

THE COMPARATIVE MIND-SET AND MANAGERIAL DECISION MAKING

Christine Moorman*
T. Austin Finch, Sr. Professor of Business Administration
Fuqua School of Business
Duke University
moorman@duke.edu

Alison Jing Xu
Assistant Professor of Marketing
Rotman School of Management
University of Toronto
alison.xu@rotman.utoronto.ca

Vivian Yue Qin
Ph.D. Student in Marketing
Fuqua School of Business
Duke University
yue.qin@duke.edu

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Abstract

Making comparative judgments in one domain activates a *comparative mind-set* that disposes consumers to purchase a product rather than deferring choice in a different domain. We extend the theory in three ways by examining the influence of this mind-set on managerial decision making. First, we examine the process by which the comparative mind-set increases choice. Specifically, we demonstrate that a comparative mind-set increases the accessibility of “which-to-choose” procedures and decreases the accessibility of “whether-to-choose” procedures, which consequently decrease the importance attached to negative attributes in the decision making process. Second, we find that activating a comparative mind-set not only increases managers’ likelihood of making purchases, but also elevates their spending levels. Third, we identify a set of real-world moderators involving decision scrutiny and knowledge accessibility that either amplify or weaken the influence of the comparative mind-set on managers’ decisions. In addition to four lab studies involving actual managers, we conduct three quasi-experiments using presidential elections to evoke the comparative mind-set. We find that activating a comparative mind-set elevates managers’ spending levels in real business decisions.

Keywords: Comparative mind-set, managers, spending levels, quasi-experiment, presidential election, decision scrutiny, knowledge accessibility

INTRODUCTION

Consumer purchases often involve three sequential steps (e.g., Chiang 1991; Dhar and Nowlis 2004). First, consumers decide whether or not to buy a product or service. If yes, they compare the options in their consideration set and identify the most preferred option. Finally, they implement the choice, which involves exchanging money for the use and/or possession of the product. Although the process may be more formalized and scrutinized, this sequence of steps also occurs in many managerial decisions involving company purchase of products or services as well as in decisions to spend on marketing initiatives.

Stored knowledge about such choice procedures can be viewed as a script because activating one stage automatically prompts the next stage in the process (Schank and Abelson 1977). Therefore, once consumers or managers have decided whether-to-choose, it is natural for them to consider which-to-choose as the next step in the process. In this paper we consider what happens to managerial decision making when the natural progression of these steps is disrupted. In particular, we examine the consequence of activating a *comparative mind-set* which disposes managers to decide which-to-choose without first considering whether-to-choose.

The comparative mind-set effect has been demonstrated among consumers making purchase decisions. This research has shown that asking consumers to compare options in an unrelated task can dispose consumers to consider which-to-choose without considering whether-to-choose at all (Xu and Wyer 2007, 2008). When this occurs, consumers are more likely to make a purchase rather than to defer choice. Despite these important observations, the underlying mechanism by which a comparative mind-set influences choice has not been formally tested. Moreover, little is known about the boundary conditions surrounding this effect and whether the comparative mind-set also affects spending levels.

Given these limitations, our first goal is to extend the theory about the comparative mind-set effect in three ways. First, all research to date has examined the comparative mind-set effect on *choice*—the probability of making a choice versus deferring choice. We extend this research and examine whether a comparative mind-set also has a *spending level effect*—how much money is spent after making choices. Second, we examine the mechanism through which the comparative mind-set influences choice outcomes. Specifically, a comparative mind-set increases the accessibility of which-to-choose procedures and decreases the accessibility of whether-to-choose procedures, which consequently decrease the impact of negative attributes when managers integrate product information to make decisions. Third, past research has offered a limited view of the nomological net surrounding this interesting effect. We contribute to this literature by examining a set of real-world moderators that reflect forces associated with decision scrutiny and knowledge accessibility.

Our second goal is to investigate the degree to which activating a comparative mind-set influences marketers' decisions. If decisions are swayed by factors that are not important to firm strategy, including the mental states the manager is in during the decision making process, the return on these marketing investments is not likely to be optimal (e.g., Hanssens, Rust, and Srivastava 2009). If such spending patterns persist, this will weaken the contributions of marketing to the firm (see Moorman and Rust 1999).

We test our predictions in four lab experiments and three quasi-experiments. Using a non-managerial mind-set manipulation from consumer research (comparing animals from Xu and Wyer 2008) among business professionals in an EMBA program, experiment 1a shows that activating a comparative mind-set increases participants' likelihood of purchasing computers for their companies. Experiment 1b introduces a new comparative mind-set manipulation involving

a managerial context of rating individual ads versus comparing pairs of ads. Performing this study among purchasing professionals recruited from the American Purchasing Society, we replicate the findings of experiment 1a. In experiment 2, we recruit advertising managers from an advertising agency and employ the same comparative mind-set manipulation as in experiment 1b. We find that a comparative mind-set increases accessibility of which-to-choose procedures and decreases accessibility of whether-to-choose procedures. In experiment 3, we recruit marketing professionals from the American Marketing Association and observe that the comparative mind-set increases choice in making marketing decisions. Moreover, this effect is mediated by the reduced impact of negative attribute information when managers integrate product information to make the decisions (i.e., attaching less importance to negative attributes). We also rule out the alternative process that attention to positive or negative attributes at the information search stage mediates the comparative mind-set effect. Finally, the comparative mind-set also increases managers' spending levels.

We then turn to our three quasi-experiments to offer stronger external validity. We use presidential election years as a natural manipulation of a comparative mind-set because the frequency with which political candidates are compared in these years should increase the accessibility of the comparative procedure central to our expected effect. Our first quasi-experiment examines MBA and EMBA students' advertising decisions while playing a business strategy simulation and shows that players spend more money on advertising and marketing during presidential elections years. Our second quasi-experiment establishes the effect of the comparative mind-set on advertising expenditures and selling, general, and administrative (SG&A) spending levels in firms headquartered in the U.S. relative to a control group of firms headquartered outside the U.S. Our third quasi-experiment examines two sets of boundary

conditions associated with decision scrutiny and strength of knowledge accessibility. Alternative explanations are ruled out and the effect is found to be robust to alternative formulations of our measures and models.

THEORETICAL BACKGROUND

The Effect of Behavioral Mind-Sets

A *behavioral mind-set* reflects the activation and persistence of a cognitive procedure. Performing one cognitive operation or motor behavior can activate a behavioral mind-set, which increases the likelihood of performing a similar operation or behavior in subsequent, even unrelated, situations (e.g., Wyer and Xu 2010; Wyer, Xu, and Shen 2012). For example, activating an abstract (concrete) mind-set in one task directs people to focus on the long-term (short-term) benefits of a different activity (Freitas, Gollwitzer, and Trope 2004) and disposes people to construe a situation in terms of abstract values (Torelli and Kaikati 2009).

Research has found that mind-sets influence search, evaluation, and decision making. In search, for example, Levav, Reinholtz, and Lin (2012) show that ordering decisions by increasing choice-set size (e.g., 5 options for the first decision, 10 for the second ... 50 for the tenth decision) activates a maximizing mind-set, which increases information search depth. However, ordering the decisions by decreasing choice-set size activates a satisficing mind-set, which decreases search depth. Other behavioral mind-sets involving mental simulation of alternatives (Hirt, Kardes, and Markman 2004), bolstering/counter-arguing (Xu and Wyer 2012), and relational/independent processing (Kim and Meyers-Levy 2008) impact evaluation.

In the decision making area, Gollwitzer and his colleagues (Gollwitzer and Bayer 1999; Gollwitzer, Heckhausen, and Steller 1990) suggest that a decision regarding whether or not to pursue a particular goal requires an evaluation of its pros and cons and, therefore, induces a

deliberative mind-set. In contrast, considering the sequence of actions necessary to attain a chosen goal activates an implemental mind-set. Once activated, these mind-sets change the way people process information in the subsequent situation (Gollwitzer et al. 1990). Extending these findings, Dhar, Huber, and Khan (2007) begin with the assumption that consumers approach a purchase situation with a deliberative mind-set. However, they find that making an initial purchase can shift consumers' mind-sets from the deliberative mode to the implemental mode. Consequently, those who make a purchase in the initial situation are more likely to develop the momentum to purchase again in the next situation.

The Comparative Mind-Set in Consumer Decision Making

Xu and Wyer (2007, 2008) document a *comparative mind-set* effect among consumers which refers to a state in which consumers make comparisons between options. This mind-set has been found to increase the likelihood of making subsequent related or unrelated choices. The authors show in a series of studies that making preference judgments about two alternative options in one product domain (e.g., computers) increases not only the likelihood that participants chose to buy the preferred option (rather than deferring choice), but also the likelihood of making a purchase in a different product domain (e.g., vacation packages).

To account for this effect, Xu and Wyer theorize that consumers engage in three information processing steps when making a purchase decision. First, consumers consider whether any of the options available may be above the threshold of acceptance (i.e., deciding whether-to-buy). If yes, in the second step, they compare the options that have met the whether-to-buy criteria, and decide which option is most preferred (i.e., deciding which-to-buy). Third, they decide how to implement the purchase. These three steps are often pursued sequentially. However, making preference judgments about related or unrelated products in a prior situation is

theorized to increase the accessibility of the second, “which-to-buy,” step. Consequently, instead of first evaluating whether any options are good enough, consumers already in this comparative mind-set approach the second decision by jumping to consider which option is preferred. Once they have determined their preferences, consumers are more likely to move on to purchase implementation rather than go back to the earlier step of deciding whether-to-buy at all.

This and subsequent research vary both the content and format of comparative judgments to test the generalizability of this mind-set effect. Results show that comparative judgments such as preference judgments (e.g., “Which do you prefer, dragonflies or butterflies?”) and similarity judgments (e.g., “How similar is Korea to China?”) give rise to a comparative mind-set that subsequently increases consumers’ choice likelihood in unrelated contexts.

In the current research, we aim to enrich the theory of comparative mind-set effect by exploring the underlying processes, identifying the moderating factors that either strengthen or weaken this effect, and extending the effect on choice to other types of decisions such as spending level decisions. We accomplish these goals in the context of investigating how comparative mind-set influences managerial decision making.

CONCEPTUALIZATION

The Effect of the Comparative Mind-set on Manager Choice

Although consumer decision making and manager decision making occur in different contexts and may vary in the scope of their implications, there are two important similarities. First, previous research shows that managers, like consumers, experience decision biases. In terms of specific biases, Roggeveen and Johar (2004) find that managers fall prey to an evoked-range bias when integrating discrepant sales forecasts. Glazer, Steckel, and Winer (1992)

observe that the mere presence of irrelevant information has dysfunctional effects on manager's decision making. Eisenstein and Hutchinson (2006) observe that managers learn less accurately about choice options further from goals when they are asked to focus on their goals. Bolton (2003) finds that priors generated through nonanalytic (vs. analytic) thinking are stickier in new product forecasting, and hence induce more bias. In general, research finds that manager overconfidence reduces decision accuracy (Mahajan 1992) and that manager use of decision models is effective only when the market is simple and dynamics are limited (Chakravarti, Mitchell and Staelin 1979, 1981).

A second similarity, which is more essential for the current research, is that managers, like consumers, often utilize a general decision making process involving deciding whether-to-choose, which-to-choose, and how-to-implement-the-choice. For example, when a major competitor successfully launches a marketing campaign, managers need to decide whether or not they should launch a counter-campaign, which media they should use, and how to implement the plan. Likewise, managers need to decide whether or not they will add to their product lines, which offerings to expand, and how to implement the strategy (partnerships, licensing, or acquisition).

Given evidence of decision biases and a similar decision process, despite the fact that managers' decisions may be more important and have broader financial implications, we expect that managers will, like consumers, also exhibit a greater likelihood of choosing when they have acquired a comparative mind-set. We predict:

H1: Activating a comparative mind-set increases managers' choice likelihood.

The Effect of the Comparative Mind-set on Manager Choice – Underlying Mechanisms

Theory on behavioral mind-set argues that it reflects the activation and persistence of cognitive procedures (Wyer and Xu 2010). In the current research, if making comparative judgments in one domain activates a comparative mind-set, it should change the accessibility of whether-to-choose and which-to-choose procedures. To formalize this hypothesis:

H2: Activating a comparative mind-set increases the accessibility of which-to-choose procedures and decreases the accessibility of whether-to-choose procedures.

We expect that the decreased accessibility of whether-to-choose procedures and increased accessibility of which-to-choose procedures will influence how managers process information to make decisions. Specifically, following Dhar and Nowlis (2004), we propose that when managers decide whether-to-choose, they form separate evaluations of each individual option across attributes in order to uncover if it meets minimum thresholds of acceptance. Previous research on the negativity bias shows that when people combine information to form overall evaluations of people or products, negative information tends to have a greater impact than positive information (Ahluwalia 2002; Kanouse 1984; Skowronski and Carlston 1989). Although several theories have been proposed to account for the negativity bias, the most acceptable explanation is that negative information is considered to be more diagnostic than positive information for categorizing targets into evaluative categories (Ahluwalia 2002; Herr, Kardes, and Kim 1991). Consistent with these findings, we propose that when managers decide whether to choose an option, which involves determining whether it meets a threshold of acceptance, negative attributes will have greater impact than positive attributes because negative information is more diagnostic for categorizing whether an option is good or bad.

In contrast, if the comparative mind-set disposes managers to consider which-to-choose without deciding whether-to-choose, managers would compare the attributes of alternative options in order to decide which option has more superior attributes (Mantel and Kardes 1999;

Nowlis and Simonson 1997). In this process, positive attributes and negative attributes would be treated more equally because the categorization process leading to negativity bias is absent. We predict:

H3: Activating a comparative mind-set decreases the impact of negative product attributes in the decision making process, which increases choice likelihood.

Although the most accepted explanation for negativity bias is that negative information is perceived as more informative and diagnostic than positive information and thus receives greater weight at the information integration stage (Ahluwalia 2002; Herr et al. 1991; Skowronski and Carlston 1989), some evidence shows that the negativity bias may also occur because individuals may pay more attention to negative attributes at the information search stage (Fiske 1980). We propose that the comparative mind-set decreases the weight of negative information during information integration for the following reasons.

First, the comparative mind-set directly changes the procedures employed to utilize information to make decisions, not the procedures used to search for information. Second, Xu and Wyer (2008, experiment 1) make an initial assessment of the role of attention by manipulating a comparative mind-set and attention to either positive or negative product attributes. Their results show that the comparative mind-set increases choice in the subsequent situation, regardless of manipulated attention to positive or negative attributes. These results provide preliminary evidence that attention to positive or negative attributes is not driving the comparative mind-set effect. However, to completely exclude the role of attention, it is necessary to manipulate comparative mind-set and show that (a) compared to the control condition, the comparative mind-set does not alter attention to positive or negative attributes and (b) attention to positive or negative attributes does not mediate the influence of the comparative mind-set effect on choice. We predict:

H4: Activating a comparative mind-set increases choice likelihood by decreasing the importance of negative attributes during information integration, not by decreasing attention to negative attributes during information search.

The Effect of the Comparative Mind-set on Manager Spending Level

Previous research on the comparative mind-set effect assumes that consumers make purchase decisions through three steps—whether-to-choose, which-to-choose, and implement-the-choice. Under this framework, choice has a deterministic effect on how much money consumers will spend on the chosen product (assuming consumers do not purchase multiple items). However, in a managerial context, the decision to choose a strategic action is often followed by the decision about how much to spend on the chosen strategy. For example, if a manager decides to advertise a product, she must then decide how much to spend on advertising. The weights that managers attach to positive and negative attributes are important inputs for this spending level decision. Given the comparative mind-set reduces the impact of negative attribute information, we expect that it will not only increase choice but also spending level. We predict:

H5: Activating a comparative mind-set increases managers' spending levels.

EXPERIMENTS

Experiment 1a: The Effect of the Comparative Mind-Set on Choice

Subjects and procedures. One hundred seven Executive MBA students from a southeastern university participated in this study as part of a classroom exercise. These participants had an average of 13.95 years of work experience ($SD = 5.71$) and most worked as senior managers as a precondition for EMBA admission. Half of the participants were randomly assigned into the *comparative mind-set* condition, in which they stated a preference for one animal from twenty pairs of animals (elephants vs. hippos, kangaroos vs. zebras, etc.). The other

half were randomly assigned into the *control* condition, in which they first solved twenty word-completion problems about animal names (e.g., ___ u ___ er ___ ly, H ___ p ___ __, etc.).

Upon completion of the first task, both groups were introduced to an ostensibly unrelated managerial decision making task, described as “to understand how managers make decisions based on incomplete information.” Participants were asked to imagine that they were the Vice President for a company and were planning to buy computers for their company. Information on two models of computers (adapted from Xu and Wyer 2007) was provided. Each computer was described by six attributes—two positive attributes, two negative attributes, and two neutral attributes. Based on the information provided, participants decided whether they would “Purchase Computer A,” “Purchase Computer B,” or “Defer Choice and Search for Other Models.” Finally, all participants reported their length of work experience and demographic information such as gender and ethnicity.

Results. Choice was coded as “1” if participants chose either “Purchase Computer A” or “Purchase Computer B” and coded as “0” if they decided to “Defer Choice and Search for Other Models.” This dependent variable was analyzed as a function of mind-set in a binary logistic regression analysis. Responses from two participants were not included in the analysis due to missing data. Compared to the control condition and controlling for the manager’s length of work experience, participants in the comparative mind-set condition had an increased likelihood of purchasing either of the two computers ($M_{\text{comparative}} = 52\%$ vs. $M_{\text{control}} = 33\%$) and the difference is significant (Wald $\chi^2(1) = 3.85, p = 0.05$) (see Table 1). These results support H1. There are two weaknesses in this study—the comparative mind-set manipulation is not managerially-relevant and the sample of EMBA students does not necessarily have expertise in the purchasing task. Experiment 1b replicates experiment 1a’s finding resolving these concerns.

[Insert Table 1 here]

Experiment 1b: The Effect of the Comparative Mind-set on Choice

Subjects and procedures. Seventy-one purchasing professionals were recruited from the American Purchasing Society to participate in this study. They were entered into a drawing for an iPad or its cash equivalent for participation. They had an average of 26.47 years of work experience (SD = 11.63) with an average of 16.62 years of work experience in the field of purchasing (SD = 11.16). We also collected information about the highest position obtained in their career (1 = Entry level; 10 = President/CEO level), which has a mean level of 5.77 (SD = 2.34). We control for both work position level and work experience level in our analysis.

Participants were sent a link in an email inviting them to participate in the study. Clicking the link, participants took part in an “advertising study” and were asked to evaluate a series of ads. Half of them were randomly assigned to the *comparative mind-set* condition, which involved comparing six pairs of ads. In each case, two ads (ad A and ad B) were presented side by side and participants were asked to compare the ads on a range of criteria (e.g., “Which ad has the more attractive model?” or “Which picture is more likely to prompt consumers to click through on a website?”). The sequence of ads was randomized. The measures were 11-point scales anchored by “Definitely Ad A” on one end and by “Definitely Ad B” on the other end. The other half of the participants were randomly assigned to a *control* condition, in which they rated six ads individually on the same criteria (e.g., “How attractive is the model in this ad?” or “Is this picture likely to prompt the consumer to click through on a website?”). In each case, one of the two ads from a pair was randomly selected and the ad sequence was randomized. The measures were 11-point scales anchored by “Not at all” and “Very.”

After completing the advertising study, participants were introduced to a similar decision making study as in Experiment 1a involving a computer purchase,¹ and were asked to decide whether they would “Purchase Computer A,” “Purchase Computer B,” or “Defer Choice and Search for Other Models.” Finally, all participants provided information related to their work experience (e.g., years in total, years in purchasing, highest position obtained in career) and demographic information such as gender and ethnicity.

Results. Responses from two participants were not included in the analysis due to missing data. Replicating the results of experiment 1a and supporting H1, participants in the comparative mind-set condition were more likely to purchase one of the two computers compared to control participants ($M_{\text{comparative}} = 35\%$ vs. $M_{\text{control}} = 14\%$, Wald $\chi^2(1) = 3.88$, $p < 0.05$), controlling for the manager’s years of work experience and position level (see Table 1). Neither work experience nor position level influenced choice. In the next experiment, we test the influence of a comparative mind-set on the accessibility of whether-to-choose and which-to-choose procedures, which should be reflected in the thoughts that managers generate in the subsequent decision situation.

Experiment 2: The Effect of the Comparative Mind-Set on Procedure Accessibility

Subjects and procedures. Forty-eight participants recruited from an advertising agency completed the experiment in return for being entered into a drawing for an iPad or its cash equivalent. Participants had an average of 13.79 years of work experience ($SD = 9.26$) with an average of 11.25 years of work experience in the advertising industry ($SD = 8.28$).

¹ Pretests showed that purchasing managers had higher thresholds for making buying decisions. Hence, we modified the descriptions of the computer models used in experiment 1a to enhance their attractiveness. Specifically, the attributes of “Low CPU speed” for Computer A or “Low hard disk capacity” for Computer B were replaced by “Energy Consuming” and “Little Software Included” respectively.

Participants were sent a link in an email inviting them to participate in the study. Clicking the link, participants took part in an “advertising study” and were asked to evaluate a series of ads. Half were randomly assigned to the *comparative mind-set* condition, which involved comparing six pairs of ads. The other half were randomly assigned to the *control* condition, in which they rated six ads on the same criteria. After completing the advertising study, all participants were asked to play the role of Vice President of a computer manufacturing company and make next-year advertising spending decisions for the company. They were informed that they had an advertising budget and that they could choose the model to advertise and allocate the budget. Participants then read the descriptions of two computer models² that were ready to be sold next year. They were also informed that company had two models under development that could be introduced to the market next year. This information was added to ensure that participants did not feel undue pressure to advertise the current models and that the decision about whether or not to advertise the current models at all was an important decision to make.

Based on the above information, participants were asked to describe how they would approach making the decision to develop their company’s advertising plan. Space was provided for a written response. Finally, all participants provided information related to their work experience (e.g., years in total, years in the advertising industry, the level of highest position obtained in their career), and reported demographic information such as gender and ethnicity.

Results. We predicted that activating a comparative mind-set would increase the accessibility of “which-to-choose” procedures and decrease the accessibility of “whether-to-choose” procedures. The accessibility of different cognitive procedures was reflected in the type

² We modified the descriptions of the two computer models used in experiments 1a because some attributes are more relevant to purchase decisions and less relevant to advertising decisions. For Computer A, “Payment after Delivery” was changed to “Fast Delivery” and “Recommended by friends” was replaced by “Energy Saving.” For Computer B, “Sold in a reputable store” was changed to “Standard Software Package Included.”

of thoughts participants generated when describing their decision processes. Given respondents offered an average of 2.33 thoughts ($SD = 1.17$), we coded whether or not participants generated thoughts in any of the three categories (whether-to-choose, which-to-choose, and how-much-to-spend). If thoughts reflected considerations in one category, a “1” was recorded and “0” otherwise. Two coders coded all thoughts and agreed 81.94% (Cohen’s Kappa = 0.63, $p < 0.001$).

- *Whether-to-choose* thoughts reflected participants’ considerations of whether or not they wanted to advertise the two models presented. Two types of “whether-to-choose” thoughts were identified. First, participants expressed a desire to collect more information before making the decision (e.g., “I’d also want to know the characteristics of the two new models coming out before making a decision on how to allocate the budget for each product”). Second, participants wrote that the current models were not attractive enough to be advertised (e.g., “...would do a brand campaign, not specific models—those features aren’t compelling or differentiating”) or the current models were attractive enough to be advertised (e.g., “...each model will be attractive to different folks”).
- *Which-to-choose* thoughts reflected comparison between the two models presented (e.g., “There’s nothing significantly better about Model B. Model A is more appealing for these reasons people care about...”; “While both have similar number of pros and cons, the pros in Model A offer more exciting options to advertise”).
- *How-much-to-spend* thoughts reflected consideration on how to allocate the budget (e.g., “I would allocate 30% of my budget to this model. The rest of my budget would go to model A...”).

The likelihood of generating thoughts in each category was analyzed as a function of the comparative mind-set manipulation, controlling for the manager’s years of work experience and position level. Results indicate that compared to control participants, those in the comparative mind-set condition were more likely to think about “which-to-choose” ($M_{\text{comparative}} = 61\%$ vs. $M_{\text{control}} = 32\%$, Wald $\chi^2(1) = 3.58$, $p < 0.06$), less likely to think about “whether-to-choose” ($M_{\text{comparative}} = 35\%$ vs. $M_{\text{control}} = 72\%$, Wald $\chi^2(1) = 6.62$, $p < 0.01$), and equally likely to think about “how-much-to-spend” ($M_{\text{comparative}} = 17\%$ vs. $M_{\text{control}} = 28\%$, Wald $\chi^2(1) = 0.77$, *ns*). We also calculated the number of thoughts in each category and analyzed the number as a function of mind-set. Results replicate using this approach.

Discussion. This study demonstrates that activating a comparative mind-set increased the accessibility of “which-to-choose” procedures and decreased the accessibility of “whether-to-choose” procedures. These results support H2. This is the first study showing that making comparative judgments in one domain can change the accessibilities of different procedures in making subsequent and unrelated decisions. Experiment 3 extends this idea by examining whether these changing accessibilities associated with the comparative mind-set reduce the impact of negative product attributes in the decision making process because they decrease the importance managers attach to negative attributes during the information integration stage (H3 and H4). We also assess the alternative process by examining whether a comparative mind-set influences managers’ attention to negative attributes.

Experiment 3: The Effect of the Comparative Mind-set on Choice and Spending Levels – Testing the Underlying Mechanism

Subjects and design. One hundred sixty-one marketing professionals were recruited from a local chapter of the American Marketing Association to participate in this study and were entered into a drawing for an iPad or its cash equivalent for participation. They were randomly assigned to one of four conditions in a 2 (mind-set: comparative vs. control) x 2 (process condition: attribute importance rating vs. attribute recall) between-subjects design. Participants had an average of 19.23 years of work experience (SD = 8.40) with an average of 13.89 years of work experience in the field of marketing (SD = 7.72). We also collected information about the highest position obtained in their career (1 = Entry level to 10 = President/CEO level), which has a mean level of 6.78 (SD = 2.24). We control for both work position level and work experience level in our analysis.

Procedure. Participants were sent a link in an email inviting them to participate in the study. As in experiment 2, participants were randomly assigned to either the *comparative mind-*

set condition, which involved comparing six pairs of ads, or the *control* condition, in which they rated six ads on the same criteria. After completing this advertising survey, all participants were asked to play the role of a marketing manager of a computer manufacturing company and make marketing plans for the company for the entire next year. They were informed that they had a budget to market products and that they could choose the model they wanted to market and allocate the budget. As in Experiment 2, participants were informed that two computer models were ready to be sold next year and that two different computer models under development could be introduced next year. They then read the attribute descriptions of two computer models that were ready to be sold next year. Each model was described by two positive attributes, two negative attributes, and two neutral attributes.³ Based on the computer descriptions information, participants decided among three options: “Market Model A,” “Market Model B,” or “Market Neither Model and Save the Budget.”

Contingent on their choice, participants were directed to different questions. Those who chose to market Models A or B were asked to decide what percent of their marketing budget they would like to spend and what percent they would like to save. Participants who decided to market neither model and save the entire budget did not make this spending level decision.

After making the choice and spending level decisions, participants were randomly assigned to one of two process conditions that would allow us to determine whether the comparative mind-set would influence decision making during the integration stage or the attention stage. We used an *attribute importance rating* condition (N = 73) to assess the impact

³ These positive (+), negative (-), and neutral (o) attributes were selected based on a pretest among a separate sample of managers from the same population. Model A was described by High RAM (+), Installment Payment Available (o), Low Monitor Display Resolution (-), Good Post Purchase Repair Service (+), Multiple Colors Available (o), and Low CPU Speed (-). Model B was described by Two-year Warranty with No Extra Cost (+), Low Hard Disk Capacity (-), Standard Software Package Included (o), Stable Operation (+), Poor Sound Quality (-), and Keyboard with New Design (o).

during the integration stage. In this condition, participants were asked to rate the importance of twelve different product features (presented in a randomized order) in making their decisions (0 = Not at all important, 10 = Extremely important). We used an *attribute recall* condition (N = 85) to assess the impact during the attention stage. In this condition, participants were asked to free-recall features of the computers they had just examined. They were instructed to recall as many features as possible and as accurately as possible but did not need to indicate whether a particular feature recalled belonged to Model A or B. To form our measures, in the *attribute importance rating* condition, we averaged the importance ratings of the four positive attributes, four negative attributes, and four neutral attributes to generate an average negative, positive, and neutral attribute rating score. In the *attribute recall condition*, we counted the number of positive attributes recalled, the number of negative attributes recalled, and the number of neutral attributes recalled. Finally, all participants provided information related to their work experience described earlier and demographic information such as gender and ethnicity.

Results. Choice. Choice was coded as “1” if participants chose either “Market Model A” or “Market Model B,” and coded as “0” if participants decided to “Market Neither Model and Save the Budget.” Choice was analyzed as a function of mind-set in a binary logistic regression analysis, controlling for the years of work experience and work position level. Responses from three participants were not included in the analysis due to missing data. Results reveal that participants in the comparative mind-set condition were more likely to choose to market one of the models ($M_{\text{comparative}} = 57\%$) than control participants ($M_{\text{control}} = 34\%$), Wald $\chi^2(1) = 8.81$, $p < 0.005$ (see Table 2). These results support H1. In addition, participants were also less likely to choose if they had more work experience (Wald $\chi^2(1) = 4.21$, $p < 0.05$).

[Insert Table 2 here]

Spending levels. Participants who decided to market one of the two computer models indicated the percentage of their budget they would spend. By default, participants who chose to advertise neither model allocated zero percent of the budget into the spending account. We perform two tests with the second more definitive. First, the percentage of budget allocated into the spending account was analyzed as a function of comparative mind-set, controlling for years of work experience and work position level. Participants in the comparative mind-set condition committed a greater percentage of the budget to spending ($M_{\text{comparative}} = 33.96\%$) than those in the control condition ($M_{\text{control}} = 15.47\%$), $F(1, 154) = 15.47, p < 0.001$. A second test involves comparing spending levels conditioned on choice. Specifically, among participants who chose to market one of the computer models ($N = 73$), those in the comparative mind-set condition spent more of the budget ($M_{\text{comparative}} = 59.87\%$) than their counterparts in the control condition ($M_{\text{control}} = 44.81\%$), $F(1, 69) = 7.47, p < 0.01$. These results support H5.

Attribute importance ratings. Importance of positive attributes and importance of negative attributes were analyzed as a function of attribute valence and comparative mind-set in a repeated-measure MANOVA, controlling for work experience and work position level. The mind-set by attribute valence interaction on importance ratings was significant, $F(1, 71) = 4.25, p < 0.05$. Additional analyses showed that in the control condition, participants rated negative attributes as more important than positive attributes, ($M_{\text{positive}} = 6.85$ vs. $M_{\text{negative}} = 7.86$; $F(1, 34) = 4.62, p < 0.04$), which is consistent with the negativity bias. However, in the comparative mind-set condition, positive attributes and negative attributes were rated as equally important ($M_{\text{positive}} = 6.83$ vs. $M_{\text{negative}} = 6.43$; $F(1, 37) = 0.65, ns$).

A more critical test of H5 is to analyze the simple effect of comparative mind-set on importance of negative attributes. Results indicate that the comparative mind-set decreases the

importance of negative attributes ($M_{\text{comparative}} = 6.43$ vs. $M_{\text{control}} = 7.86$; $F(1, 69) = 6.23, p < 0.02$). Additional analysis revealed that the comparative mind-set influences neither the importance of positive attributes ($M_{\text{comparative}} = 6.83$ vs. $M_{\text{control}} = 6.85$; $F(1, 69) = 0.01, ns$) nor the importance of neutral attributes ($M_{\text{comparative}} = 5.02$ vs. $M_{\text{control}} = 5.58$; $F(1, 69) = 1.23, ns$).

Attribute recall. Recall of positive or negative attributes was analyzed as a function of attribute valence and comparative mind-set in a repeated-measure MANOVA, controlling for work experience and position level. The main effect of attribute valence was significant, indicating participants recalled more negative attributes than positive attributes ($M_{\text{positive}} = 1.37$ vs. $M_{\text{negative}} = 1.75, F(1, 83) = 9.01, p < 0.005$). Neither the main effect of comparative mind-set, nor the comparative mind-set by attribute valence interaction on recall was significant. Therefore, although participants paid greater attention to negative attributes than positive attributes, this allocation of attention did not interact with the comparative mind-set to predict recall. Therefore, it is unlikely that attention during information search drives the comparative mind-set effect. Subsequent mediation analyses will provide further evidence for this conclusion.

Mediation analysis. H4 predicts that activating a comparative mind-set increases choice likelihood by decreasing the importance of negative attributes during information integration. We test the mediating role of the negative attribute importance ratings in three steps. First, together with our two control variables, we find that the comparative mind-set decreases the importance of negative attributes in decision making ($M_{\text{comparative}} = 6.43$ vs. $M_{\text{control}} = 7.86$; $b = -1.43, SE = 0.57, t = -2.50, p < 0.02$) and increases choice likelihood ($M_{\text{comparative}} = 55\%$ vs. $M_{\text{control}} = 31\%$; $b = 1.06, SE = 0.51, \text{Wald } \chi^2(1) = 4.36, p < 0.04$). Second, the importance of negative attributes also affects choice likelihood ($b = -0.62, SE = 0.16, \text{Wald } \chi^2(1) = 15.00, p < 0.001$). Third, the influence of the comparative mind-set manipulation on computer choice becomes

insignificant when including the negative attribute importance mediator into the model ($b = 0.51$, $SE = 0.62$, $Wald \chi^2 (1) = 0.69$, ns). A bootstrap test confirmed the mediating role of negative thinking (95% Bias Corrected Confidence-Interval using 5000 bootstrap samples: 0.206 to 2.143; Preacher and Hayes 2004, 2008).

We repeated the above procedure using the number of negative attributes recalled as the mediator. Results indicate that although comparative mind-set increases the likelihood of choice ($M_{\text{comparative}} = 59\%$ vs. $M_{\text{control}} = 36\%$; $b = 0.98$, $SE = 0.47$, $Wald \chi^2 (1) = 4.38$, $p < 0.04$), it does not influence the recall of negative attributes ($M_{\text{comparative}} = 1.74$ vs. $M_{\text{control}} = 1.76$; $b = -0.02$, $SE = 0.23$, $t = -0.09$, ns). Furthermore, including recall of negative attribute mediator in the model does not attenuate the effect of comparative mind-set on choice. Instead, the effect remains positive and significant ($b = 1.00$, $SE = 0.47$, $Wald \chi^2 (1) = 4.42$, $p < 0.04$).

Discussion. This study provides evidence that a comparative mind-set not only increases choice, but also enhances spending levels. Furthermore, in terms of process, the influence of a comparative mind-set on choice is mediated by decreased impact of negative attributes because managers attach less importance to negative attribute information at the information integration stage. However, the comparative mind-set does not influence the amount of attention that managers pay to negative attributes during information search. These results support H4. Furthermore, the finding that comparative mind-set does not influence attention to positive or negative attributes is consistent with the conclusions of experiment 1 in Xu and Wyer (2008), showing that the comparative mind-set effect is independent of manipulated selective attention to positive or negative attributes in a prior task. Based on our findings, we conclude that selective attention to positive or negative attributes does not account for the comparative mind-set effect.

FACTORS MODERATING THE COMPARATIVE MIND-SET EFFECT

Consumer research has tended to focus on the generalizability of the comparative mind-set effect. As such, the boundary conditions of this effect have been explored less. As one example, Xu and Wyer (2007) show that the comparative mind-set effect is more pronounced if choice options are more attractive than if choice options are perceived to be less attractive. The reason is that when available options are unattractive and the choice deferral option is available, participants are reluctant to choose one of the options even after comparison.

We examine three factors that we theorize should influence the likelihood of observing the comparative mind-set effect among marketing managers making advertising decisions. Two of these factors—recession and firm profit—influence the scrutiny of making marketing spending decisions. The other factor—strength of mind-set activation—reflects the strength of the comparative mind-set due to knowledge accessibility.

Given we are not in the lab, we seek a surrogate for the comparative mind-set that occurs naturally in the field. General principles of behavioral mind-set effects indicate that the likelihood that a procedure is activated and applied in the subsequent situation depends on both the *recency* with which it has been used in the past and the *frequency* with which this procedure have been applied in the past (Wyer and Xu 2010; see Bargh, Bond, Lombardi, and Tota 1986). Our lab experiments show that a comparative mind-set can be activated and become temporarily accessible by making recent comparative judgments. However, given our inability to control the timing of measurement, which is an essential feature of quasi-experiments, it was necessary to utilize the frequency mechanism. Specifically, following Xu and Wyer's (2008) exploratory investigation,⁴ we selected presidential election years as a context that stimulates a comparative

⁴ Xu and Wyer (2008) offered two preliminary descriptive analysis showing that: (1) the average U.S. personal-consumption expenditure (1929-2002) during presidential-election years was 2.2% greater than in the years

mind-set among managers. Specifically, in presidential election years, the primaries and general election create an intensive period of comparing candidates that gives rise to a comparative mind-set. Because managers are frequently exposed to political campaign information in election years (due to political ads, debates, and news coverage), the comparative judgment procedures become chronically accessible in that year. Therefore, managers may be subject to the influence of a comparative mind-set in making managerial decisions in this time period.

The Impact of Decision Scrutiny

In a consumer decision context, the value of options is determined by trading off quality perceptions (i.e., benefit) against the monetary sacrifice associated with acquiring a product (i.e., cost) (Monroe 1990; Rao and Sieben 1992). This benefit/cost tradeoff also occurs in managerial decisions. *Decision scrutiny* is defined as the extent to which people make cost/benefit tradeoffs. At the low end of the decision-scrutiny continuum, people make fewer cost/benefit tradeoffs and instead focus their attention on the benefits associated with alternative courses of action. Given costs are not heavily evaluated, more financial resources may be used to take the chosen course of action. In contrast, at the high end of the decision-scrutiny continuum, people make serious benefit/cost tradeoffs and pay more attention to the cost of implementing the decision.

Decisions made by managers for their companies can receive more or less scrutiny depending on several factors. One factor is the level of uncertainty in the macro-environment. Specifically, we investigate the impact of a *recession* on the comparative mind-set effect. Recent research indicates that recessions can prompt both increases and decreases in firm advertising allocations (Srinivasan, Lilien, and Sridhar 2011). However, this research shows that decreases occur approximately six times more frequently than increases (35% of firms decrease vs. 6% of

immediately before and after and (2) the total retail-store sales (1953-2000) were 9.4% higher during the three months prior to election in election years than during comparable periods during non-election years.

firms increase) during a recession, confirming other practitioner research on this topic (Ryan 1991). The scrutiny that we believe accompanies this decrease is also expected to moderate the comparative mind-set effect. Specifically, marketing managers are likely to be worried that investments in acquiring and retaining customers will not pay off during a recession. Furthermore, budgets are likely to be tight during recessions, thereby increasing the level of scrutiny for the advertising decision. Given this increased level of scrutiny, we expect the comparative mind-set effect to be weakened. We predict:

H6: The influence of the comparative mind-set on firm marketing spending is weakened when the economy is in a recession.

A second factor affecting decision scrutiny is the *firm's profit level*. As the level of firm profits increases, we expect decision scrutiny to decrease and marketing managers to pay less attention to the cost of their advertising spending decisions. Consistent with this, research shows that when firms have high levels of profits, marketing managers generate lower returns from marketing actions (Slotegraaf, Moorman, and Inman 2003). Consistent with our explanation, this may happen because marketers attend less carefully to the costs of their decisions. Given this, we expect that higher company profits will amplify the comparative mind-set effect. We predict:

H7: The influence of the comparative mind-set on firm marketing spending is amplified when the manager's company has higher profits.

The Impact of Knowledge Accessibility

Activation and persistence of a mind-set is governed by general rules of knowledge accessibility. One important rule is that the frequency with which a cognitive procedure has been used will determine the strength of a mind-set (Bargh et al. 1986; Förster and Liberman 2007; Wyer and Xu 2010). In this paper, we examine the hypothesis that making comparative judgments more frequently will magnify the strength of the comparative mind-set.

We do so by suggesting that the intensity of political campaign activities varies from state to state depending on whether or not the state is a *swing state*, defined as the degree to which it has a historical tendency to shift its vote toward Republican and Democratic candidates. Due to the winner-take-all nature of the U.S. Electoral College, there is less incentive for candidates to spend time and resources in a safe state. Instead, political candidates spend more time and resources on swing states that are contestable (Edwards 2011). Therefore, political campaign activities, including political advertising, should be more intensive in swing states.⁵ As a result, the influence of the comparative mind-set due to the presidential election on marketing managers' advertising spending decisions should be strongest for decision-makers of firms located in swing states. We predict:

H8: The influence of the comparative mind-set on firm marketing spending is amplified when the manager's company is located in a swing state.

QUASI-EXPERIMENTS

We used three quasi-experiments to test four predictions. All three quasi-experiments used the presidential election as our operationalization of the comparative mind-set. This exogenous event was expected to affect firm spending. Given we studied actual firm marketing spending, we first replicated H5 which predicts that activating a comparative mind-set increases spending levels. We then tested H6-H8 which propose a set of moderating factors that amplify or attenuate the comparative mind-set effect. Because some factors are not relevant across all studies, we tested those predictors relevant to each context. Web Appendix A contains an experiment that tested the assumption that exposure to political campaigns prompts people to

⁵ We performed a separate test to support this assumption.

make comparative judgments. Web Appendix B contains our robustness checks and Web Appendix C offers tests of alternative explanations.

Quasi-Experiment 1: Business Simulation Study

Subjects and procedures. Our first quasi-experiment involved observing teams of EMBA and MBA students making decisions while playing StratSim, a business strategy simulation. The unit of analysis for this study is the team of students managing one of five firms (Firms A-E) that compete in an industry. The starting characteristics of Firms A-E did not change over the course of our data collection and we used firm-specific indicators to control for firm heterogeneity. At the beginning of each period, teams reviewed their financial performance from the prior quarter and submitted a series of spending decisions for the following quarter. We focused on two types of marketing spending activities in the game—advertising expenditures and other marketing expenditures. Advertising expenditures involve all spending to use media to promote the firm’s products while other marketing expenditures involve spending on corporate advertising, public relations, product advertising and promotions, and sales force compensations. Both expenditures are discretionary in nature and may be subject to the comparative mind-set.

In 2005, one of the authors purchased three years of data (2002-2004) from Interpretive Software for their business simulation game StratSim. We attempted to acquire data from 2005 to the present in 2013, but the data were no longer available. Given this, we utilized the three years of data available with a focus on comparisons between non-election years (2002 and 2003) and the election year (2004).

Advertising decisions were made during each period at the product level by firms in different industries. The data included 194 industries across all universities that played the simulation. However, 3 universities playing in 9 industries were outside the U.S. and were

eliminated from the sample because these teams would not be exposed to the U.S. presidential election. We also eliminated 6 undergraduate universities playing in 92 different industries in order to keep the focus on managers with experience. This left a sample of 93 industries, each of which includes five firms that made up to 11 periods of advertising decisions.

Advertising decisions were made at the product level and products varied across the course of the game as firms added and deleted products from their portfolios. Each firm began with two products and the average throughout the sample was 4.81 products ($SD = 2.15$) per firm. The resulting sample of teams across 20 universities in 93 different industries reflected 2,662 industry-firm-product combinations and 18,186 industry-firm-product-period observations. Because we included 2 years of lagged values for advertising (to deal with the dynamic nature of advertising spending) and a one-year lagged value for profitability (as a predictor in H7) as independent variables, we lost 359 observations and our final sample was 2,303 industry-firm-product combinations and 12,868 industry-firm-product-period observations.

Measures and Modeling Approach. We focused on the main effect of election as a test of the main effect of the comparative mind-set in H5. A recession occurs in the middle of the simulation, so we were able to determine whether or not this mitigates the comparative mind-set as predicted in H6. Likewise, we have information about firm profits in each period, which allowed us to examine whether profits exacerbate the comparative mind-set as predicted in H7. We were not able to test the moderating effect of swing-state status (H8) given that universities from only 16 states played the stimulation and that many EMBA students travel to different states to get their MBAs which made determining their exposure to the comparative mind-set due to presidential election advertising difficult. We controlled for whether the team playing was comprised of EMBA or MBA students with an *EMBA* dummy variable. Finally, given schools

only play in certain year-quarter, the effect of elections became time-invariant and dropped out in a fixed-effect model. As a result, we instead used a random-effect panel model. Our model is:

$$\begin{aligned} Spend_{ifpt} = & \alpha_0 + \alpha_1 * Spend_{ifpt-1} + \alpha_2 * Spend_{ifpt-2} + \alpha_3 * Election_i + \alpha_4 * Recession_t + \\ & \alpha_5 * Firm_Profit_{ift-1} + \alpha_6 * Trend_i + \alpha_7 * EMBA_i + \alpha_8 * Election_i * Recession_t + \\ & \alpha_9 * Election_i * Firm_Profit_{ift-1} + \alpha_{10} Firm_dummies_f + \alpha_{ifp} + v_{ifpt} \end{aligned} \quad (1)$$

where $Spend_{ifpt}$ is advertising or other marketing spending for product p of firm f in industry i in period t and $Spend_{ifpt-1}$ and $Spend_{ifpt-2}$ are one- and two-period lagged-spending values respectively, $Election_i$ is a dummy indicating whether industry i is played in presidential election year or not, $Firm_Profit_{ift-1}$ is profit margin of firm f in industry i in period $t-1$, $Trend_i$ is the year trend given teams played in different years, $Firm_dummies_f$ are included to control for firm heterogeneity, α_{ifp} is the industry-firm-product random effect, and v_{ifpt} is the error term, $v_{ifpt} \sim N(0, \sigma_{ifpt}^2)$.

Results. Results, shown in Table 3, indicated that the overall model fit was good (Wald $\chi^2(14) = 23515.94, p < 0.0001$) and the Durbin-Watson statistic close to 2 (DW = 1.720) indicated no first-order serial correlation. We observed a positive and significant effect for *Election* ($\alpha_3 = 10.703, p < 0.001$) on advertising spending, supporting H5. Consistent with H6, we observed that the comparative mind-set effect associated with the presidential election weakens during recession ($\alpha_8 = -4.967, p < 0.05$). However, the effect of the comparative mind-set strengthened when companies were more profitable ($\alpha_9 = 0.001, p < 0.001$), as predicted by H7. All results replicated for other marketing spending (see Table 3).

[Insert Table 3 here]

In terms of robustness checks, results replicated when we controlled for panel-specific first-order autocorrelation and heterogeneity in the error terms. Furthermore, given teams played the games in the spring, summer, or fall terms, we further explored the comparative mind-set

effect of election by including the interaction of *Election*Fall* (as well as a dummy variable for Fall) in the model. If comparative mind-set was induced by the election, this effect should be amplified when simulations were played during the fall in presidential years as this is when the election occurred. We found this was the case for advertising spending ($\alpha_{11} = 9.882, p < 0.001$) and for other marketing spending ($\alpha_{11} = 625.839, p < 0.001$). Given our quasi-experiment used a continuous dependent variable, we also replicated our result using a dependent variable similar to the choice/defer choice used to test H1 in our lab experiments.⁶

Quasi-Experiments 2 and 3: Secondary Data Studies

These two field studies used secondary data about firm marketing spending from a sample of U.S. companies exposed to the U.S. presidential election compared to a control group of non-U.S. companies. We tested H5 in this context. We then examined the three moderating conditions (H6-H8) among the sample of U.S. companies. Web Appendix B offers a series of robustness and Web Appendix C contains a refutation of alternative explanations of our results.

Samples. The sample of U.S. firms was drawn from TNS Media's Ad\$summary. Specifically, because our predictions ideally are tested with firms that make media buys over time, we focused on the Top 1000 Advertising Spenders listed in Ad\$summary. We began with the Top 1000 list in 2001 and then cross-listed this year with a list from the 1990s (1993) and the 1980s (1985), adding unique firms that appeared during these years. This produced a sample of 1907 unique firms. We then determined whether firms were public (necessary to observe advertising data in Compustat), which reduced the sample to 1003 unique firms. Then we searched for firm accounting information and excluded 84 firms with missing data in Compustat

⁶ To perform the choice replication, we create a measure that is the percentage change in advertising spending (marketing) from $t-1$ to t . Given median 14% (0%) and mean 7% (3%) percentage changes, we define 10% (5%) as the threshold for "choice" and create a dummy variable coded as 1 corresponding if a firm changes advertising (marketing) spending beyond +10% (5%) and 0 otherwise for "defer" option. Our results are quantitatively similar.

during the period 1950-2011. Given our desire for a panel structure, firms not on the list for at least four consecutive years (to include one presidential election) were dropped, which further reduced the sample to 870 unique firms. Next, we looked up each firm in Compustat and observed whether the firm's headquarters are or are not in the U.S. and found that 70 of the firms on the list had non-U.S. headquarters. We moved this group of firms to the International sample (see next section). Finally, we lost 21 firms due to missing information on company headquarter state ($n = 6$) and firm profitability ($n = 15$). For quasi-experiment 2, our final sample of public U.S. firms was an unbalanced panel of 779 firms and 18,090 firm-year observations.

Our control group was a sample of international firms operating during the same time period as our U.S. firms, but whose headquarters were based outside the U.S. This reduced managers' exposure to the U.S. presidential election⁷ and, hence, the comparative mind-set. We began with the sample of 70 international head-quartered firms from the Ad\$Summary sample. To provide a more definitive sample, we collected the population of international head-quartered firms from Compustat (1950-2011). These 361 firms were all headquartered outside the U.S. but are available in Compustat because they are traded as American Depository Receipt stock (ADR), which is stock that trades on U.S. exchanges, but represents shares in a foreign corporation. Companies traded as ADR stock are required to report financial information to banks, which is available in Compustat. As expected, the original 70 international head-quartered firms from the Ad\$Summary sample were completely nested in this sample. As with the U.S. sample, we dropped firms available for fewer than four sequential years, which reduced the sample to 345 firms. We further excluded firms with missing information on profitability and market capitalization. Our final sample of public international firms was an unbalanced panel of

⁷ Managers are exposed to presidential elections in their own countries, which have cycles that differ from the U.S. However, these elections occur randomly across companies in our sample and should not influence our results.

301 firms and 8072 firm-year observations. We took additional steps to more tightly match the U.S. and international firm samples in our robustness checks.

Data and measures. For each firm in our sample, we collected advertising dollars spent, company location, and profits for each year between the years 1950-2011 from Compustat.⁸ The voting history of each state was collected from http://www.archives.gov/federal-register/electoral-college/votes/votes_by_state.html.

Our dependent variable was *firm advertising level*, as measured by the company's reported advertising dollars spent in the year on advertising media (radio, television, newspapers, and periodicals) and promotional expenses. We also replicated our results using *firm selling, general, and administrative expenses* (SG&A). SG&A represents all commercial expenses of operations (including expenses not directly related to production) incurred in the regular course of business operation, such as corporate expenses, marketing expenses, administrative charges, etc. Prior studies have used SG&A as the proxy for firm spending on marketing, market research, sales efforts, trade expenses, and other related activities (e.g., Dutta et al. 1999).

Our hypothesized predictors include the following variables. *Presidential election year* was a dummy variable coded 1 for each of the fifteen years in our sample involving presidential election years (1952, 1956, 1960, 1964, 1968, 1972, 1976, 1980, 1984, 1988, 1992, 1996, 2000, 2004 and 2008) and 0 otherwise. *U.S. headquarters* was coded using the headquarter country code (LOC) in Compustat. Companies with U.S. headquarters were coded 1 and 0 if headquartered outside the U.S. In terms of moderators, *recession* is dummy coded 1 if the economy is in a recession that year. Following Srinivasan, Lilien, and Sridhar (2011), we used

⁸ Advertising dollars spent is also available in TNS Media. We used Compustat data instead because TNS Media could not be pulled electronically from the sources available to us. With the use of the firm's PERMNO, Compustat allowed us to pull electronically all of the advertising data for each company in each year in our sample.

the National Bureau of Economic Research's (NBER) declaration of recession. We classified a year as recession if the majority of the year occurs during an NBER-classified recession trough. We identified eight years in the sample that qualify as recessions and code these as 1 (1960, 1970, 1974, 1980, 1982, 1990, 2001 and 2008) and 0 otherwise. *Firm profit* was the firm's one-year lagged EBITDA—earnings before interest, taxes, depreciation, and amortization. Finally, *swing state* was calculated as the probability that the company's headquarter state changed its electoral college vote from one party to another (from Democrat to Republican or from Republican to Democrat) based on the past two elections. A time *trend* variable was included as a predictor given the importance of accounting for time in our forecasting model in general.

Modeling and estimation approach. Past literature indicates that advertising is dynamic in nature (e.g., Dekimpe and Hanssens 1995; Steenkamp et al. 2005). Therefore, we used a fixed-effect autoregressive panel-forecasting model (Arellano and Bond 1991). This estimator takes the first difference of the data to remove unobserved firm heterogeneity in order to produce consistent estimates. Serial correlation in the first-differenced errors at an order higher than one implies that the moment conditions are invalid and results are biased. Given this, we checked the second-order serial correlations in the error terms before interpreting the model estimates. We mean-centered all main effects in our model before forming interactions to improve our interpretation of the interaction effects.

For quasi-experiment 2, we expected to observe that managers will spend more on advertising in presidential election years due to a comparative mind-set effect relative to a control group. We estimated the following fixed-effect autoregressive panel-forecasting model:

$$Spend_{it} = \alpha_0 + \alpha_1 * Spend_{it-1} + \alpha_2 * Spend_{it-2} + \alpha_3 * Election_t + \alpha_4 * Election_t * U.S._Headquarter_i + \alpha_5 * Trend_t + \alpha_i + v_{it} \quad (2)$$

where $Spend_{it}$ is either advertising or SG&A spending for firm i in year t and $Spend_{it-1}$ and $Spend_{it-2}$ are one-year and two-year lagged spending values respectively, $Election_t$ is a dummy indicating whether year t is presidential election year or not, $U.S._{Headquarter}_i$ is the status of firm i 's headquarters, $Trend_t$ is the time trend, and α_i is the firm-specific fixed effect.

$U.S._{Headquarter}$ can, technically, vary over time. However, in our sample of firms, it did not. Therefore, given our use of a fixed-effect model, the main effect of $U.S._{Headquarter}$ dropped out of the model.

For quasi-experiment 3, we examined a set of moderating factors in the U.S. sample. Given this, we should observe a main effect of election in this sample. However, our focus in Model 3 was on the interactions reflecting our boundary condition predictions (H6-H8):

$$Spend_{it} = \alpha_0 + \alpha_1 * Spend_{it-1} + \alpha_2 * Election_t + \alpha_3 * Recession_t + \alpha_4 * Firm_Profit_{it-1} + \alpha_5 * Swing_State_{it} + \alpha_6 * Trend_t + \alpha_7 * Election_t * Recession_t + \alpha_8 * Election_t * Firm_Profit_{it-1} + \alpha_9 * Election_t * Swing_State_{it} + \alpha_i + v_{it} \quad (3)$$

as defined above and $Firm_Profit_{it-1}$ is firm i 's profits in year $t-1$ and $Swing_State_{it}$ is the probability firm i 's headquarter state “swings” between parties. Based on our predictions, our focus was on the two-way interactions of $Election * Recession$ associated with α_7 (H6), $Election * Firm_Profit$ associated with α_8 (H7), and the two-way interaction of $Election * Swing_State$ associated with α_9 (H8).

Quasi-experiment 2 results. Table 4 contains Model 2 results. The overall fit of the model was good: Wald $\chi^2(5) = 584.19, p < 0.001$. Our model is a good approximation of the data without serial correlations at the second-order of the error terms ($z = -1.219, ns$). We also test for third-order effects of past advertising and find such lags are insignificant ($\alpha = -0.047, ns$).

[Insert Table 4 here]

As expected, we did not observe a main effect for *Election* in the sample of both U.S. and non-U.S. firms. However, as predicted, we did observe a positive significant effect for *Election*U.S._Headquarter* for firm advertising spending ($\alpha_4 = 2.061, p < 0.01$). We also investigated the effect of election on SG&A spending. The augmented Dickey-Fuller test failed to reject the existence of unit root ($p > 0.90$). Hence, we took the first difference of the data as our dependent variable. The *Election*U.S._Headquarter* effect replicated using SG&A spending ($\alpha_4 = 33.173, p < 0.05$). This result supports H5 which predicts that the comparative mind-set will affect firm spending levels (see Web Appendices B and C for additional robustness checks and tests to rule out competing explanations).

Quasi-experiment 3 results. Table 5 contains the results of our estimated Model 3. The overall fit of the model was adequate (Wald $\chi^2(9) = 1582.11, p < 0.00001$) and a good approximation of the data without serial correlations at the second-order of the error terms ($z = -0.512, ns$). We also tested for second-order effect of past advertising and found it was insignificant and of much smaller magnitude ($\alpha = 0.061, ns$). Hence, only one lag was included.

[Insert Table 5 here]

With respect to our predictions, we observed a positive and significant effect for *Election* ($\alpha_2 = 2.272, p < 0.05$). This result indicates a main effect of the comparative mind-set, consistent with H5. Consistent with H6, we observed that the interaction of *Election*Recession* was negative and significant ($\alpha_7 = -6.021, p < 0.05$), indicating that the comparative mind-set effect associated with the presidential election weakens during tough economic times. The interaction of *Election*Firm_Profit*, on the other hand, was positive and significant ($\alpha_8 = 0.002, p < 0.10$), indicating that when managers' companies were more profitable, the effect of the comparative mind-set strengthened. This supports H7. Considering the two-way interaction of

*Election*Swing_State*, we found a positive and significant effect ($\alpha_9 = 5.120, p < 0.05$), indicating that the effect of the comparative mind-set strengthened when the state associated with the manager's company headquarter was a swing state. This supports H8.

We again investigated the effect of election on SG&A spending. The augmented Dickey-Fuller test failed to reject the existence of unit root ($p > 0.44$). Hence, we took the first difference of SG&A. Supporting H5, we observe a positive main effect for *Election* ($\alpha_2 = 14.809, p < 0.001$). Consistent with H6-H8, the election effect weakened during recessions ($\alpha_7 = -68.595, p < 0.001$) but strengthened when firms had higher profits ($\alpha_8 = 0.037, p < 0.001$), and/or headquarters in swing states ($\alpha_9 = 57.231, p < 0.05$). Web Appendix B contains robustness checks for quasi-experiment 3 including evidence that swing states receive higher levels of political advertising than non-swing states while Web Appendix C rules out alternative explanations.

GENERAL DISCUSSION

Summary of Findings

Four lab experiments and three quasi-experiments demonstrate that managers exhibit the comparative mind-set when making decisions. Summarizing our experimental evidence, first, we find that activating a comparative mind-set by making animal preference judgments increases managers' likelihood of purchasing one of the two computers for their company rather than deferring choice (Experiment 1a). Second, comparing ads on different dimensions also activates a comparative mind-set and increases actual purchasing managers' likelihood of purchasing computers (Experiment 1b). Third, the comparative mind-set increases the accessibility of which-to-choose procedures and decreases the accessibility of whether-to-choose procedures (Experiment 2). Decreased accessibility of whether-to-choose procedures leads managers to

attach less importance to negative attributes at the stage of information integration, which mediates the impact of the comparative mind-set on choice (Experiment 3). Fourth, activating a comparative mind-set not only increases managers' likelihood of choosing to market a product, but also enhanced marketing spending levels for that product.

In terms of quasi-experimental evidence, first, we find that frequently comparing political candidates in presidential election years also gives rise to a comparative mind-set, leading managers to spend more on advertising while playing a business strategy simulation (QE 1). Second, presidential elections also increase advertising spending among marketing managers in companies headquartered in the U.S. relative to a control group of companies headquartered outside the U.S. (QE 2). Third, we identify a set of factors that amplify or weaken this effect among U.S. firms. Specifically, high decision scrutiny mitigates the effect of a comparative mind-set on advertising spending decisions, whereas low decision scrutiny strengthens the mind-set influence. Decision scrutiny is influenced by both macroeconomic factors (e.g., economic recessions) and microeconomic variables (e.g., firm profit). Furthermore, the frequency of making comparative judgments strengthens the influence of a comparative mind-set corresponding to the finding that marketers residing in swing states are more strongly influenced by the mind-set activated by comparing political candidates.

We also conducted additional experiments and secondary data analyses to exclude alternative explanations including (1) the price of advertising is higher in election years; (2) commercial advertising needs to break through the clutter of presidential election advertising; (3) managers are more optimistic in election years; (4) managers spend more because consumers spend more; (5) managers believe consumers pay more attention to advertising during election

years; and (6) managers advertise more because of the summer Olympics not because of the comparative mind-set (See Web Appendix C for complete analysis).

Contributions to the Literature on the Comparative Mind-Set

These studies extend the theory about the comparative mind-set in three ways. First, our paper is the first to demonstrate the underlying process by which the comparative mind-set affects choice. We demonstrate that a comparative mind-set increases the accessibility of which-to-choose procedures and decreases the accessibility of whether-to-choose procedures. Furthermore, the comparative mind-set decreases the importance attached to negative attributes during the information integration stage of the decision making process.

Second, previous research has focused exclusively on the impact of the comparative mind-set on choice decisions. Other mind-set research in the decision making domain has also emphasized choice (Dhar et al. 2007). We extend the scope of investigation and show that the influence of comparative mind-set also impacts the amount of money spent in both hypothetical and real-world advertising spending decisions. This is important both theoretically and practically. Theoretically, it extends the theory of comparative mind-set effect and shows that the comparative mind-set may have broader influence than assumed in previous research. Practically, because spending level decisions naturally follow choices in many managerial decision situations and spending level decisions may be more easily traceable than choice decisions, understanding that a comparative mind-set also influences spending level decisions can help managers identify whether their decisions have been influenced by a comparative mind-set, and if so, where to focus their debiasing efforts.

Third, generating our theory in a managerial domain led us to uncover a set of moderators that influence the impact of the comparative mind-set on spending levels. We

identify a set of market-based (recession, swing state) and firm-based (firm profit) factors that influence the impact of the comparative mind-set. Drawing on traditional theories of decision scrutiny and knowledge accessibility, we took initial steps to map out a more complete portrait of the nomological network of ideas that are operating in this research area.

Fourth, our research uses a multi-method approach to examine the comparative mind-set effect. The use of lab experiments and quasi-experiments complement one another and make unique contributions to advance our understanding of the comparative mind-set effect. For example, the lab experiments allow us to use different manipulations of comparative mind-set and different dependent variables (choice, spending) to capture the mind-set influence, which increases the robustness of our findings. In contrast, the quasi-experiments demonstrate the external validity of the comparative mind-set effect and provide important information about the real-world moderators of this effect.

Contributions to the Study of Managerial Decision Making

First, our findings indicate although managers are hired and rewarded for their ability to process information and make decisions, they are not unlike consumers. Both managers and consumers are subject to the influence of a comparative mind-set. For managers, however, the results of this effect can be quite profound. Specifically, if marketers spend when they should defer or spend more than they should, the return on marketing spending will decrease. Using parameters from our model, we calculate that U.S. companies spend \$2.54M or 2.94% more in presidential election years compared to non-presidential election years. Although we cannot prove that these spending levels are “wasteful,” we expect that consistent overspending will lead to questions about marketing’s role and impact in the company.

Second, we suspect that a comparative mind-set is not an uncommon occurrence in managerial settings. We expect that in addition to external stimuli, such as presidential elections, managers are more likely to experience the comparative mind-set when they are, for example, choosing between acquisition targets, new product prototypes, and advertising campaign options. In these and similar situations, we expect that managerial activities involving comparisons could inadvertently tip managers into agreeing to decisions and overspending.

Third, our paper bridges research on consumer decision making and research on managerial decision making. Because similar underlying processes may apply to both domains, findings documented in consumer research literature may also explain the behavioral patterns of managers, and vice versa. We hope our research will spur collaborations between researchers working in these two domains. Doing so in our research allowed us to extend the scope of previous research in several key ways (e.g., mediating process, moderating variables, and spending outcomes).

Future Research Directions

We identified several moderating factors of the comparative mind-set effect in our managerial decision context. For example, in the quasi-experiment, we found that factors increasing decision scrutiny (i.e., economic recession) or decreasing decision scrutiny (i.e., profit margin) attenuate or amplify the comparative mind-set effect, respectively. Translating these findings back into consumer behavior research and identifying real-world factors in a consumer context that increase decision scrutiny (i.e., economic recession) or decrease decision scrutiny (i.e., discretionary household income) would be an interesting contribution to the literature.

Our quasi-experiments show that activating a comparative mind-set increases discretionary spending items such as ad spending and selling, general, and administrative

spending, but does not impact non-discretionary spending items such as R&D spending.

Additional tests of the effect of the comparative mind-set on consumer spending for luxury versus necessity products would be an interesting complement to our findings.

Our focus on presidential elections as a factor that spurs the comparative mind-set in the real world is similar to other literature addressing the importance of environmental factors in human behavior. For example, research has demonstrated the impact of weather on stock returns (Hirshleifer and Shumway 2003), voting locations on voting preferences (Berger, Meredith, and Wheeler 2008), and sporting event victories on automobile fatalities (Wood, McInnes, and Norton 2011). We hope our research will contribute to additional research of this type, especially as it relates to manager decisions.

Future research involving managers could fruitfully extend this line of research by determining the generalizability of our findings across different kinds of decision contexts and decision makers. Considering decision contexts, future research could examine whether our findings apply to tasks that are strategically more important, for example, the choice to acquire a firm or to invest in a new distribution channel. As importance increases, decision scrutiny may increase, which may weaken the mind-set effect.

In terms of types of decision makers, Morrin et al. (2002), for example, examine stockbrokers and uncover two dominant profiles—contrarian and momentum investors. Other research has documented the presence of decision makers who are maximizers versus satisficers (Schwartz et al. 2002) or who are prevention-focused (i.e., concerned with security and focused on reducing errors of commission) vs. promotion-focused (i.e., concerned with growth and accomplishment and focused on reducing errors of omission) (Crowe and Higgins 1997). Mahajan (1992) examines managers with different levels of expertise. Based on our initial

findings, we might predict that managers who are momentum-focused, maximizers, promotion-focused, and have more expertise will be more likely to exhibit the comparative mind-set effect given these decision makers may be less likely to revisit “whether to buy.”

Finally, given the long history of research in marketing on de-biasing decisions (see Bolton 2003; Boulding et al. 1997), we hope that future research will also consider techniques to remove the potentially damaging effects of the comparative mind-set. For example, techniques that clearly delineate the decision to buy from the consideration of options should reduce the comparative mind-set effect within a given decision.

CONCLUSION

Previous research shows that making comparative judgments in one domain activates a comparative mind-set that disposes consumers to purchase a product rather than deferring choice in a different domain. We examine the influence of this comparative mind-set on managers’ decision making. In four lab studies involving managers with expertise in the domain and in three large-scale quasi-experiments using presidential elections to evoke the comparative mind-set, we find that activating a comparative mind-set not only increases managers’ likelihood of making hypothetical purchases but also elevates their advertising and marketing spending levels in real business decisions. We extend the theory on the comparative mind-set by uncovering the mediating process underlying the increased likelihood of choice and by identifying a set of real-world moderators involving decision scrutiny and knowledge accessibility that amplify or weaken its influence on managers’ decisions.

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TABLE 1. THE COMPARATIVE MIND-SET EFFECT ON CHOICE

	Experiment 1a: Executive MBA Students		Experiment 1b: Purchasing Professionals	
	Choice Effect (H1)		Choice Effect (H1)	
Main Effects	Coefficient (SE)	Wald χ^2 (1)	Coefficient (SE)	Wald χ^2 (1)
Comparative Mind-set	0.807 (0.412)	3.848*	1.204 (0.612)	3.875*
Work Experience Level	-0.043 (0.037)	1.392	-0.038 (0.027)	2.037
Work Position Level	--	--	0.041 (0.132)	0.097
Number of Observations	105		69	

*** $p < 0.001$, ** $p < 0.01$, * $p \leq 0.05$, + $p < 0.10$.

**TABLE 2. THE COMPARATIVE MIND-SET EFFECT
ON CHOICE AND SPENDING LEVELS**

	Experiment 3: Marketing Professionals			
	Choice Effect (H1)		Spending Level Effect (H5)	
Main Effects	Coefficient (SE)	Wald χ^2 (1)	Coefficient (SE)	F (1, 154)
Comparative Mind-set	1.020 (0.344)	8.807**	18.495 (4.702)	15.472***
Work Experience Level	-0.047 (0.023)	4.206*	-0.750 (0.306)	6.02*
Work Position Level	-0.071 (0.089)	0.632	-0.716 (1.234)	0.337
Number of Observations	158			

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.10$.

TABLE 3. QUASI-EXPERIMENT 1 RESULTS

	Firm Advertising Spending Level	Firm Other Marketing Spending Level
	Coefficient (SE)	Coefficient (SE)
Intercept (α_0)	14.746 (2.863)***	1903.239 (97.389)***
One-year Lag Firm Spending Level (α_1)	0.741 (0.009)***	0.521 (0.008)***
Two-year Lag Spending Level (α_2)	0.209 (0.010)***	0.239 (0.008)***
H5: Presidential Election (α_3)	10.703 (2.313)***	376.447 (67.849)***
Recession (α_4)	-0.647 (1.584)	74.394 (47.752)
Firm Profit (α_5)	0.000 (0.000)	0.002 (0.001)*
Trend (α_6)	-3.807 (1.512)*	-96.039 (44.458)*
EMBA (α_7)	-2.558 (2.219)	-56.549 (65.966)
H6: Presidential Election * Recession (α_8)	-4.967 (2.181)*	-618.163 (65.729)***
H7: Presidential Election * Firm Profit (α_9)	0.001 (0.000)***	0.067 (0.005)***
Number of Industry-Firm-Product-Period Observations	12868	
Number of Industry-Firm-Product Observations	2303	
Wald χ^2 (14)	23515.94***	19498.41***

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Note: Model also contains four firm dummies.

TABLE 4. QUASI-EXPERIMENT 2 RESULTS

	Firm Advertising Spending Level	Firm SG&A Spending Level
	Coefficient (SE)	Coefficient (SE)
Intercept (α_0)	-19.709 (8.035)*	-6.453 (29.281)
One-year Lag Firm Spending Levels (α_1)	0.499 (0.098)***	-0.058 (0.035)+
Two-year Lag Firm Spending Levels (α_2)	0.169 (0.065)**	
Presidential Election (α_3)	0.076 (0.405)	-16.271 (8.355)+
H5: Presidential Election*U.S. Headquarters (α_4)	2.061 (0.764)**	33.173 (15.472)*
Trend (α_5)	0.917 (0.311)**	0.959 (0.832)
Number of Firm-Year Observations	27535	25816
Number of Firms	1101	1087
Wald χ^2 (5) / Wald χ^2 (4)	584.19 ***	16.45**

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.10$.

TABLE 5. QUASI-EXPERIMENT 3 RESULTS

	Firm Advertising Spending Level	Firm SG&A Spending Level
	Coefficient (SE)	Coefficient (SE)
Intercept (α_0)	3.832 (4.104)	28.885 (18.123)
One-year Lag Firm Spending Level (α_1)	0.947 (0.077)***	-0.067 (0.040)
H5: Presidential Election (α_2)	2.272 (0.997)*	14.809 (4.067)***
Recession (α_3)	-3.650 (1.168)**	-0.813 (6.460)
Firm Profit (α_4)	-0.005 (0.004)	-0.052 (0.038)
Swing State (α_5)	1.163 (2.453)	15.912 (19.319)
Trend (α_6)	0.577 (0.477)	4.092 (2.450)+
H6: Presidential Election * Recession (α_7)	-6.021 (2.466)*	-68.595 (18.254)***
H7: Presidential Election * Firm Profit (α_8)	0.002 (0.001)+	0.037 (0.003)***
H8: Presidential Election * Swing State (α_9)	5.120 (2.168)*	57.231 (24.278)*
Number of Firm-Year Observations	18090	17328
Number of Firms	779	776
Wald χ^2 (9)	1582.11***	372.08***

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.10$.

WEB APPENDIX A: TESTING THE EFFECT OF PRESIDENTIAL CAMPAIGNS ON THE GENERATION OF COMPARATIVE THOUGHTS

Our quasi-experiments are predicated on the assumption that political campaigns in election years prompt people to make more comparative judgments, and thus activate a comparative mind-set. Research has shown that the percentage of comparative ads for commercial products is approximately 3.52% of the total number of ads (Gnepa 1993). Therefore, in a non-presidential election year, most advertising to which people are exposed is non-comparative in nature. We performed Experiment 4 to validate our assumption that political advertising is more comparative than typical commercial ads. To do so, we recruited 79 participants from Amazon's Mechanical Turk. All were U.S. residents and at least 18 years old (mean = 32.95, SD = 12.33) and received \$1.00 USD for participating. Participants were randomly assigned into one of two conditions. In the *political ad* condition, participants watched four political ads in random order. Two supported Barack Obama and two supported Mitt Romney.⁹ In the *commercial ad* condition, participants watched four typical ads in random order for products such as Tropicana orange juice and Volkswagen Passat. After watching each ad, participants were asked to write down the thoughts they had while they watched the ad. Upon completion, they reported their political party affiliations, interest in political campaigns, gender, and age. Participants' thoughts towards each ad were coded as "1" if they reflected comparison and "0" otherwise. In the *political ad* condition, the comparative thoughts generally fell into one of three categories: (i) comparisons between candidates (e.g., "Everyone always blames Obama for the economy, but it's not really his fault. The economy fluctuates, and it takes time to recover. It'll be the same thing under Romney"); (ii) comparisons between alternative courses of action based on the ad content (e.g., "Obama is for keeping jobs in America, Romney outsources jobs and gives tax breaks to companies that outsource jobs"); and (iii) preferences for one ad (e.g., "Of all the ads, I liked this one the best"). In the *commercial ad* condition, three types of comparative thoughts were identified: (i) comparison of the featured product and other products (e.g., "Who drinks orange juice in the morning to wake up?! Coffee please!"); (ii) comparisons based on the ad content (e.g., "I assumed that the bear wearing the blue scarf was representing Pepsi. He fought for his Coke and emerged victorious"); and (iii) preference for one ad over other ads (e.g., "One of the best commercials I have seen").

The number of comparative thoughts towards the four ads was analyzed as a function of political ad vs. commercial ad, party affiliation, and interest in political campaigns. Results indicated that participants generated more comparative thoughts towards political ads ($M_{\text{political_ad}} = 1.39$) than commercial ads ($M_{\text{commercial_ad}} = 0.46$), $F(1,72) = 17.42$, $p < 0.001$. Party affiliations and interest in political ads had no effect. These results validated our assumption that political campaign ads evoke more comparative thoughts than typical commercial ads. Importantly, we would not expect exposure to comparative commercial ads to differ from exposure to political ads. Our purpose with this validation study was to show that political advertising promoted more comparisons than typical ads (Gnepa 1993).

⁹ Political ads: <http://www.youtube.com/watch?v=I1pPFlcGav4>; <http://www.youtube.com/watch?v=r1D1jI61ckY>; <http://www.youtube.com/watch?v=H3a7FC0Jkv8>; <http://www.youtube.com/watch?v=OQUyS9H6ioI>) and commercial ads: <http://www.youtube.com/watch?v=TFVJ0bN8xyo>, <http://www.youtube.com/watch?v=5oOVqUInA6w>, <http://www.youtube.com/watch?v=06d0Pe2bq64>, and <http://www.youtube.com/watch?v=S2nBBMbjS8w>.

WEB APPENDIX B: ROBUSTNESS CHECKS FOR QUASI-EXPERIMENTS 2 AND 3¹⁰

Quasi-Experiment 2 Robustness Check

We took several steps to examine the robustness of the interaction of *Election*U.S._Headquarter*. First, we ensured the U.S. and non-U.S. firm samples were similar by limiting the firms in our international sample to the industries found in the U.S. sample by matching two-digit ($n = 1016$ firms), three-digit ($n = 748$ firms), and four-digit SIC codes ($n = 635$ firms). Results replicated across all three samples. We also matched firms on sales revenue ($n = 423$ firms), EBITDA ($n = 856$ firms), and market capitalization ($n = 804$ firms) and results replicated. Second, given our quasi-experiment used a continuous dependent variable, we also replicated our result using a dependent variable similar to the choice/defer choice used to test H1 in our lab experiments.¹¹

Third, because firms were nested in 25 countries, we replicated our findings using a Hierarchical Linear Model (HLM). Though we had no a priori prediction regarding the differences in coefficients among groups of firms in different countries, this approach allowed for both correlated error terms within each group and different variances across groups. Our model was a three-level HLM with nested structure of year-firm-country, controlling for autocorrelation within each firm across years. For advertising, the random-intercept model failed to reject the null hypothesis that the intercept is the same ($\chi^2(3) = 0.16, ns$). Specifically, the country-level random effect was not significant ($\delta = 0.984, ns$). As a result, HLM is not necessary. For SG&A spending, a random-intercept model was preferred to a non-hierarchical model ($\chi^2(3) = 9.46, p < 0.05$). Further results showed that a random-slope model that allowed for the differential impact of election outperformed a random-intercept model (LR $\chi^2(1) = 93.97, p < 0.0001$). Results replicated and indicated that the main effect for *Election* was not significant ($\alpha_2 = -20.834, ns$), but there was a positive significant effect for *Election*U.S. Headquarter* ($\alpha_3 = 36.458, p < 0.05$) as expected.

Fourth, to resolve potential concerns about endogeneity, we utilized an instrumental variable approach given lagged advertising spending may be correlated with error terms. The Arellano and Bond (1991) estimator uses additional lags of the dependent variables as instruments and our results were robust to the number of instruments used. Another possible source of endogeneity arises from firm profitability because of an omitted variable bias. Random shocks as omitted variable can affect both firm advertising spending and profits. We took two steps to address this issue. We used a lagged measure of firm profitability instead of contemporaneous measure to rule out random shock effect and to establish causality. Further, as an additional precaution, we used additional lags of past firm profitability as instruments in a Two-stage Least Squares (2SLS) approach. This was because additional lags of firm profits are correlated with firm profitability, but not the potential omitted variable. Results rejected the weak instrument test. Specifically, the AP $F_{2, 15950} = 42509.16$ meets the highest threshold for the

¹⁰ For all model tests performed in Web Appendices B and C, overall model fits were significant ($p < 0.001$). For the fixed-effect models, there were no significant serial correlations at the second-order of the error terms (Arellano and Bond 1991). For the random-effect models, Durbin-Watson statistics were all close to 2, indicating no first-order serial correlation. Details are available from authors.

¹¹ We created a measure that is the percentage change in advertising (SG&A) from $t-1$ to t . Given median 7.7% (9.2%) and mean 16.4% (0.2%) percentage changes, we defined 10% (5%) as the threshold for “choice” and create a dummy variable coded 1 if a firm changes advertising spending beyond +10% (5%) and 0 otherwise for “defer” option. Our results replicated and also hold if we used 15% (1%) and 20% (2%) as the threshold.

Stock–Yogo weak IV test of two instruments (threshold= 19.93). Results using this formulation replicated. We also used past firm revenues as an instrument and results are similar.

Finally, we tested our model using *firm research and development (R&D) spending* which represents all costs related to the development of new products and services. Because our theory only holds for discretionary spending that is subject to comparative mind-set, the election effect was not expected to be to hold for these less discretionary expenditures (Sood and Tellis 2009). As expected, *Election*U.S._Headquarter* was not significant ($\alpha_4 = 1.065, ns$).

Quasi-Experiment 3 Robustness Checks

We performed several robustness checks to determine the validity of our results. First, we tested a key assumption in quasi-experiment 3—that swing states receive more political advertising. To test this assumption, we collected information about the level of political advertising directed to each of 49 states (Delaware was not included in the data) and Washington DC during the 2008 election published by TNS Media Intelligence (http://www.tvb.org/media/file/TVB_PB_2008_Candidate_Spending.pdf). We used our measure of swing state probability from quasi-experiment 3 and estimated this model:

$Political_Advertising_Spending_i = \alpha_0 + \alpha_1 * Swing_State_i + v_{it}$ (A1). Results indicated a positive significant effect for *Swing_State* ($\alpha_1 = 27.961, p < 0.05$) which supported the assumption that swing states received more political advertising in election years.

Second, similar to quasi-experiment 2, given our quasi-experiment used a continuous dependent variable, we replicated our results using a dependent variable more like the choice/defer choice used in our lab experiments. Third, because our sample of U.S. firms is nested in 42 different states, we replicated our findings by using a three-level random-intercept HLM with nested structure of year-firm-state, controlling for autocorrelation within each firm. Finally, we retested our model using firm R&D spending level. As expected, given the less discretionary nature of this spending, neither the main effect of *Election* nor the interactions were statistically significant.

WEB APPENDIX C: ALTERNATIVE EXPLANATIONS FOR QUASI-EXPERIMENTS 2 & 3

Alternative Explanation 1: Managers Spend More Because the Price of Advertising is Higher in Presidential Election Years

The first alternative account of our effects is that increased advertising spending levels were due to an increase in the price of advertising in presidential election years. Specifically, if demand for advertising increases in presidential election years, perhaps the price, not the level, of advertising, increases. This explanation could also include the possibility that political campaigns purchase lower-priced advertising slots, leaving only higher-priced advertising for companies. We took several steps to rule out this explanation. First, we acquired data from Kantar Media that include yearly advertising volume and spending levels from 61 industries from 1986-2011 both at the aggregate level and across thirteen media (excluding all radio data and outdoor volume data which were not available). Given this, our unit of analysis was the industry level i . Given price information was not available in the data, we calculated the average price of advertising overall and at the level of each media type by dividing the total advertising dollars spent by the number of ads run. Using this dependent variable, we performed two sets of analyses. First, we examined whether the price of advertising increased during presidential election years. To do so, we aggregated the media into three broad classes: Television, Magazine, and Newspaper. We estimated the model: $Ad_Price_{it} = \alpha_0 + \alpha_1 * Election_t + \alpha_2 * Trend_t + \alpha_3 * Television_i + \alpha_4 * Magazine_i + \alpha_i + v_{it}$ (A2). Newspaper is the baseline and α_0 is the mean for this group. *Television* is a dummy variable coded as 1 for TV advertising spending and 0 otherwise. Likewise, we created a dummy variable *Magazine* coded as 1 for Magazine advertising and 0 otherwise. We adjusted for heteroskedasticity in the error terms for the same media. The effect of *Election* was not significant in this model ($\alpha_1 = 0.258$, *ns*) indicating that the price of advertising did not increase, on average, at the industry level.

Second, given that most political advertising occurs on TV, if the alternative explanation holds, we should observe that the price of TV advertising increased during presidential election years. To test this idea, we tested a model with variables reflecting the interaction of each media type and *Election*. This model is: $Ad_Price_{it} = \alpha_0 + \alpha_1 * Election_t + \alpha_2 * Trend_t + \alpha_3 * Television_i + \alpha_4 * Magazine_i + \alpha_5 * Election_t * Television_i + \alpha_6 * Election_t * Magazine_i + \alpha_i + v_{it}$ (A3). *Election* did not change the price of TV advertising ($\alpha_5 = 0.418$, *ns*), although the price of magazine advertising decreased during presidential election years ($\alpha_6 = -3.363$, $p < 0.10$). Third, given local media advertising price is most likely influenced by demand for advertising during election years (given the need to advertise within states due to the electoral college), we created a variable, *Local_Media*, that was coded 1 for those media contained in this data that are local, including local newspaper, magazines, and spot TV and estimate the model: $Ad_Price_{it} = \alpha_0 + \alpha_1 * Election_t + \alpha_2 * Trend_t + \alpha_3 * Local_Ad_i + \alpha_4 * Election_t * Local_Media_i + \alpha_i + v_{it}$ (A4). There was no significant effect of election on the price of local media advertising ($\alpha_4 = 0.288$, *ns*).

Alternative Explanation 2: Managers Spend More Because They Must Break Through the Clutter of Presidential Election Advertising

To rule out this explanation, we acquired Kantar firm-level advertising data. If this explanation is true, we should observe that firms spent more on local media advertising in swing states in

presidential years because local, not national, media, is the focus of political campaigning given the Electoral College makes national advertising inefficient. To examine this idea and whether it affects our results, we used a sample of 21,961 firms' spending on 16 types of media in 101 standard metropolitan statistical areas (SMSAs) 1998-2012. The 16 media were: network TV, cable TV, magazine, local radio, spot TV, SLN TV, syndication, Sunday magazines, local magazines, B2B, national newspaper, local newspaper, Hispanic newspaper, national spot radio, detailed local radio and internet display. We coded local radio, spot TV, local magazines, local newspapers, national spot radio and detailed local radio as *Local_Media* = 1 and 0 otherwise.¹² We then coded each SMSA into a state. At the SMSA level, we had an unbalanced panel of firm spending on each media with 2,330,610 firm-media-SMSA and 15,537,250 firm-media-SMSA-year combinations. At the national level, we had an unbalanced panel of firm spending on each media in each year, with 339,970 firm-media and 2,007,735 firm-media-year observations. For our first test regarding SMSA-level advertising spending, our model was: $Ad_Spend_{imct} = \alpha_0 + \alpha_1*Ad_Spend_{imct-1} + \alpha_2*Ad_Spend_{imct-2} + \alpha_3*Election_t + \alpha_4*Recession_t + \alpha_5*State_Swings_{ct} + \alpha_6*Trend_t + \alpha_7*Local_Media_m + \alpha_8*Election_t*State_Swings_{ct} + \alpha_9*Election_t*Local_Media_m + \alpha_{10}*Local_Media_m*State_Swings_{ct} + \alpha_{11}*Election_t*Local_Media_m*State_Swings_{ct} + \alpha_{imc} + v_{imct}$ (A5), where Ad_Spend_{imct} is advertising spending for firm i on media m in SMSA c and year t , Ad_{imst-1} and Ad_{imct-2} are one and two-year lagged-advertising values respectively, $Election$ is a dummy indicating whether year t is presidential election year or not, $Recession$ is a dummy indicating whether year t is 2001 or 2008, $State_Swings$ is the probability of the state of SMSA c "swings" between parties in years t as defined earlier, $Trend$ is the year trend for year t , $Local_Media$ is a dummy indicating whether media m is local or not, and α_{imc} is the firm-media-SMSA random effect. v_{imct} is the error term with $v_{imct} \sim N(0, \sigma_{mct}^2)$. Results indicated a positive significant effect for $Election$ ($\alpha_3 = 2.180, p < 0.0001$) and $Election*Local_Media$ ($\alpha_9 = 6.201, p < 0.0001$), which suggested that firms did, on average, spend more on local media advertising in election years. The two-way interaction of election and swing state was negative and significant ($\alpha_8 = -1.948, p < 0.0001$)¹³. In contrast, the three-way interaction of election, local media advertising, and swing states was positively significant ($\alpha_{11} = 11.642, p < 0.0001$) indicating firms only spent more on local media advertising in swing states during presidential election years.

Given these results, there is a possibility that our results are due to competition for local media advertising in swing states. To probe this effect further, we limited the sample to non-local media advertising (1,627,730 firm-media-SMSA observations and 12,544,324 firm-media-SMSA-year observations) and tested this model: $Ad_Spend_{imct} = \alpha_0 + \alpha_1*Ad_Spend_{imct-1} + \alpha_2*Ad_Spend_{imct-2} + \alpha_3*Election_t + \alpha_4*Recession_t + \alpha_5*Trend_t + \alpha_6*Election_t*Recession_t + \alpha_{imc} + v_{imct}$ (A6). This included the effect of the election (H5) and the interaction of election*recession (H6). We did not test the interactions of election*firm profit (H7) and election*firm swing state headquarters (H8) given it would be necessary to code this information for 21,961 firms in this data set. Our predictions for the comparative mind-set hold for non-local media advertising. Specifically, we observed a positive significant effect for $Election$ ($\alpha_3 = 0.158, p < 0.0001$) and a weakening of this effect during recession ($\alpha_6 = -0.527, p < 0.0001$).

These results were encouraging. However, in order to exclude the alternative explanation that our prior results on national spending in quasi-experiments 2 and 3 were driven by local

¹² A greater variety of local media are identified in this version of the database.

¹³ This swing state variable, *State_Swings*, captures firm spending in different geographic markets. This differs from the swing state variable, *Swing_State*, which capture firm headquarter locations in quasi-experiment 3.

media advertising increases in swing states and not the comparative mindset induced by the election, we aggregated the data from the state to the national level and investigated whether the interaction of the election*local media advertising (as defined above) was predicted overall national advertising levels by firms (which include both local and non-local advertising). If so, this could explain our results. Our model was: $Ad_Spend_{imt} = \alpha_0 + \alpha_1*Ad_Spend_{imt-1} + \alpha_2*Ad_Spend_{imt-2} + \alpha_3*Ad_Spend_{imt-3} + \alpha_4*Election_t + \alpha_5*Recession_t + \alpha_6*Trend_t + \alpha_7*Local_Media_m + \alpha_8*Election_t*Local_Media_m + \alpha_{im} + v_{imt}$ (A7). We included the third-order lagged value of advertising because second-order only did not lead to model convergence. We observed a positive and significant effect for *Election* ($\alpha_4 = 21.024, p < 0.0001$). However, the interaction of election and local media advertising was not significant ($\alpha_8 = -7.183, ns$), which suggests that local media advertising spending was not significantly different from non-local media advertising during election years which means that the effect of local media advertising spending in swing states was not observable at the national level. This validated our previous findings and suggests that although greater advertising and the clutter effect may have existed in certain states, our results were not driven by this effect. To verify this, we performed one final test which is to limit our sample to non-local media advertising only (212,507 firm-media combinations and 1,254,920 firm-media-year observations) in order to see if our effects held. Our model was: $Ad_Spend_{imt} = \alpha_0 + \alpha_1*Ad_Spend_{imt-1} + \alpha_2*Ad_Spend_{imt-2} + \alpha_3*Ad_Spend_{imt-3} + \alpha_4*Election_t + \alpha_5*Recession_t + \alpha_6*Trend_t + \alpha_7*Election_t*Recession_t + \alpha_{im} + v_{imt}$ (A8). We observed a positive significant effect for *Election* ($\alpha_4 = 26.170, p < 0.000$) and that this effect weakened during tough economic times ($\alpha_7 = -39.577, p < 0.0001$).

Alternative Explanation 3: Managers Spend More Because They Are More Optimistic in Presidential Election Years

Increased advertising spending could also due to managers' increased optimism about the U.S. economy that might occur during presidential elections. To test this idea, we collected the Investor Sentiment Index from 1952-2008, which is based on six underlying proxies for sentiment—the closed-end fund discount, NYSE share turnover, the number and average first-day returns on IPOs, the equity share in new issues, and the dividend premium (Baker and Wurgler 2006). Using this Index, we estimated the model: $Sentiment_t = \alpha_0 + \alpha_1*Sentiment_{t-1} + \alpha_2*Election_t + \alpha_3*Trend_t + \alpha_4*Recession_t + v_t$ (A9). *Election* was not significant ($\alpha_2 = 0.117, ns$), indicating that investors were not significantly more optimistic in election years. We also re-estimated Model 3 in quasi-experiment 3 and included Investor Sentiment Index as a control variable. Both the main effect of *Election* and all predicted interaction effects replicated, whereas investor sentiment was not significant.

Alternative Explanation 4: Managers Spend More Because Consumers Spend More in Presidential Election Years

This explanation argued that managers are not acting on the comparative mind-set, but instead are responding to consumers who are purchasing more due to the comparative mind-set arising from the election (Xu and Wyer. To rule this out, we collected data about consumer spending in the form of retail store sales from 1953 to 2000 (available from U.S. Census Bureau). These sales data are on the monthly level and so we aggregated to the annual level by averaging across months in the year. We then estimated the model: $Retail\ Sales_t = \alpha_0 + \alpha_1*Retail$

$Sales_{t-1} + \alpha_2 * Retail\ Sales_{t-2} + \alpha_3 * Election_t + \alpha_4 * Trend_t + \alpha_5 * Recession_t + v_t$ (A10). *Election* was significant ($\alpha_3 = 4473.34, p < 0.001$), indicating consumers do spend more during presidential years. Given this, we re-estimated Model 3 in quasi-experiment 3 and included Retail Sales as an additional control. Results indicated that both the main and interaction effects associated with the presidential election replicated for advertising, whereas retail sales was not significant ($\alpha_{10} = 0.000, ns$). Though retail sales positively influenced SG&A spending ($\alpha_{10} = 0.000, p < 0.001$), all the main and interaction effects replicated in the SG&A model, indicating consumer purchase momentum did not subsume the effect of comparative mind-set. Given the error terms in the firm spending and consumer spending equations were likely correlated, we repeated this analysis using seemingly-unrelated regressions (SUR) allowing for clustered errors terms within firms. As above, results indicated that consumers spent more during election years ($\alpha_3 = 5944.741, p < 0.001$). Both the main and interaction effect of presidential election with recession and swing state replicated for advertising ($\alpha_2 = 2.707, p < 0.05$; $\alpha_7 = -11.485, p < 0.01$; $\alpha_9 = 6.497, p < 0.05$), controlling for retail sales ($\alpha_{10} = -.000, ns$). Though the interaction of election and firm profitability became insignificant, this is not surprising given retail sales are correlated with firm profitability ($r = 0.256, p < 0.05$). Similar results were found for SG&A spending.

Alternative Explanation 5: Managers Spend More Because They Believe Consumers Pay More Attention to Advertising during Election Years

We conducted a survey to measure managers' expectations about how presidential elections may influence consumer behavior. Forty MBA students from a southeastern university completed the survey. They had mean work experience of seven years. The survey was ostensibly interested in investigating managers' beliefs about customer behavior during periods of political and economic uncertainty. Participants read a list of consumer activities and reported whether or not these activities would "Increase," "Decrease," or have "No change" in presidential election years. The activities included the key item of "Customer interest in viewing advertising," and other control items such as "Customer purchasing levels," "Customer loyalty," "Customer interest in new products or services," "Customer search for information about products and services," and "Customer interest in viewing political advertising." To reduce potential demand effect, participants also rated how economic recession would change these behaviors. Responses of "Increase," "Decrease," and "No change" were coded as "1", "-1", and "0", respectively. Given this, the mean score of each item was tested against "0." Results reveal that participants expected the presidential election to increase only customers' interest in viewing political advertising ($M_{political_ad} = 0.48, t(39) = 3.55, p < 0.001$), but not their interest in viewing non-political advertising ($M_{commercial_ad} = -0.08, t(39) = -0.90, ns$). Moreover, they did not expect the presidential election to influence any other consumer behaviors. Given these findings, it is unlikely that managers increased advertising spending because they believe that customers will pay more attention to all advertising during the year of an election.

Alternative Explanation 6: Managers Advertise More Because of the Olympics

The Olympics, which occur every four years, corresponding with the U.S. presidential election, could be the source of increased spending. Quasi-experiment 2 involving the control group ruled out this explanation because we demonstrated that U.S. firms outspend non-U.S.

firms in presidential election years. Therefore, even if there is a bump due to the Olympics, the comparative mind-set due to the presidential election produced an increase beyond this level.