| | P = participant | | |
| | # This group is common to all three studies | | |

2.4.2.1 Independent and Dependent Variables

Our principal dependent variable is FINAL PERIOD BELIEFS (two-item measure, $\alpha=0.87$). This reflects the propensity, in period 2, to persist with the RXT 1 project; and includes the dichotomous (Yes, No) measure relating to whether the participant wishes to persist with the project⁶. The next measure, SALESMAN QUALITY, reflects salesperson valuation. This is a two-item measure, with $\alpha=0.62$, but

⁶ The two questions are: (1) Will you recommend that QVC continue with the RXT 1 project? How confident are you as regards your answer? and (2) What is your overall impression of the RXT 1 project? We normalize responses to adjust for differences in scale. We use this variable, rather than use the dichotomous (persist, not persist) variable, as this variable is continuous and has more scale variance

our results hold even when we use the individual items⁷. The third measure is PRIOR PERIOD BELIEFS. This is exactly similar to FINAL PERIOD BELIEFS, but relates to whether or not the participant chooses to initiate the RXT 1 project in Period 1. We also take single-item measures of participants' estimates of (1) market growth, (2) market share, and (3) time before competitive entry, i.e. time before GLVC is expected to enter the market with a competitive product.

2.4.2.2 Contrasting the Two Experimental Subgroups

We set up two subgroups within Study 1 experimental group. In one subgroup, we first ask participants to make a decision about whether or not to persist with the RXT 1 project, and then we ask participants to provide measures of salesperson quality. In the other subgroup, we ask the same questions, but in the reverse order. We then test, across the two subgroups, for differences in FINAL PERIOD BELIEFS and SALESMAN QUALITY. There are no significant differences across the two groups (for both measures; $t(65) < 0.35$, $p > 0.75$), and so – going forward – we combine the two subgroups.

2.4.2.3 Testing for Differences between John and Peter

We also use experimental group data to further examine if there are any significant differences between John and Peter. This analysis complements our pre-test

⁷ The two questions are: (1) Do you think that John (or Peter) will be able to perform well (i.e. perform as well as Maria predicts) at QVC? How confident are you as regards your answer? and (2) Do you think that John (or Peter) will be able to bring in incremental profits (revenues less product costs less firm overheads) which will exceed his total compensation i.e. salary and any agreed-on bonus? We normalize responses to adjust for differences in scale

analyses. Of the 67 experimental group participants, 40.3% participants select John and the balance participants select Peter. We find no significant propensity to select any one candidate over the other (binomial test; $z=1.47, p>0.1$). Contrasting those who selected John and those who selected Peter, we find no significant differences in SALESMAN QUALITY ($t(65)<1.25, p>0.2$). The above findings are consistent with the pretest results. First, there is no significant propensity to pick one salesperson over the other. Second, the two salespersons, on average, are viewed as equal in quality.

2.4.2.4 Contrasting the Baseline Group Against the Experimental Group

We use this comparison to illustrate the managerial illusion of control, and also to examine our proposed process model for the managerial illusion of control. The major analyses are shown in Table 2.

Table 2: Study 1 Results

| Study 1 | | |
|---------------------|---|---|
| Hypothesis 1 | <i>Linear regression</i> DV: SALESMAN QUALITY IV: CONDITION** | $B= 0.66; t(121)= 4.66; p<0.05$ |
| Hypothesis 2 | <i>Linear regression</i> DV: FINAL PERIOD BELIEFS IV: CONDITION** | $B= 0.36; t(121)= 2.18; p<0.05$ |
| Hypothesis 3 | <i>Linear regression</i> DV: FINAL PERIOD BELIEFS IV: CONDITION** SALESMAN QUALITY | $B= 0.08; t(120)= 0.49; p=0.6$ $B= 0.43; t(120)= 4.23; p<0.05$ |

** [0] = Baseline Group; [1] = Experimental Group

We first test for differences in that specific aspect of the project associated with the *control cue*, i.e. for differences in evaluations of salesperson valuation. We do this by regressing SALESMAN QUALITY against the variable CONDITION ([0]=baseline group, [1]= experimental group). We find that CONDITION is positively signed, and significant ($t(121)=4.66, p<0.05$). This result supports Hypothesis 1.

We find that 80.6% experimental group participants choose to persist with the RXT 1 project, whereas only 64.3% baseline group participants choose to persist with the RXT 1 project. More formally, we regress FINAL PERIOD BELIEFS against the variable

CONDITION. We find that CONDITION is positively signed and significant ($t(121)=2.18, p<0.05$). This result supports Hypothesis 2.

Finally, we test for mediation. We run an ANOVA, with FINAL PERIOD BELIEFS as the dependent variable, and with CONDITION and SALESMAN QUALITY as independent variables. The two independent variables do not interact ($F(1,119)=0.19, p>0.65$). We now examine whether SALESMAN QUALITY mediates/ explains the impact of CONDITION on FINAL PERIOD BELIEFS. We run a regression for FINAL PERIOD BELIEFS, with CONDITION and SALESMAN QUALITY as independent variables. Here, we find that CONDITION is not significant ($t(120)=0.49, p>0.60$), whereas SALESMAN QUALITY is positive and significant ($t(120)=4.23, p<0.05$). Next, as per Baron and Kenny (1986), we calculate a Sobel z statistic ($z=3.40, p<0.05$) and we find that the path $\text{CONDITION} \rightarrow \text{SALESMAN QUALITY} \rightarrow \text{FINAL PERIOD BELIEFS}$ is significant. This result supports Hypothesis 3.

2.4.2.5 Examining Other Variables

First, we note that there are no significant differences in PRIOR PERIOD BELIEFS across conditions ($t(65)<0.4, p>0.7$). We also run an ANOVA, with FINAL PERIOD BELIEFS as the dependent variable, and with CONDITION and PRIOR PERIOD BELIEFS as independent variables. The two independent variables do not interact ($F(1,119)=1.78, p>0.15$). We now re-run the mediation analysis (similar to that reported above) using PRIOR BELIEFS as an additional independent variable. The

earlier-reported results sustain, with Sobel $z=3.30$ ($p<0.05$), and the path CONDITION → SALESMAN QUALITY → FINAL PERIOD BELIEFS remains significant. These results suggest that the variable PRIOR PERIOD BELIEFS does not impact our analyses.

Next, we examine if there are differences between the baseline group and the experimental group on other project components. We ask both the baseline group and the experimental group for estimates⁸ of (1) market growth, (2) market share, and (3) years before the competitor GLVC enters the market. In the case of market growth and market share, the period 2 analyst's report explicitly provides updated estimates. In the case of (potential) market entry by GLVC, no explicit information is provided. In all cases, there are no significant differences ($t(121)<1.2$, $p>0.2$) between estimates provided by the two groups. It appears that project components not directly linked to the *control cue* are not impacted (impact as measured by statistically significant differences across the two groups) by the *control cue*.

2.4.3 Discussion

Study 1 results make three important points. First, it provides support for our hypotheses. It provides an illustration of managerial illusion of control, whereby experimental group participants (who get to act on a *control cue*, i.e. who get to choose

⁸ Overall, as in Biyalogorsky, Boulding and Staelin (2006), participants significantly distorted estimates. The project report indicated that market share was 18%; participants' responses ($M=22.2$) are significantly higher ($t(122) = 4.66$, $p<0.05$). The project report indicated that market growth was 5% p.a.; participants' responses ($M=6.6$) are significantly higher ($t(122) = 2.34$, $p<0.05$). Here however we are not interested as to whether or not participants distorted estimates; we are more interested in differences between groups.

between two “identical” salespersons) have a greater propensity to persist with a failing project than baseline group participants (who are assigned one of two “identical” salespersons). It then elaborates on the process model underlying such behavior, by showing that experimental group participants have an increased value for the project component linked to the *control cue* (i.e., have an increased valuation for salesperson quality), and it is this increased valuation that leads to an increased propensity to persist with the project. Second, other values (less closely linked to the *control cue*, like market share, market growth and time to competitive entry) are not impacted. Past work on the illusion of control has used very simple stimuli, with few components, and hence has not parsed out which components are impacted by a *control cue* and which components are not impacted by the *control cue*. This enriches our understanding of the precise mechanism underlying the illusion of control.

This second result, which states that only some variables are impacted by the *control cue*, goes counter to alternate explanations that suggest that the increased propensity to persist with the failing project is linked to general/ overarching constructs like increased confidence etc. If indeed the results were driven by constructs like increased confidence, then this would impact across multiple variables, and we should see that experimental group participants view multiple project components as different from the baseline group. However, this is not the case.

Third, in one subgroup of the experimental group, we first ask for an overall project valuation, and then we ask for a specific valuation for the project component (i.e. salesperson quality) impacted by the *control cue*; in the other subgroup, we reverse the order of asking. Given that there is no significant difference across the two subgroups, we rule out an alternate explanation which suggests that experimental group participants have a higher overall project valuation, and it is this overall high project valuation (and not the *control cue*) that drives higher salesperson quality valuations.

With all this noted, there is an alternate explanation for Study 1 results, and this alternate explanation is conceptually similar to the criticism leveled against Langer's (1975) lottery ticket study. As seen from the pretest data, while some participants seem to think that the two salespersons are exactly "identical", the balance participants do seem to prefer one or the other salesperson. It may hence be argued that (1) in the experimental group, everyone gets their preferred salesperson, but (2) in the baseline group, some may not get their preferred salesperson. It is perhaps this dichotomy which drives experimental group participants to have an enhanced salesperson valuation, and consequently have an increased propensity to persist with the project. This explanation may well underlie other illusion of control results, and hence we examine this issue in Study 2. The open question is, if both baseline group participants and experimental group participants get/ are "matched" with their preferred salesperson, then would we

still find evidence for the managerial illusion of control, and would our proposed process model still hold?

2.5 Study 2

In Study 2, we create a new baseline group, but we use the same experimental group ($n=67$) as in Study 1. As shown in Table 1, participants in this new baseline group get assigned/ “matched” with their preferred salesperson. We come at this issue in two ways, using two subgroups in the baseline group.

2.5.1 Pretest

Hitherto participants are told that while both John and Peter are perceived as very good, half the senior management prefers John and the balance prefers Peter. We now insert a single line in the stimulus indicating that although opinions are split, if a decision had to be made today, QVC would hire Peter. A pretest ($n=21$) indicates that this single line creates a preference for Peter (20 out of 21 pretest participants select Peter over John; binomial test, $z=3.93$, $p<0.05$).

2.5.2 Main Study

We recruit 71 new participants, and we assign these to the baseline group. We retain the experimental group used in Study 1.

2.5.2.1 Subgroup 1 of the Baseline Group

In Subgroup 1 ($n=22$), all participants are “allotted” Peter, and (based on the pretest) we know that participants prefer Peter. Note here that Subgroup 1, a subgroup

in which participants are assigned their preferred salesperson, is created based on a manipulated variable (i.e., a manipulated preference for Peter).

2.5.2.2 Subgroup 2 of the Baseline Group

Here we revert back to the original Study 1 stimulus. In this subgroup, 49 participants are told the specific identity of the salesperson to be hired if QVC should continue with the project, i.e., some participants are “allotted” John, and the remaining are “allotted” Peter. This is in contrast to Study 1, where base line participants are told that, if QVC persists with the RXT 1 project, QVC will hire one of John or Peter. After the participants complete the study, these participants perform an additional task – they examine two (additional) CVs. These CVs are identical to the CVs of John and Peter, except that we remove any identifying features (e.g. name, e-mail address), making it difficult for these participants to identify which CV relates to John and which CV relates to Peter. Participants rate the two “anonymous” CVs using the same questions used in the Study 1 pretest. These ratings allow us to identify the participant’s preference for either John or Peter, without this preference being “contaminated” by the fact that the participant was earlier “allotted” either John or Peter. We then identify that subset of participants ($n=38$) whose ratings indicate that the participants think that their allotted salesperson (John or Peter, as the case may be) is either better than or equivalent to (but not worse than) the other salesperson. This subset of participants is included in the subgroup; others ($n=11$) are excluded. Hence Subgroup 2 (i.e. a subgroup in which

participants are assigned their preferred salesperson) is created based on a measured variable (this measured variable is their rating of the anonymous CVs), and by definition participants in this subgroup are allotted their preferred salesman. We should hence expect that these participants have higher SALESMAN QUALITY valuations than the excluded participants ($n=11$). This is exactly what we find. SALESMAN QUALITY for Subgroup 2 ($n=38$) is higher relative to those 11 participants excluded from Subgroup 2 ($t(47)=2.27, p<0.05$).

2.5.2.3 Combining Subgroups in the Baseline Group

We find no significant difference between the two subgroups, either in terms of SALESMAN QUALITY, or in terms of FINAL BELIEFS ($t(58)<0.4, p>0.65$). Hence we combine both subgroups. Note now that, in this Study 2 baseline group ($n=60$), participants are “matched” with their preferred salesperson.

2.5.2.4 Contrasting the Baseline Group Against the Experimental Group

Study 2 results (see Table 3) replicate Study 1 results.

Table 3: Study 2 Results

| Study 2 | |
|---------------------|--|
| Hypothesis 1 | <i>Linear regression</i> DV: SALESMAN QUALITY IV: CONDITION** |
| | $B= 0.32; t(125)= 2.22; p<0.05$ |
| Hypothesis 2 | <i>Linear regression</i> DV: FINAL PERIOD BELIEFS IV: CONDITION** |
| | $B= 0.38; t(125)= 2.37; p<0.05$ |
| Hypothesis 3 | <i>Linear regression</i> DV: FINAL PERIOD BELIEFS IV: CONDITION** |
| | $B= 0.25; t(124)= 1.61; p=0.1$ |
| | SALESMAN QUALITY $B= 0.43; t(124)= 4.64; p<0.05$ |

** [0] = Baseline Group; [1] = Experimental Group

First, we test for differences in evaluations for that aspect of the project associated with the *control cue*. We regress SALESMAN QUALITY against the variable CONDITION ([0]=baseline group, [1]= experimental group). We find that CONDITION is positively signed, and significant ($t(125)=2.22, p<0.05$). Second, as reported previously, we find that 80.6% Experimental group participants choose to persist with the RXT 1 project. Now we find only 68.3% of the Study 2 Baseline group participants choose to persist with the RXT 1 project. More formally, we regress FINAL PERIOD BELIEFS against the variable CONDITION. We find that CONDITION is positively signed, and significant ($t(125)=2.37, p<0.05$). Third, we run a set of regression equations as outlined in Baron and Kenny (1986). We run a regression for FINAL PERIOD BELIEFS, with

CONDITION and SALESMAN QUALITY as independent variables. We find that CONDITION is not significant ($t(124)=1.61, p>0.1$), whereas SALESMAN QUALITY is positive and significant ($t(124)=4.64, p<0.05$). Then, we calculate a Sobel z statistic ($z=2.08, p<0.05$) and we find that the indirect path CONDITION → SALESMAN QUALITY → FINAL PERIOD BELIEFS is significant.

2.5.3 Discussion

Study 2 addresses a (potential) limitation of Study 1 (and other illusion of control studies that involve choice). A prima facie valid explanation for the illusion of control studies (especially the lottery ticket studies) is that (1) experimental group participants always get their preferred item (in this case, salesperson), but (2) baseline group participants do not always get their preferred item. In Study 2, using two (complementary) approaches, we create a new baseline group wherein participants are “matched” with their preferred salesperson. We then show that our hypotheses hold even when baseline group participants are assigned/ matched with their preferred salesperson.

Study 2 adds further support for our proposed process model of managerial illusion of control. This process model is now robust across two studies, and hence we now take this process model as given, and – going forward – we use this process model to design debiasing mechanisms that should dampen managerial illusion of control.

2.6 Study 3

We now switch focus and examine ways to reduce managerial illusion of control. Prior work has used “intrusions of reality” (i.e., prompted participants to think of base rates; see Koehler, Gibbs and Hogarth 1994) to reduce illusion of control. However, these studies took no process measures, and so it is somewhat unclear how exactly these “intrusions of reality” reduced illusion of control. At this point, we note that, in Study 1 and in Study 2, we illustrate a process model which shows that increased valuations of salesperson quality (i.e. the project component most linked to the *control cue*) lead to an increased propensity to persist with the project. Given the robustness of this process model, we propose a mechanism that is expected to reduce valuations of salesperson quality, and hence we expect that this will also reduce the incidence of managerial illusion of control. We borrow from the literature on counterfactuals (e.g., Roese 2000; Sanna, Schwartz and Shocker 2002) and use counterfactuals to suppress salesperson valuation, thus reducing the propensity to persist with the project.

2.6.1 Main Study

2.6.1.1 Setting up the Groups

In Study 3, we use the same experimental group ($n=67$) as in Study 1; we relabel this Experimental Group 1. Also, we create a new experimental group ($n=33$); we call this Experimental Group 2. Experimental Group 2 is exactly similar to the experimental group in Study 1 (i.e., participants choose one of John or Peter), except that here we ask

participants two additional questions before (1) participants make the decision about whether or not to persist with the project, and before (2) participants provide a salesperson valuation. These two questions relate to counterfactuals, and ask the participant to write (1) a couple of reasons why the non-selected salesperson may be better than the selected salesperson, and (2) a couple of reasons why the selected salesperson may not be better than the non-selected salesperson. After the participants answer these questions, the participants are asked whether the salespersons are “equal” in ability, or whether one salesperson is superior to the other. About 84.8% participants indicate that the two salespersons are “equal”. As a purely illustrative point, this result contrasts with prior results (e.g. Study 1 pretest results) wherein only about 21% of the participants thought that the two salespersons are equal⁹. This does however suggest that using counterfactuals may reduce the “spread” between the two salespersons, and consequently reduce the valuation of the chosen salesperson. The two experimental groups are shown in Table 1.

2.6.1.2 Contrasting Experimental Group 1 with Experimental Group 2

Given that we expect that the same process model will operate, we run the same analyses as in Study 1 and in Study 2 (see Table 4).

⁹ The two results are not directly comparable (due to scale differences, stimulus-related differences)

Table 4: Study 3 Results

| Study 3 | |
|--------------------------|--------------------------------------|
| <i>Linear regression</i> | |
| DV: SALESMAN QUALITY | |
| IV: CONDITION** | $B = -0.38; t(98) = -2.12; p < 0.05$ |
| <hr/> | |
| <i>Linear regression</i> | |
| DV: FINAL PERIOD BELIEFS | |
| IV: CONDITION** | $B = -0.40; t(98) = -2.02; p < 0.05$ |
| <hr/> | |
| <i>Linear regression</i> | |
| DV: FINAL PERIOD BELIEFS | |
| IV: CONDITION** | $B = -0.26; t(97) = -1.35; p = 0.15$ |
| SALESMAN QUALITY | $B = 0.36; t(97) = 3.51; p < 0.05$ |

** [0] = Experimental Group 1; [1] = Experimental Group 2

First, we test for differences in evaluations for that aspect of the project associated with the *control cue*. We regress SALESMAN QUALITY against the variable CONDITION ([0]=Experimental Group 1, [1]=Experimental Group 2). We find that CONDITION is negatively signed, and significant ($t(98) = -2.12, p < 0.05$). This indicates that, as expected, Experimental Group 2 has significantly reduced salesperson valuations vis-à-vis Experimental Group 1.

Next, as reported previously 80.6% Experimental Group 1 participants choose to persist with the RXT 1 project, whereas only 69.7% Experimental Group 2 participants choose to persist with the RXT 1 project. To test this more formally, we regress FINAL

PERIOD BELIEFS against the variable CONDITION. As expected, we find that CONDITION is negatively signed and significant ($t(98) = -2.02, p < 0.05$).

Finally, we test for mediation. We run a regression for FINAL PERIOD BELIEFS, with CONDITION and SALESMAN QUALITY as independent variables. We find that CONDITION is not significant ($t(97) = -1.35, p > 0.15$), whereas SALESMAN QUALITY is positive and significant ($t(97) = 3.51, p < 0.05$). Then, as outlined in Baron and Kenny (1986), we calculate a Sobel z statistic ($z = 1.86, p < 0.1$) and we find that the indirect path CONDITION → SALESMAN QUALITY → FINAL PERIOD BELIEFS is significant.

2.6.2 Discussion

Study 3 results make two important points. First, it illustrates a new mechanism (use of counterfactuals) which may be used to debias the illusion of control, and this has implications for practice (we discuss this further in the next section). Second, it adds support for our proposed process model for managerial illusion of control. Study 3 results show that the debiasing mechanism works along exactly the same process path identified in Studies 1 and 2, and hence we can use our proposed theory to exactly explain why, and how, this debiasing mechanism works. It also suggests that we can use our process model for designing other mechanisms that could be expected to debias the managerial illusion of control.

2.7 Conclusions and Discussion

In this paper, we run three studies wherein we illustrate managerial illusion of control, and we validate our proposed process model for the managerial illusion of control. We show that when managers act on a *control cue*, they have an increased valuation for the project component linked to the *control cue* (other components are not significantly impacted), and this increased valuation feeds forwards and leads to suboptimal marketing decisions at the project level.

We believe these studies are the first to show direct empirical evidence of managerial illusion of control in a complex marketing setting, and the negative consequences thereof. In addition, as requested for by the field, we take multiple process measures, and hence we provide a rich process model for how managerial illusion of control impacts the downstream decision-process. Further, we show that the illusion of control results still hold when we contrast those who choose their preferred item to those who are merely “matched” with their preferred item. This is an important result, and it responds to a key gap in the illusion of control literature.

Finally, based on the established link between the overestimation of the project component associated with the *control cue* (and only that component), and the overvaluation of the project, we show how to develop (new) debiasing mechanisms to dampen the impact of managerial illusion of control. Specifically, we use counterfactuals

to dampen valuations of the project component associated with the *control cue*, and in turn this dampens the impact of managerial illusion of control.

Like any laboratory-run experimental study, there is a question around how far our results generalize to the managerial world. However, the presence of *control cues* is fairly probable in managerial settings. For example, in a recent interview with a project manager in a Fortune 500 company, we were told that managers who review new projects at stage-gates often have the opportunity to add (or can recommend to add) a new marketing manager to a failing project, or introduce a new channel of distribution for a failing new product. Thus *control cues* have face validity. Furthermore, we use a participant-sample that has been exposed to such decision environments and is drawn from a pool similar to that used in Bialogorsky, Boulding and Staelin (2006). These researchers conducted external robustness checks for their sample and showed that their results mirrored results obtained from more experienced managers.

2.7.1 Relevance for Theory

2.7.1.1 Theory Contribution

This research broadens the concept of managerial conceit to include a version of managerial conceit triggered by *control cues*. That is, this research suggests that managerial conceit can also be a state variable. In this article, we refer to this state variable version of managerial conceit as the managerial illusion of control. This

research hence connects two hitherto disparate literatures – the literature on managerial conceit and the literature on the illusion of control.

Specifically, this research introduces a new dimension into both the conceptualization of, and the causal mechanism of, managerial conceit, and thus has important implications for marketing practice. Managerial conceit may well impact a broad swath of strategic marketing initiatives, like the launch of a new product, a new channel, or a new pricing policy. Hence this research, and future research, into managerial conceit is expected to impact a wide variety of marketing domains.

Second, this research contributes to the research on escalation, a field of inquiry that marketing scholars have identified as a key area for future research (Hauser, Tellis and Griffin, 2006). As a stylized fact, we note that, in Studies 1 and 2, the presence of a (non-impactful) *control cue* increased the propensity to escalate commitment to a failing product by an incremental 15-25%, which is a non-trivial increase. Researchers have hitherto attributed escalation to factors like (1) improper use of prior beliefs (Bolton 2003; Biyalogorsky, Boulding and Staelin, 2006), (2) sunk cost fallacy (Staw 1981) and (3) increased personal responsibility (Schmidt and Calantone 2002). All the aforementioned factors share a common feature; namely that the manager in question must have participated in, or formed beliefs about, the project in a prior period. However, our research suggests that escalation may also be influenced by factors independent of whether or not the manager was associated with the project in a prior period.

Specifically, our research indicates that managerial illusion of control leads to increased escalation, and this should evidence even after adjusting for how the manager is involved with the project in a prior-period. Rather, the manager simply has to have the opportunity to act on a *control cue* that suggests he/ she can affect the future success of the project.

More broadly, this research contributes to the literature on the illusion of control. First, responding to the field, it develops and validates an enriched process model for the illusion of control. This process model provides us with a more fine grained understanding of the illusion of control phenomenon, explaining exactly how acting on a *control cue* leads to overvaluation of an overall project associated with the *control cue*. Second, it addresses some open questions in the illusion of control literature, e.g. will illusion of control still be evidenced when we control for “matching” (here, refer Study 2). Third, given the robustness of our process model, we use this process model to design a mechanism (in this case, counterfactuals) to dampen the illusion of control, and then we explain such dampening using our process model. This makes two contributions. The first is that we connect the literature on counterfactuals to the literature on the illusion of control. The second is that we show, by example, exactly how to use our process model to design mechanisms that dampen the illusion of control.

2.7.1.2 Avenues for Future Research

This research also opens up multiple avenues for future research in managerial decision making. Given that there is little to no empirical research about managerial conceit in complex marketing settings, and given that managerial conceit potentially impacts a variety of strategic marketing initiatives, there is need to understand more about managerial conceit. Since managerial conceit is triggered by *control cues*, there is also a need to understand what types of cues function as *control cues*. The answer to this question is not obvious since other researchers (e.g., Chau and Phillips 1995) have shown that not all choice cues trigger the illusion of control in gambling contexts. There is also a need to understand how managerial illusion of control is influenced by various task characteristics and actor characteristics (see Miller and Ross (1975) for a partial review of analogous research in the domain of individual decision-making). Task characteristics certainly impact individual decision-making. For example, Burson, Larrick and Klayman (2006) discuss the above-average effect, and show that under certain conditions individuals may actually consider themselves below average, i.e., exhibit the conceptual opposite of conceit. This phenomenon is triggered when individuals engage in tasks they perceive to be difficult. Thus it may be necessary to identify boundary conditions where managers consider tasks to be easy (difficult), as this could moderate the presence of managerial illusion of control (in passing, we note

that illusion of control has always been explored in domains wherein the task is easy, e.g. a lottery ticket choice, a die roll etc.).

This research also suggests new directions for research on the illusion of control. First, it would be good to validate the process model in other settings (i.e., in settings that use a different manipulation). Second, it would be useful to use this process model to explain past work on the dampening on the illusion of control. Finally, it would be interesting to use this process model, but to use other mechanisms (i.e., other than counterfactuals), to dampen the illusion of control.

2.7.2 Relevance for Practice

Our model of managerial illusion of control suggests that (1) if a *control cue* is present, and (2) if a manager has the opportunity to personally act on such *control cue*, then such manager is likely to overestimate his/ her actions on expected outcomes. As our studies show, this overestimation can sometimes lead to suboptimal marketing outcomes. We see two ways of reducing the managerial illusion of control, and we elaborate on these below.

2.7.2.1 Removing Control Cues from the Decision Environment

One way to reduce managerial illusion of control is to remove *control cues* from the managers' immediate decision environment (see results in Study 1 and in Study 2). We propose three types of organizing mechanisms that firms can adopt to reduce managerial illusion of control. Firms can create an in-house group of experts who

independently evaluate marketing projects, but who are not asked to get involved in the project in terms of providing managerial actions. By way of example, consider how large banks like HSBC manage corporate loans. Apart from line units, these banks have an audit team of experts, wholly separated from line units, which independently evaluates such loans. Given that the audit team does not directly manage loans, they cannot act on *control cues*, and thus their evaluation is less likely to be tainted by managerial conceit.

Alternatively, firms can bring in external experts to evaluate marketing projects, or at least, provide a second opinion on projects. For example, consulting firms like Bain Consulting are sometimes called in by private equity firms to independently evaluate private equity investments. Given that these consulting firms are not/ will not be involved in managing such investments (i.e., they cannot directly act on any *control cues*), their evaluations are less likely to be affected by managerial conceit.

We recognize that the previous two solutions can be expensive. Not all organizations can afford to have a strong bench of experts who are deliberately kept insulated from project management. Nor can all organizations afford to regularly bring in external consultants. Thus, a third possible solution is to set up workaround methods for controlling managerial conceit. As an example, W L Gore brings in managers from outside the product development team to make hard decisions about whether or not a product should be “killed”. This brings to the table a group of managers who have some

domain expertise, and yet such managers are sufficiently separated from direct project management such that managerial conceit is minimized.

2.7.2.2 Dampening the Impact of Control Cues

In many cases, it is not possible to reduce *control cues*. We recently interviewed the COO for a fast growing, locally-based, company that sells B2B software. This COO is actively involved in all aspects of new product development. While conceding the possibility for the managerial illusion of control, he indicated that the company was too small, and had too few senior managers, to have new products evaluated by an independent set of outside experts/ senior managers. It is in such situations that one may consider using debiasing mechanisms like the one outlined in Study 3. Given the robustness of our process model, one can apply this process model to any case where there are *control cues*, identify those project components most likely to be impacted by the *control cue*, and then implement a targeted mechanism (like, counterfactuals) that dampens valuation of these prior-identified project components.

2.7.2.3 Other Applications

More generally, managerial conceit affects any business situation where the manager gets to act on *control cues*. For example, consider the case where a line manager provides project estimates. A popular view is that a line manager is best able to provide informed estimates. However, since this line manager will also have some direct “control” over the future of the project, this manager is very likely to be impacted by the

opportunity to personally act on *control cues*. As such, estimates from line managers may well exhibit an element of managerial conceit. A typical response by firms is to use some algorithm to modify estimates given by line managers, but if managers anticipate this, then this has limited utility. Just as we use indirect questions in market research to reduce response bias (e.g., case of Social Desirability Bias, see Fisher 1993), it might be better to ask the line manager to provide estimates assuming that another manager is running the project, or perhaps provide estimates for other managers, thus reducing *control cues*, and consequently reducing managerial conceit.

2.7.3 Is Managerial Illusion of Control, or more generally Managerial Conceit, all Bad?

March and Shapira (1987) explicitly state that it is not clear whether or not managerial conceit is always a bad thing. In fact, these researchers explicitly state “we may prefer to have managers imagine (sometimes falsely) that they can control their fates, rather than suffer the consequences of their imagining (sometimes falsely) that they cannot.” Certainly one can find many instances in business history where managers have struggled in the face of extreme odds, and in such cases managerial conceit was probably a good thing.

The more relevant question therefore is - when is managerial conceit likely to have adverse results? This is certainly a fruitful avenue for future research. As an initial hypothesis, we propose that in cases where a manager is responsible for running a project, and responsible for the success of a project (e.g. a project manager responsible

for developing a new product, a brand manager seeking to introduce a new channel of distribution), managerial conceit may not necessarily be a bad thing – in fact, the provision of a *control cue* may well have some motivating effects. On the other hand, in cases where managers must review and decide between competing priorities, and when resources are scarce (e.g., a stage-gate manager deciding between two competing new product ideas), then elements of managerial conceit might lead to selectively preferring those projects where the reviewing manager has stronger *control cues*, and in this case managerial conceit might have negative consequences.

2.7.4 Closing Discussion

We close by noting that the addition of managerial skill normally has a positive affect on a marketing project. However, a manager's beliefs about his/ her managerial skill can also have a dark side, leading firms to enter into/ persist with suboptimal marketing strategies. It therefore behooves academics to actively research the antecedents and consequences of managerial illusion of control, and by extension managerial conceit, and it behooves firms to implement suitable organizational mechanisms which will minimize the formation of managerial conceit.

3. Real Options: A Gold Mine, or Fool's Gold?

3.1 Introduction

The first real option dates back at least to ancient Greece (Copeland and Keenan 1998). Thales divined (from some tea leaves) that there would be a bumper crop of olives six months forward. Thales then invested a little money and purchased, from some owners of olive presses, the right to rent olive presses six months ahead at the current rate. Six months forward, there was indeed a bumper olive harvest, and in turn olive growers sought increased pressing capacity. Thales rented out presses at a high rate, paid only the agreed-upon rate to the owners of the presses, and pocketed the difference.

Real options are an integral part of how marketing managers structure, and value, strategic initiatives. For example many new product development initiatives are structured as real options (e.g., Merck actively uses real options while developing new product alliances). Likewise, many marketing joint ventures are valued as real options (e.g., Anheuser-Busch's expansion into Chile, wherein Anheuser-Busch took minority stakes in various local brewing companies, has been valued using real options; Arnold and Shockley 2005).

As illustrated by the Thales example, there are a number of key aspects associated with a real option. First, there is uncertainty about a future event, i.e. whether or not there would be a bumper harvest. Second, the option holder (in this case, Thales) has the

opportunity to react in different ways depending on the harvest outcome. If the olive harvest turns out to be bountiful, Thales could “exercise the option”, i.e. acquire a valuable asset (olive pressing capacity) at a preferential price. However, if the olive harvest turns out to be poor (i.e., the option turns out to be a “failing option”), Thales need not (and should not) exercise the option. The latter point is important; abandonment of failing options contributes substantially to the benefits from using real options.

Third, the real option process consists of two distinct stages. In Stage 1, the “option-purchase” stage, the option-holder pays some upfront amount and buys the option. Very typically, there is some uncertainty around a focal event at the time of this initial purchase decision (in this case, the uncertainty was around whether or not there will be a bumper harvest in the future), and the real option structure allows the option-holder to make different choices depending on the observed outcome of this focal event. In Stage 2, the “option-exercise” stage, the option-holder must make the decision of whether or not to exercise the option after observing the focal event (e.g., in the Thales’ example, he observes the size of the olive harvest). Note however that although there may be less uncertainty in Stage 2 (i.e., the outcome of the focal event is known), it is normally the case in marketing situations that there is still some residual uncertainty in Stage 2 (e.g., in the Thales’ example, Thales will still be uncertain about – for example – whether any new pressing capacity has recently been built). Thales would have had to

make a “best estimate” about such uncertainty before making his option-exercise decision.

Most real options research has focused on the Stage 1 “option-purchase” decision. There is much research on the benefits of buying a real option (Li, James, Madhavan and Mahoney, 2007), on the optimal conditions for using real options (Moel and Tufano 2002; McGrath and Nerkar 2004; Reuer and Tong 2005) and how behavioral biases creep into the upfront valuation of options (Miller and Shapira 2004). However, there is less research into the “option-exercise” decision stage (Reuer and Tong 2007). More specifically, there is very little research into what factors make a marketing manager more (or less) likely to make a suboptimal exercise decision.

We start with the observation that, in most marketing situations, although the uncertainty related to the focal event is often resolved or at least reduced, there is usually some residual uncertainty related to other factors when the manager must make the Stage 2 decision. This assumption differs from most extant real option research, and this extant research thus makes the assumption that managers will make a normatively appropriate decision in the “option-exercise” stage (e.g., see Haenlein, Kaplan and Schroder 2006). Interestingly, researchers have now queried whether managers do indeed make optimal option-exercise decisions (Adner and Levinthal 2004; Guler 2007). Most recently Reuer and Tong (2007) indicated the need to identify the drivers of suboptimal option-exercise decisions, while Coff and Laverly (2007) examined some

factors (e.g., linkage between options and organizational routines) that may make managers more (or less) likely to make a suboptimal option-exercise decision in this stage.

Our paper directly addresses this very topic, i.e. what factors may make a manager more (or less) likely to make a suboptimal option-exercise decision. We conduct a controlled experiment, with managers as participants, and we show how option-structure influences whether managers are more (or less) likely to make a suboptimal option-exercise decision. We also identify the endowment effect as the underlying driver of how differences in option-structure leads managers to be more (or less) likely to make a suboptimal option-exercise decision.

In taking this approach, our paper differs from most work on real options, since almost all this former work has been driven by theory from Finance, Economics and Management Science. By introducing elements of Psychology and Behavioral Economics into real option theory, we push forth the boundaries of real options theory. Specifically, we hope that this research will be an early contributor to a new stream of research that not only looks at how managers make option-exercise decisions, but also (more broadly) looks at the important issue of how firms manage real options after these firms have made the option-purchase decision.

Next, we are able to provide deeper insights into the endowment effect by showing three key results. The first result is that the endowment effect is primarily driven by the

individual's feelings of psychological ownership of the asset, independent of whether the individual legally owns the underlying asset. The second result is that endowment effect is triggered not only by differences in ownership, but also by differences in contingent ownership – this thus extends the boundaries of endowment effect to a new class of assets. The third result is that endowment effect extends not only to cases where the individual owns/ does not own the asset, but also to cases where the group, to which the individual is affiliated, owns/ does not own the asset.

Further, this paper contributes to practice, with implications for (1) how to structure real options, (2) how to value real options, and most important (3) once the firm purchases an option, how should marketing managers (better) manage such options.

3.2 Theory Development

We start our discussion with an example from the pharmaceutical sector. In the 1990s, Merck signed an agreement with Biogen to develop an asthma drug. Merck structured the deal as a real option. That is, Merck paid Biogen \$15M upfront, and consequently Merck had the right, but not the obligation, to buy the drug for a further \$130M at a specified later date. In return, Biogen had to shepherd the drug through the FDA approval process by the specified purchase date. There was much uncertainty about the focal event, i.e., whether or not, and when, the drug would ultimately get FDA approval. This option gave Merck preferential access to the asthma drug, and hence unlimited upside; but this option also capped Merck's downside at \$15M. If, for

example, FDA feedback cast doubts on whether or not the drug would be approved, or if another competitor came out with a better drug in the interim, Merck could exit the arrangement and not exercise the option, i.e. Merck had the option not to make the \$130M payment to Biogen.

We note here that, at the option-exercise stage, the uncertainty relating to FDA approval would have been resolved, but other uncertainties (e.g., new government regulation) would still persist. In such a situation, Merck would have to make a decision based on its best estimate of future costs and future benefits. We know from the literature on escalation of commitment that when decisions are made in the presence of uncertainty, managers are prone to making suboptimal decisions (Boulding, Morgan and Staelin 1997). Moreover, the propensity to make suboptimal decisions is increased in cases where the manager has, in a prior decision-stage, formed some positive beliefs about the project. Such prior positive beliefs will cause the manager to warp/ reinterpret future data to be more positive than it truly is (Biyalogorsky, Boulding and Staelin 2006). This two stage process is also present in real options. Thus the manager makes a “go” decision in the option-purchase stage, and hence at that stage this manager has positive beliefs about the underlying asset. It is therefore reasonable to assume that managers may well make suboptimal decisions in the option-exercise stage. All the factors, which have been hitherto identified by escalation researchers as leading to an increased propensity to make a suboptimal decision, are also likely to be applicable to real options

settings. To ensure that our research makes a contribution over and above such past research, we limit our investigation to those factors which are specific to real options, and investigate how these factors may make managers more (or less) likely to make suboptimal option-exercise decisions.

One factor that is specific to real options, and that may impact a manager's propensity to make a suboptimal option-exercise decision, is the structure of the option. To elaborate, the Merck-Biogen real options example represents a call option. Here (typically) the firm pays a small amount upfront (this is non-refundable). In the future, the firm has the right, but not the obligation, to buy an asset for a fixed price. If conditions are favorable, the firm should exercise the option and buy the asset. Else, the firm should not exercise the option and walk away from the deal.

However, real options may be structured as (financially equivalent) put options. Here (typically) the firm pays a small amount upfront (this is non-refundable), and also the firm pays a certain (typically large) amount to buy the asset. In the future, the firm has the right, but not the obligation, to sell the asset for a fixed price. If conditions are favorable, the firm should not exercise the option and not sell the asset. Else, the firm should exercise the option and sell the asset. For example, MedPharm, a privately owned generic pharmaceutical company, entered into a strategic alliance with World Diagnostics, Inc., a provider of medical diagnostic tests. MedPharm was to cross-market World Diagnostics' rapid diagnostic tests to its customers, and World Diagnostics, Inc.

was to introduce generic pharmaceuticals to its customer base. As part of the deal, MedPharm sought a put option whereby it could sell its share of jointly owned assets to World Diagnostics, Inc. if future conditions warranted.

We note that different option-types imply differences in ownership of the asset underlying the real option. In the case of call options, the firm does not own the asset, but has the option to buy the asset. In the case of put options, the firm owns the asset, but has the option to sell the asset. The work on endowment effect by Thaler (1980) and others suggests that managers may have a relatively higher valuation for the asset in the case of a put option, since the firm owns the asset. This higher valuation could, in turn, result in managers – in the put option condition – having a greater propensity to make suboptimal option-exercise decisions.

More specifically, in the call option case, the firm does not own the asset, and so the manager is expected to have a relatively lower valuation for this asset. Hence the manager is less likely to buy the asset, i.e. less likely to exercise the option. In turn, the manager is less likely to make a suboptimal option-exercise decision. In contrast, in the put option case, the firm owns the asset, and so the manager is expected to have a relatively higher valuation for the asset. Hence the manager is less likely to give up the asset, i.e. less likely to exercise the option to exit the asset. In turn, the manager is more likely to make a suboptimal option-exercise decision. This leads to the following hypothesis:

H 1: Managers are more likely to make suboptimal option-exercise decisions in the case of put options (vis. call options).

Although there is substantial agreement that the endowment effect exists, there is “limited amount of research on, and little agreement about, the psychological mechanisms underlying the endowment effect” (Johnson, Haubl and Keinan 2007). Past research has shown that legal ownership, although strongly related to the endowment effect, does not have much explanatory power when explaining why consumers tend to stay with the product they are given. Nor has there been much success in finding a measure that fully explains/ mediates the endowment effect (Johnson, Haubl and Keinan, 2007). Some researchers have suggested a memory-based mechanism, whereby value differences are due to differential perceptions of the traded item (Nayakankuppam and Mishra 2005), or (very similarly) due to differences in how sellers and buyers ask (themselves) queries about the traded object and thence construct values (Johnson, Haubl and Keinan 2007). To elaborate, Johnson, Haubl and Keinan (2007) show that “sellers” construct values by asking themselves a series of queries that disproportionately look at why the traded object should have an increased value, whereas “buyers” construct value by asking themselves a series of queries that disproportionately look at why the traded object should have a decreased value. These researchers then go on to show that, by merely changing the order of such queries, they can eliminate the endowment effect.

In contrast, Shu and Peck (2008) show that the construct of psychological ownership (partially) mediates/ explains a typical endowment effect result. This type of ownership (Pierce, Kostova and Dirks 2003) appears to be somewhat different from legal ownership and is characterized by the feeling that something is “mine”. For example, when truck drivers are assigned a specific truck to drive, they often develop “feelings” for their truck, even to the extent of personalizing the relationship with the truck by giving the truck a name. Hence individuals may “feel” ownership of an object without actually owning it, and vice versa. The key antecedents to psychological ownership appear to be the ability to control the object and the ability to insert “self” into the object (Pierce, Kostova and Dirks 2003).

It is an open question whether such feelings of psychological ownership would similarly be evidenced in real options settings, since these settings are very different from the typical settings used by endowment effect researchers. For example, Shu and Peck (2008) use the setting where an individual is endowed with a slinky, or not; Johnson, Haubl and Keinan (2007) use a setting where an individual is endowed with a mug, or not. In these settings, it is easy for the individual participant to imagine himself/herself touching and owning the object, and in some cases the individuals are even given the objects to hold. In contrast, in the setting we use (1) the firm, and not the manager, would have more direct control over the object, (2) it is more difficult for the managers to invest “self” into the object, and (3) the manager may not directly oversee

the use of the object. Nevertheless, some have suggested that psychological ownership is evidenced by employees towards firms (which are amorphous, intangible objects) that employ such employees (Pierce, Kostova and Dirks 2003). Hence it is not that much of a reach to hypothesize that psychological ownership may also manifest in cases of assets that are organizational assets (i.e., other than personal assets), and may also manifest in cases wherein the firm (and not the manager) more directly controls or invests in such assets.

We conjecture that managers will exhibit greater psychological ownership in cases where the firm owns the asset but has the right to sell (i.e., a put option) relative to cases where the firm does not own the asset but has a right to buy (i.e., a call option). And from there it is a natural extension (following from Shu and Peck 2008) to suggest that increased levels of psychological ownership will lead to increased valuations. This may explain why asset valuations are higher in the put option case (vis. the call option case), and may also explain why managers may be more likely to make a suboptimal option-exercise decision in the case of put options.

The above discussion leads us to develop the following two hypotheses:

H2: Managers are more likely to have higher psychological ownership for the asset in the case of put options (vis. call options)

H3: Differences in psychological ownership will mediate differences in the propensity of managers to make suboptimal option-exercise decisions.

In the next section, we describe a controlled experiment wherein we test the three hypotheses above.

3.3 A Controlled Experiment

3.3.1 Setting up the Cells

We test our hypotheses in a controlled experiment, using 92 post-graduate business students as participants in a 2 x 2 cell, between-subjects experiment. The stimuli that participants see differ along two dimensions (1) option-type (i.e., whether the option is a call-option, or whether the option is a put-option), and (2) option-purchase-decision (i.e., whether the participant makes the option-purchase decision, or not). As explained next, we include the second manipulated variable in order to better isolate the effects of psychological ownership.

Boulding, Morgan and Staelin (1997) contrasted escalation decisions across two conditions. The first condition was when the focal manager made a prior decision to move forward with a project, and then this same manager had to make a go/ no go decision when subsequent news indicated that the project was a failing project. The second condition was when another manager made a prior decision to move forward with a project, and then the focal manager had to make a go/ no go decision when subsequent news indicated that the project was a failing project. Their research indicates that managers are more likely to make an escalate commitment towards a failing project if they were associated with making the initial decision to move forward. We create

analogous conditions in a real options context. In one condition, the participant makes an upfront option-purchase decision, and then subsequently makes an option-exercise decision. In the other condition, the participant is told that some other manager in the firm made the upfront option-purchase decision, and then subsequently the participant makes an option-exercise decision. Our a priori belief (based on Boulding, Morgan and Staelin 1997) is that – despite seeing the same information at the time of option-exercise - significantly more participants will make a suboptimal option-exercise decision in the case where the same participants also made the prior option-purchase decision. However, there is no reason to believe that psychological ownership should interact with this prior-decision effect. Thus we do not expect endowment effect to mediate/ explain such differences.

3.3.2 Stimuli Equivalence

In order to be able to compare the decisions across the two option type conditions we ensure that all the participants (regardless of which cell they are in) see stimuli wherein cash flows associated with the option-exercise decision are mathematically equal. Second, in the two cells where participants (also) make a Stage 1 option-purchase decision, i.e. make the decision to either purchase a call option or make the decision to purchase a put option, these participants see stimuli wherein (again) cash flows are mathematically equal.

3.3.3 The Stimulus

We design stimuli which meet two criteria. First, the stimuli describe a business situation in which using a call option or using a put option would, *prima facie*, be acceptable. Second, the stimuli relate to a domain wherein few participants have prior work experience (and hence participants are less likely to rely on outside-of-stimuli knowledge)¹.

All participants play the role of a manager working for a (focal) company, a company based in South-East USA, which processes transactions for banks². This manager's role is to approve new business proposals. These new business proposals are typically put forward by the VP-Marketing, and this VP-Marketing is overall in charge of day-to-day customer-facing operations. In other words the participants were not expected to have any day to day connection with the project, but only make decisions concerning the proposed real option.

As indicated, some participants make both the option-purchase decision and the option-exercise decision, whereas others make just the option-exercise decision. We describe below the stimuli for those participants making both decisions (an abridged

¹ Less than 10% participants (6 out of 92) have prior exposure to the domain we choose

² Upfront, participants are given some information about why, and how, banks outsource transaction processing, and then participants are asked four questions to ensure that they have some basic understanding of this industry. Participants scored fairly well (M= 3.6, maximum was 4), indicating that they understood this information.

version is presented to those making just the option-exercise decision, and those participants are told that, two years ago, their firm made the option-purchase decision).

3.3.3.1 Stage 1 – The Option-Purchase Decision

After taking a test to determine if the participants understood the basic setting for the case, participants are presented with the following scenario. The focal company is approached by a bank in Colorado and is asked to process this bank's transactions. Since the focal company does not have any processing capabilities in Colorado, someone must build a transaction processing unit in Colorado.

Given that there is high fixed cost associated with the building of the processing center and the processing fee per transaction is fixed, the business model shows that it only makes sense for the focal company to continue with the deal if net revenues are high. There are only two ways to have high net revenues. The first is to have high volumes, and in turn, high volumes arise when the bank has a large number of customers. The second is to have low variable costs, i.e. low costs of processing individual transactions.

The Colorado bank indicates that it plans to make bids (over the next two years) to buy out/ acquire three smaller banks. If these acquisition bids go through, then the number of customers will be much larger than present, and hence transaction volumes will increase substantially. However, if the acquisition bids do not go through, the bank's volumes will not grow, and it will not be worthwhile for the focal company to

service the bank's transactions. A key uncertainty is therefore whether (or not) the Colorado bank's acquisition bids for three smaller banks will be successful. The focal company hence structures the deal as a real option – either as a call option, or as a put option. We describe each option sequentially.

3.3.3.2 Call Option Stimulus (Stage 1 – The Option-Purchase Decision)

In the call option version, the Colorado bank builds a processing center for \$5M. Separate from this, and as a precondition to buy the option, the focal company invests a small amount (\$1M) towards improving this center. Two years forward, the focal company has a call option, i.e. the right, but not the obligation, to buy the processing center for \$6.05M (this is the two-year forward value of building cost of \$5M – the business model case states that the applicable discount rate is typically 10% p.a.). Note that two years forward, a key uncertainty will be resolved, i.e. either the bank's acquisition bids would have been successful, or not. Note here however, some other uncertainties, like those related to cost of processing transactions, will remain even in period 2 (i.e. the option-exercise stage). If enough of the bank's bids have gone through (and hence customer numbers/ transaction volumes are high enough), then (all else equal) the focal company should exercise the call option and buy the processing center; else the focal company should walk away from the deal. The initial business model report, prepared by an experienced business analyst working for the focal company, assigns probabilities to whether (or not) the bank will be successful in its bids. The

analyst also prepares two distributions of NPV estimates that reflect all the uncertainties associated with the option-purchase decision. The first estimate is for the case if the bids are successful, and the second estimate is for the case if the bids fail. Comparing (1) the weighted average of the two NPV distributions with (2) the upfront (and non-refundable) \$1M that the focal company must pay towards the processing center, the project report is extremely positive, and it makes financial sense for the focal company to buy the call option by investing a small amount (\$1M) in the processing center.

3.3.3.3 Call Option Stimulus (Stage 2 – The Option-Exercise Decision)

Two years forward, the participants are told that only two of the three acquisition bids were successful, and hence the number of (bank) customer is not as high as would have been expected if all bids had gone through. Further, there is still some residual uncertainty about costs (in fact, average cost of processing transactions is now expected to be higher than what had been projected a priori). Given this new information, the analyst prepares a revised distribution of NPV estimates that reflects the remaining uncertainties. The mean of this distribution of NPV estimates (\$ 5.5M) is less than the certain amount that the focal company must pay (\$6.05M) to buy the processing center. Hence the normatively appropriate decision – from the point of view of the focal company - is not to exercise the call option, and to write off the \$1M (sunk cost) invested two years earlier.

3.3.3.4 Put Option Stimulus

The put option stimulus is similar, with minor differences, as described further. In this case, the focal company builds the processing center for (\$5M), as also invests an additional (and non-refundable) amount (\$1M) into the processing center. After two years, the focal company has a put option, i.e. the right, but not the obligation, to sell the processing center to the Colorado bank for \$6.05M. This \$6.05M is the two-year forward value of the building cost of \$5M (explained earlier), and hence allows the focal company to recover the building cost. Two years forward, the revised distribution of NPV estimates prepared by the analyst shows a mean value of \$ 5.5M (exactly the same as the call option version), and hence the focal company is normatively better off selling the processing center to the bank for the certain value of \$6.05M. As in the call option version, if (as is normatively better) the focal company terminates its involvement with the processing center by exercising the put option, then the focal company writes off the \$1M (sunk cost) invested earlier.

3.3.4 Key Measures

We list here two measures. The first measure relates to the decision about whether (or not) participants, in the role of a manager in the focal company, makes a suboptimal decision by either (1) buying the processing center (i.e., by exercising the call option), or (2) retaining the processing center (i.e., by not exercising the put-option). This measure is a three-item measure ($\alpha = 0.89$) and is a continuous measure. We label this

measure as MOVE FORWARD. Table 5 lists the three items (here, as an illustration, we use items from the call option stimulus) we use to create this measure. The second measure relates to the participant's ownership-related perceptions about the processing center i.e., PSYCHOLOGICAL OWNERSHIP. We use a four-item measure ($\alpha = 0.92$) by including all three items from Shu and Peck (2008), and selecting another item from van Dyne and Pierce (2004). Table 5 lists the four items we use to create PSYCHOLOGICAL OWNERSHIP.

Table 5: Measures

Measure: MOVE FORWARD

Items:

#1A - What should we do? (Select one of 2 options below by ticking that option)

Buy DFB's processing center for \$ 6.05 million, and process the client bank's transactions

Do not buy DFB's processing center, and exit the Colorado region

#1B - How confident are you about your answer to question #1?

#2 - Based on the new/ updated information, what is your overall impression of the value of the proposed outsourcing deal?

#3 - Based on the new/ updated information, do you think that it makes sense for us to process the client bank's transactions?

Note: Items 1B, 2 & 3 are answered on a 9 point scale, where 1=Low and 9=High.

We normalize responses before combining items, so as to adjust for scale differences

Measure: PSYCHOLOGICAL OWNERSHIP

Items:

#1. I feel like this is our processing center

#2. I feel a very high degree of ownership of the processing center

#3. It is hard for me to think of the processing center as ours (Reverse-scaled item)

#4. I feel like the processing center is owned by

Note: Items 1-3 are answered on a 9 point scale, where 1=Disagree and 9=Agree.

In item 4, 1= Client bank and 9=Us

3.3.5 Results

3.3.5.1 Hypothesis 1

First, we look at participants' propensity to make a suboptimal decision (i.e., persist with the outsourcing venture) as a function of option-structure. Like Boulding, Morgan and Staelin (1997), we are less interested in whether or not participants make a suboptimal option-exercise decision; rather our focus is on differences in the propensity to make a suboptimal option-exercise decision across conditions. First, we note that more participants persist with the outsourcing venture in the put option condition (M=72.7%) than in the call option condition (M=54.2%). More formally, we regress MOVE FORWARD against the variables OPTION-TYPE, OPTION-PURCHASE

DECISION and an interaction term (see Table 6). We find that OPTION-TYPE is positively signed³ and significant ($t(88)=2.11, p<0.05$). These results indicate that there is significantly greater propensity to persist with the (suboptimal) outsourcing venture in the put option condition than in the call option condition, and these results support Hypothesis 1.

Table 6: Testing Hypothesis 1

| | | |
|--------------------------|--|--|
| Hypothesis 1 | | <i>Linear regression</i> |
| DV: MOVE FORWARD | | |
| IV: OPTION-TYPE | | $\beta = 0.39; t(88) = 2.11; p < 0.05$ |
| OPTION-PURCHASE DECISION | | $\beta = -0.36; t(88) = -1.90; p < 0.1$ |
| OPTION-TYPE* | | |
| OPTION-PURCHASE DECISION | | $\beta = -0.27; t(88) = -0.69; p = 0.45$ |

As mentioned earlier, although we a priori expected more participants to persist with the outsourcing venture in the case where they made both the option-purchase and option-exercise decision, we find that OPTION-PURCHASE DECISION is negatively

³ We code [call option] = 0, and [put option] = 1. If the variable OPTION-TYPE is positively signed, then this implies that more participants choose to stay with the project in the put option condition

signed⁴ and marginally significant ($t(88) = -1.90, p < 0.1$), indicating that participants have a greater propensity to persist with the (suboptimal) outsourcing venture in the case where the participant makes only the option-exercise decision, compared to the case where the participant makes both the option-purchase decision and the option-exercise decision. This result runs counter to our expectations, and we discuss this in the discussion section.

Finally we note that the interaction term is not significant ($t(88) = -0.69, p > 0.45$). This is as expected: we selected two independent variables that we, a priori, expected to be unrelated.

3.3.5.2 Hypothesis 2

We look to explain why there is a greater propensity to persist with the (suboptimal) outsourcing venture in the case of put options than in the case of call options. First, we note that, as hypothesized, PSYCHOLOGICAL OWNERSHIP is higher in the put option condition ($M=6.8$) than the call option condition ($M=4.8$). More formally, we regress PSYCHOLOGICAL OWNERSHIP against the variables OPTION-TYPE, OPTION-PURCHASE DECISION and an interaction term (see Table 7). We find that, as hypothesized, OPTION-TYPE is positively signed and significant ($t(88)=5.48, p < 0.05$). This result supports Hypothesis 2.

⁴ We code [not make option-purchase decision] = 0, and [make option-purchase decision] option = 1. If the variable OPTION-PURCHASE DECISION is negatively signed, then this implies that more participants choose to stay with the project in the condition where they do not make an option-purchase decision

Table 7: Testing Hypothesis 2

| | |
|--|------------------------------------|
| Hypothesis 2 <i>Linear regression</i> | |
| DV: PSYCHOLOGICAL OWNERSHIP | |
| IV: OPTION-TYPE | $B= 1.98; t(88)= 5.47; p<0.05$ |
| OPTION-PURCHASE DECISION | $B= - 0.27; t(88)= - 0.70; p>0.45$ |
| OPTION-TYPE* OPTION-PURCHASE DECISION | $B= - 0.40; t(88)= - 0.53; p>0.55$ |

The other terms are not significant (in both cases; $t(88) > - 0.7, p > 0.45$) This is as expected, since a priori we conjectured that there is no linkage between PSYCHOLOGICAL OWNERSHIP and OPTION-PURCHASE DECISION.

3.3.5.3 Hypothesis 3

We test this hypothesis as follows. First, we formally run a regression for MOVE FORWARD against the variables OPTION-TYPE, OPTION-PURCHASE DECISION, PSYCHOLOGICAL OWNERSHIP and an interaction term between OPTION-TYPE and PSYCHOLOGICAL OWNERSHIP (see Table 8, first regression equation). We note that the variable OPTION-TYPE is now not significant ($t(87) = -0.41, p > 0.65$) and the variable PSYCHOLOGICAL OWNERSHIP is positively signed and significant ($t(87) = 4.58,$

$p < 0.05$). We also note that the interaction term is not significant ($t(87) = 1.13, p > 0.25$). Second, we conduct a Sobel test (as per procedures outlined in Baron and Kenny 1986), and the test shows full mediation (Sobel $z = 3.75, p < 0.05$). Put together, these results suggest that the variable PSYCHOLOGICAL OWNERSHIP fully mediates/ explains the increased propensity to make a suboptimal option-exercise decision in the put option condition, i.e. that the path OPTION-TYPE \rightarrow PSYCHOLOGICAL OWNERSHIP \rightarrow MOVE FORWARD is significant. This result supports Hypothesis 3.

Table 8: Testing Hypothesis 3

| | | |
|--------------------------------------|-----------------------------|--------------------------------------|
| Hypothesis 3 | <i>Linear regression</i> | |
| | DV: MOVE FORWARD | |
| | IV: OPTION-TYPE | $B = -0.08; t(87) = -0.41; p > 0.65$ |
| | OPTION-PURCHASE DECISION | $B = -0.29; t(87) = -1.67; p < 0.1$ |
| | PSYCHOLOGICAL OWNERSHIP | $B = 0.23; t(87) = 4.58; p < 0.05$ |
| | OPTION-TYPE* | |
| | PSYCHOLOGICAL OWNERSHIP | $B = 0.12; t(87) = 1.13; p > 0.25$ |
| Testing for reverse causality | <i>Linear regression</i> | |
| | DV: PSYCHOLOGICAL OWNERSHIP | |
| | IV: OPTION-TYPE | $B = 1.65; t(88) = 4.92; p < 0.05$ |
| | OPTION-PURCHASE DECISION | $B = 0.45; t(88) = 0.13; p > 0.85$ |
| | MOVE FORWARD | $B = 0.84; t(88) = 4.43; p < 0.05$ |

We also note that – as we a priori expected – the variable PSYCHOLOGICAL OWNERSHIP does not mediate/ explain the relationship between OPTION-PURCHASE

DECISION and the dependent variable. Thus, in Table 8 (first regression equation), OPTION-PURCHASE DECISION remains marginally significant ($t(87) = -1.67, p < 0.1$).

As more of a technical point, when testing for mediation, it is important to show that the dependent variable does not “cause” the mediating variable (i.e., there is no “reverse causation”). We address this issue in two ways. First, we note that the mediating variable does not mediate the impact of OPTION-PURCHASE DECISION on the dependent variable. If it was the case that the dependent variable “causes” the mediating variable, then all differences in the dependent variable should be mediated by the mediator. But this is not what we find.

Second, we follow the procedures outlined by David Kenny (<http://davidkenny.net>) and – to test for reverse causation - we run a regression equation with the dependent variable and the mediator interchanged. If the dependent variable “caused” the mediator, then it should fully mediate the effect of the independent variable on the mediator. But this is not what we find. When we run a regression for PSYCHOLOGICAL OWNERSHIP, with OPTION-TYPE, OPTION-PURCHASE DECISION and MOVE FORWARD as independent variables (Table 8, second regression equation), then both OPTION-TYPE and MOVE FORWARD stay significant (in both cases; $t(88) > 4.4, p < 0.05$). Since both these variables stay significant, we here have little evidence supporting reverse causation.

3.3.5.4 Increase in Explanatory Power

We also note that the variable PSYCHOLOGICAL OWNERSHIP significantly increases the explanatory power⁵ of the independent variables in explaining our dependent variable. Consider the linear regression shown in Table 6. The R-square is 0.09. If we add the variable PSYCHOLOGICAL OWNERSHIP to this regression, the R-square increases to 0.25 (the increase is significant; $F(1,87) = 19.2, p < 0.05$). This point buttresses the importance of the variable PSYCHOLOGICAL OWNERSHIP.

3.4 Conclusion and Discussion

The empirics presented in this paper show that managers are more likely to make suboptimal option-exercise decisions in the case of put options than in the case of call options. Furthermore, this increased propensity to make suboptimal option-exercise decisions in the put option condition is driven by the endowment effect.

The results above have important implications for both theory and for practice. On the theory front, the results in this paper have implications for research on (1) marketing strategy, and specifically real options, (2) escalation, and (3) endowment effect.

⁵ Note here the issue raised earlier in the literature (e.g., by Johnson, Haubl and Keinan, 2007) that the ownership variable (on a solus basis) has little explanatory power when it comes to explaining endowment effect

3.4.1 Marketing Strategy and Real Options

There is a rich set of literature in Marketing that looks at managerial decision making, and factors that drive managers to make suboptimal decisions under different scenarios (e.g. Boulding, Morgan and Staelin 1997; Montgomery, Moore and Urbany 2005; Biyalogorsky, Boulding and Staelin 2006; He and Mittal 2007). This paper follows in that tradition. It looks at real options, a strategic lens that is an important part of marketing strategy, and it looks at managerial decision-making specific to real options.

Until now, research on real options has typically been focused along two dimensions. First, research has mainly focused on the upfront decision to purchase a real option focusing on issues like when is it optimal to use real options and how to upfront value real options. This work uses theories from Finance, Economics and Management Science, and in most cases the underlying assumption is that managers will make rational/ normatively appropriate option-exercise decisions. However, over the past five years, there has been a growing sense that managers may well make suboptimal option-exercise decisions, and thence the question arises as to what factors make a manager more (or less) likely to make a suboptimal option-exercise decision. This research is an early contribution to what will hopefully be a stream of literature that borrows from Psychology and Behavioral Economics, that looks specifically at the option-exercise decision, and that more broadly looks at how firms manage real options after option-purchase.

3.4.2 Escalation

The research in this paper also contributes to the research on escalation. Thus far, experimental research on escalation has used stimuli that use both “call option like” scenarios (e.g., He and Mittal 2007) and “put option like” scenarios (e.g., Biyalogorsky, Boulding and Staelin 2006). However, there has been no investigation into whether escalation scenario structure (i.e., whether the scenario resembles a call option, or whether the scenario resembles a put option) impacts the extent of escalation. This research identifies a key contextual variable, option-structure, and shows the exact mechanism whereby this variable influences escalation.

This research also creates opportunities for further investigation. Prior work on escalation, notably Boulding, Morgan and Staelin (1997), has shown that multi-stage involvement with a scenario leads to greater levels of suboptimal decisions. In contrast, the results of our controlled experiment suggest that multi-stage involvement with a real options related scenario leads to lesser levels of suboptimal decision making. We discuss this point below.

We do not have process measures that allow us to determine why exactly we observe this difference (above). However, we note that participants were shown scenarios wherein they saw (1) a potentially positive outcome after the initial option-purchase decision, and (2) a potentially negative outcome after the initial option-purchase decision. The stimulus then indicated that the firm was best off abandoning

the project if there was a negative outcome. As such, at the time the participants made the decision to initiate the project, the participants also viewed a form of “stopping rule” related to future decision making. This form of stopping rule has not been tested before, but it could well be that this form of stopping rule is superior to prior-used stopping rules (which are less effective). We leave the testing of this conjecture to future research, and such future research should reconcile our results with the results of prior research.

3.4.3 Endowment Effect

The research in this paper also contributes to extant research on the endowment effect. It does so in a couple of ways. First, it illustrates how psychological ownership mediates/ explains the endowment effect, and hence provides support for in-process work which investigates the endowment effect, and mediation thereof. Specifically, it shows a measure that fully mediates the endowment effect, an important advance for the field. Second, it extends endowment effect research in two new directions. Thus, this research shows that endowment effect (and the psychological ownership construct) relate not just to objects owned by the focal person, but also to objects owned by the organization with which the focal person is affiliated. In theory, this could suggest a fertile research opportunity to investigate how endowment effect plays out in settings like in-group vs. out-group, collectivist vs. individualist cultures etc. In addition, this research extends endowment effect from ownership domains to contingent ownership domains. This is an important extension, as many consumer domains today reflect

contingent ownership (e.g., purchases with a money-back guarantee). Future research can explore the nuances of how endowment effect varies between ownership domains and contingent ownership domains.

3.4.4 Implications for Practice

Finally, the research in this paper has important implications how firms should use and value real options. First, our results imply that at the time of structuring real options, firms must trade-off the benefits of using put options with the potential drawback that the firm is more likely to make a suboptimal option-exercise decision. Second, if the firm chooses to use put options, our results imply that this tendency to stick with the put option is due to the endowment effect. Consequently, a firm may want to use suitable mechanisms (e.g., the guided query mechanism specified in Johnson, Haubl and Keinan 2007) to debias managers so that managers will make better decisions. Third, consider the case of an analyst evaluating/ valuing a portfolio of options. Comparing a firm that has a portfolio of call options, and another firm that has an equivalent set of put options, the analyst should assign a lower valuation to the portfolio of put options.

3.4.5 Closing Discussion

Making investment decisions in the face of uncertainty is risky. Real options has established itself as an insightful lens through which to view such investment decisions, especially in key marketing domains like new product launches, channel additions,

customer management etc. As such, real options could well be a gold mine for firms. However, if real options are to deliver on their promise, more research is needed on reducing the propensity of marketing managers to make suboptimal option-exercise decisions. This yet unresolved, under-researched problem may well result in real options turning out to be fool's gold. For real options to occupy center stage in marketing strategy, real options theory must include elements not only from Finance, Economics and Management Science, but also elements from Behavioral Economics and Psychology. This work is an early effort towards that end.

References

- Alba, Joseph W. and J. Wesley Hutchinson. "Knowledge Calibration: What Consumers Know and What They Think They Know." *Journal of Consumer Research* 27 (2000): 123-156
- Adner, Ron and Levinthal, Daniel. "What is Not a Real Option: Considering the Boundaries for the Application of Real Options to Business Strategy." *Academy of Management Review* 29, No. 1 (2004): 74-85
- Arnold, Thomas and Shockley, Richard. "Value Creation at Anheuser-Busch: A Real Options Example." *Journal of Applied Corporate Finance* 14, No. 2 (2001): 52-61
- Baron, Reuben M. and Kenny, David A. "The Moderator-Mediator Variable Distinction in Social Psychological Research: Conceptual, Strategic and Statistical Considerations." *Journal of Personality and Social Psychology* 51, No. 6 (1986): 1173-1182
- Biyalogorsky, Eyal, Boulding, William and Staelin, Richard. "Stuck in the Past: Why Managers Persist with New Product Failures." *Journal of Marketing* 70 (2006): 108-121
- Bolton, Lisa. "Stickier Priors: The Effects of Nonanalytic Versus Analytic Thinking in New Product Forecasting." *Journal of Marketing Research* 40 (2003): 65-79
- Boulding, William, Morgan, Ruskin and Staelin, Richard. "Pulling the Plug to Stop the New Product Drain." *Journal of Marketing Research* 34 (1997): 164-176
- Bouts, Patrick and van Avermaet, Eddy. "Drawing Familiar or Unfamiliar Cards: Stimulus Familiarity, Chance Orientation, and the Illusion of Control." *Personality and Social Psychology Bulletin* 18, No. 3 (1992): 331-335
- Burson, Katherine A., Larrick, Richard P. and Klayman, Joshua. "Skilled or Unskilled, but Still Unaware of It: How Perceptions of Difficulty Drive Miscalibration in Relative Comparisons." *Journal of Personality and Social Psychology* 90, No. 1 (2006): 60-77
- Camerer, Colin, and Lovallo, Dan. "Overconfidence and Excess Entry: An Experimental Approach." *American Economic Review* 89, No. 1 (1999): 306-318

- Chau, Albert. W. L. and Phillips, James G. "Effects of Perceived Control upon Wagering and Attributions in Computer Blackjack." *Journal of General Psychology* 122, No. 3 (1995): 253-269
- Coff, Russell and Laverty, Kevin. "Real Options meet Organizational Theory: Coping with Path Dependencies, Agency Costs and Organizational Form." Eds. Reuer, Jeffrey and Tong, Tony. *Real Options Theory*, United Kingdom: JAI Press, 2007
- Fisher, Robert. "Social Desirability Bias and the Validity of Indirect Questioning" *Journal of Consumer Research* 20 (1993): 303-315
- Gawronski, Bertram, Bodenhausen, Galen V. and Becker, Andrew P. "I like it because I like Myself: Associative Self-Anchoring and Post-Decisional Change of Implicit Attitudes." *Journal of Experimental Social Psychology*, 43 (2007): 221-232.
- Goodman, Joseph K. and Irwin, Julie R. "Special Random Numbers: Beyond the Illusion of Control." *Organizational Behavior and Human Decision Processes* 99 (2006): 161-174
- Guler, Isin. "An Empirical Examination of Management of Real Options in the U.S. Venture Capital Industry." Eds. Reuer, Jeffrey and Tong, Tony. *Real Options Theory*, United Kingdom: JAI Press, 2007
- Haenlein, Martin, Kaplan Andreas M. and Schoder, Detlef. "Valuing the Real Option of Abandoning Unprofitable Customers when Calculating Customer Lifetime Value." *Journal of Marketing* 70, No. 3 (2006): 5-20
- Hauser, John, Tellis, Gerard and Griffin, Abbie. "Research on Innovation: A Review and Agenda for *Marketing Science*." *Marketing Science* 25 (2006): 687-717
- He, Xin and Mittal, Vikas. "Understanding Escalation of Commitment: The Moderating Role of Decision Risk." *Organizational Behavior and Human Decision Processes* 103, (2007): 225-237.
- Heider, Fritz. *The Psychology of Interpersonal Relations*. New York: Wiley, 1958
- Johnson, Eric, Haubl, Gerald and Keinan, Anat. "Aspects of Endowment: A Query Theory of Value Construction." *Journal of Experimental Psychology: Learning, Memory and Cognition* 33, No. 3 (2007): 461-474

- Koehler, Jonathan, Gibbs, Brian and Hogarth, Robin. "Shattering the Illusion of Control: Multi-shot vs. Single-shot Gambles." *Journal of Behavioral Decision Making* 7 (1994): 183-191
- Langer, Ellen. "The Illusion of Control," *Journal of Personality and Social Psychology* 32 (1975): 311-328
- Li, Yong, James, Barclay, Madhavan Ravi and Mahoney Joseph. "Real Options: Taking Stock and Looking Ahead." Eds. Reuer, Jeffrey and Tong, Tony. *Real Options Theory*, United Kingdom: JAI Press, 2007
- March, James and Shapira, Zur. "Managerial Perspectives on Risk and Risk-taking," *Management Science* 33 (1987): 1404-1418
- McGrath, Rita and Nerkar, Atul. "Real Options Reasoning and a New Look at the R&D Investment Strategies of Pharmaceutical Firms." *Strategic Management Journal* 25 (2004): 1-21
- Miller, Dale T. and Ross, Michael. "Self-serving Biases in the Attribution of Causality: Fact or Fiction" *Psychological Bulletin* 82, No. 2 (1975): 213-225
- Miller, Kent and Shapira Zur. "An Empirical Test of Heuristics and Biases affecting Real Option Valuation." *Strategic Management Journal* 25, No. 3(2004): 269-284
- Moel, Alberto and Tufano, Peter. "When are Real Options Exercised? An Empirical Study of Mine Closings." *Review of Financial Studies* 15, No. 1(2002): 35-64.
- Montgomery, David B., Moore, Marian C. and Urbany, Joel E. "Reasoning about Competitive Reactions: Evidence from Executives." *Marketing Science* 24 (2005): 138-149
- Nayakankuppam, Dhananjay and Mishra, Himanshu. "The Endowment Effect: Rose-Tinted and Dark-Tinted Glasses." *Journal of Consumer Research* 32 (December 2005): 390-395
- Pierce, Jon, Kostova, Tatiana and Dirks, Kurt. "The State of Psychological Ownership: Integrating and Extending a Century of Research." *Review of General Psychology* 7 (2003): 84-107

- Remus, William. "Will Behavioral Research on Managerial Decision Making Generalize to Managers?" *Managerial and Decision Economics* 17 (1996): 93-101
- Reuer, Jeffrey and Tong, Tony. "Real Options in International Joint Ventures." *Journal of Management* 31 (2005): 403-423
- Reuer, Jeffrey and Tong, Tony. "Real Options in Strategic Management." Eds. Reuer, Jeffrey and Tong, Tony. *Real Options Theory*, United Kingdom: JAI Press, 2007
- Roese, Neal. "Counterfactual Thinking and Marketing." *Psychology and Marketing* 17 (2000): 277-280.
- Sanna, Lawrence, Schwartz, Norbert and Shocker, Shevaun. "When Debiasing Backfires: Accessible Content and Accessibility Experiences in Debiasing Hindsight." *Journal of Experimental Psychology: Learning, Memory and Cognition* 28, No.3 (2002): 497-502
- Schmidt, Jeffrey. B. and Calantone, Roger J. "Escalation of Commitment during New Product Development." *Journal of the Academy of Marketing Science* 30, No. 2 (2002): 103-118
- Shu, Suzanne and Peck, Joanne (2008) "The Effect of Mere Touch on Perceived Ownership." *Working Paper*
- Staw, Barry M. "The Escalation of Commitment to a Course of Action," *Academy of Management Review* 6, No. 4 (1981): 577-587
- Thaler, Richard. "Towards a Positive Theory of Consumer Choice." *Journal of Economic Behavior and Organization* 1 (1980): 39-60
- van Dyne, Linn and Pierce, Jon. "Psychological Ownership and Feeling of Possession: Three Field Studies Predicting Employee Attitudes and Organizational Citizenship Behavior." *Journal of Organizational Behavior* 25 (2004): 439-459
- Vosgerau, Joachim, Anderson, Erin and Ross, William T. "Can Inaccurate Perceptions in Business-to-Business (B2B) Relationships be Beneficial?" *Marketing Science* 27, No. 2 (2008): 205-224

Biography

Abhijit Guha was born in Cochin, India on November 8, 1968. Prior to coming to Duke University for a PhD, Abhijit received his Bachelor of Economics (Honours) from St. Stephen's College, Delhi University (1989); his Post Graduate Diploma in Management from the Indian Institute of Management, Ahmedabad (1991); and his Master of Business Administration from INSEAD, Fontainebleau (1999).