EXCESSIVE OPTIONS AND EXCUSES

By

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Research shows that individuals in western society have felt increasingly less responsible for the outcomes in their lives over the past four decades. This dissertation examined two potential causes of this trend by determining whether excuse making after poor decisions is facilitated by (1) information overload caused by excessive options and (2) pro-excuse social environments.

In two experimental studies, participants chose between either 6 or 18 cars with the goal of finding the car that most closely matched a given standard. All participants in both studies received failure feedback. In Study 1, participants (N = 256) were also given physiological feedback that was intended to manipulate the intensity of negative affect they felt after failing. After the feedback, they completed a questionnaire measuring excuses and affect. In Study 2, participants (N = 162) were either given or not given a salient excuse for failure before the decision task. After the decision task and questionnaire, they completed a memory task with failure feedback followed by another questionnaire.

In both studies, participants with more options made more excuses for failure than participants with fewer options. These excuses ranged from relatively legitimate to relatively illegitimate, suggesting that participants with too many options disengaged their general

perceptions of responsibility over their decision, and that led them to take multiple routes of excuse making even when some were not warranted. In Study 2, participants who were provided with an excuse for failure on the first task also made more excuses after the second task despite the excuse not being relevant to the second task. This effect again suggested that making excuses in one context can lead to more excuses in another, irrelevant context—a metaphorical "culture of excuses." Finally, structural equation modeling in Study 2 showed that making excuses may actually hurt affect immediately after those excuses are used but improve affect in the long run. In general, this research suggests that trends in excuse making in western culture may be in part due to pervasive information overload resulting from an overabundance of options in everyday decision making, and a more pro-excuse culture may perpetuate this effect.

CHAPTER 1
INTRODUCTION

These studies were primarily designed to examine the effects of the number of available options on self-reported personal responsibility for a decision that turns out poorly. For example, imagine someone chose a product from a list of 10 options, and the product broke after only a few days. The probability that the person would not feel responsible for the poor outcome may differ if the number of options was higher or lower than 10. Would someone be more likely to defer responsibility for the poor decision if there were only three options from which to choose? What if there were 50? Moreover, why should option number have any effect on perceptions of responsibility at all? I hypothesized that more options would be associated with more excuse making and perceptions of less personal responsibility.

A secondary purpose of these studies was to determine if changes in reported responsibility reflect an overall change in attributional style that carries over to other tasks. In other words, would someone who made a lot of excuses regarding a poor decision be more likely to make excuses about some other, unrelated negative outcome at a later time? Would this chronic shift to a more external attributional style be more likely if the initial number of options was too cognitively difficult to process? What if the decision were simple (i.e., only a few options)? It was hypothesized that people who experienced conditions that facilitated excuse making on one task would then be more likely to make excuses on later, irrelevant tasks.

These questions were addressed in two studies that assessed the causal effects of option number on responsibility and some possible processes that could be driving those effects. The following sections will set the stage by providing contextual background for the research questions and then describing the research that was conducted to answer those questions. First, a review of some sociological trends and some examples of relevant, real-world phenomena

explains why the importance of these research questions goes beyond basic laboratory research. Second, a scientific literature review combines information from social psychological and marketing sources to establish theoretical background for predictions involving the constructs of interest. Finally, two studies are presented that address these issues.

Sociological Background

Anthony Rezza was fired for embezzling $2000 from the federal government, which he subsequently lost gambling in Atlantic City. However, the man was reinstated in his job after suing the government because, as a compulsive gambler, he was not considered responsible for his actions and was thus protected by the Americans with Disabilities Act (Rezza v. United States Department of Justice, 1988). This lawsuit is an example of someone using a self-serving excuse to defer personal responsibility and gain an advantage after a negative outcome. Ridiculous as it might seem, however, the story reflects a trend in western society over the last 40-50 years toward a social climate that is more accepting of excuses than ever before, or, at least, more accepting of excuses at the level of the individual (as opposed to the level of the corporation, school, government, etc.).

Examples of this trend are present in many aspects of contemporary society. The number of civil lawsuits in America, for example, almost tripled between 1960 and 1990, and the numbers of lawyers also increased dramatically faster than the general population (Margolick, 1991). According to some estimates, the United States has well over half the total number of lawyers in the world (e.g., Sykes, 1992) with over 1,000,000 total (Rowley & Hall, 2006)—dozens to hundreds of times more lawyers per person than any nation in the world. Between 1970 and 2000, the number of lawyers per 1,000 Americans has increased 233% (Caplow, Hicks, & Wattenberg, 2001). Although it is certainly debatable whether this tremendous increase is lawyers is a cause or an effect of the shift toward less personal responsibility on the part of the

individual, it is clear that the trend toward more civil litigation implies that it is more acceptable than ever for individuals to make excuses in a variety of situations. Although one could argue that an increase in lawsuits may actually reflect an increase in the social importance of responsibility (i.e., the purpose of lawsuits is to determine who is responsible for some outcome), that argument does not account for the fact that the people bringing all these lawsuits are doing so because they do not feel (or do not want to appear to be) responsible for their outcomes, and that is by definition an excuse. Thus, even if legal responsibility is still deemed important at some level (e.g., corporations are more accountable than ever for potential health risks posed by their products), that has not affected the increase in legal excuse-making on the part of the individual.

The number of ways in which people can be categorized as minority members has also increased dramatically in recent decades. In the 1960s, minority status tended to be based on relatively concrete attributes that people had no control over, such as race, sexual orientation, gender, and, to a lesser extent, religion. Now, however, virtually every person in the United States is a member of multiple minority groups, and membership in many of these groups provides a ready-made excuse for poor performance, health, affect, self-esteem, and many other negative traits, behaviors, and outcomes. As Charles Sykes (1992) argued, everyone these days feels like a victim in one way or another, and many transgressions can be at least partly blamed on real or perceived discrimination.

In education, the number of students who are being classified as "learning disabled" has increased exponentially since the invention of the term in the 1960s. By 1996, between 7% and 15% of students were labeled as "learning disabled" (Sykes, 1996). Some psychiatrists emphasize that, although some learning disabled students have distinct physiological

characteristics, the vast majority have no neurological deficits, identifiable genetic influences on their disorder, and no clear clinical descriptions (e.g., Breggin, 1991). Whether or not these students actually deserve their label is irrelevant to this point: giving excuses to students who are failing is more common now than it used to be. Similarly, clinicians have defined substantially more psychological disorders and diagnosed a substantially higher percentage of the population with these disorders than ever before. Psychiatrist Irwin Savodnik (2006) points out that, in the last 50 years, the number of identified mental illnesses has risen from 107 to 365, and this is entirely due to changes in what is deemed "normal" by society, not by an increase in actual disorders. The result of this tremendous increase in mental disorders is a reduction of accountability for negative outcomes (Savodnik, 2006). In other words, the more mental disorders there are, the more likely someone is to be able to use one as a realistic excuse for committing a crime, failing in school, sabotaging personal relationships, or any other type of problematic behavior. Again, whether or not these disorders are legitimate is beside the point—the point is that there are more excuses available to people than there used to be, and people seem to be very willing to use them, regardless of whether they end up better or worse off for the experience.

Even measures of traditional psychological variables have shifted more towards excuse-making attributional styles. For example, scores on Rotter's (1966) locus-of-control scale have shifted almost a full standard deviation to the external side of the scale since 1960 (Twenge, Zhang, & Im, 2004). Relative to those with a more internal locus of control, those with a more external locus of control expect that they have less control over achieving desired goals, because they believe that random interference and other external influences will have a large effect as

well. This pattern indicates a trend toward perceptions of less control in goal achievement—an important component in excuse making (Schlenker, Pontari, & Christopher, 2001).

It is important to emphasize that the purpose of this introduction and the studies that follow is not to condemn the use of excuses as accounts for behavior. Social psychological research has demonstrated that, in many cases, excuse-making attributional styles can alleviate negative affect (Mehlman & Snyder, 1985), protect self-esteem (Crocker & Major, 1989; Major, Kaiser, & McCoy, 2003; McFarland & Ross, 1982), lower the risk of depression (Peterson, Luborsky, & Seligman, 1983; Peterson & Seligman, 1984; Raps, 1982), and improve health (Snyder & Higgins, 1988), among other things. Everyone uses excuses, and, in general, excuses seem to be very effective at improving the outlook, performance, and health of the user as well as improving the user's impressions in others. On the other hand, there are costs associated with excuses, such as when a socially acceptable excuse leads to worse performance on an uninteresting, unmotivating task (Johnson & Schlenker, 2008), or when flimsy excuses make the user appear less reliable and more narcissistic (Pontari, Schlenker, & Christopher, 2002). Therefore, it would be simple-minded to argue that a shift in a society toward more excuses is clearly a good or bad thing in general; it simply is what it is.

However, because more excuse making can have clear and identifiable costs and benefits, it is important to determine some of the primary causes for a shift in personal responsibility. It is likely that a decrease in the importance of personal responsibility is multiply determined, and it is unlikely that any one cause is clearly the primary one. Therefore, although there are surely many variables that could cause more excuse making, the studies presented will focus on the increasing number of options in an increasingly complex society. This variable has the

advantage of being easy to identify in society at large and easy to manipulate in a laboratory setting to determine what causal effects it may have on personal responsibility.

Decision Making and the Lure of Choice

Over the past 40-50 years, excuse making has covaried with an increase in the daily necessity of sifting through ever-increasing amounts of information and choosing from increasingly large choice sets. Decisions such as choosing a car or deciding which career to pursue have become significantly more complex than they were decades ago, in large part because there are so many more options than there used to be. Tasks that used to be relatively straightforward and limited (e.g., making a phone call) now carry with them a host of auxiliary decisions (e.g., which carrier is best?; cell phone, land line, or ISP?; block caller ID?; etc.) and necessitate even more decisions once the call is made (e.g., voicemail services offer multiple options such as leaving call-back numbers, forwarding numbers, shipping information, text messages, and/or voice messages). The proliferation of mail-order catalogs, home-shopping networks, and the internet gives shoppers the option of considering an almost limitless number of products that can be examined, purchased, and shipped without ever leaving home. Objectively, this pervasive presence of huge amounts of information and huge amounts of alternatives when making decisions has given people more control over many of their day-to-day decisions. When choosing virtually any well-established product, today's consumer has a great deal more control over the exact specifications and attributes of that product than ever before. However, given that general perceptions of control over life's outcomes have decreased (Twenge et al., 2004) along with the increase in objective control, it is possible that the relationship between actual control and perceived control may not be as simple as it seems. Although there are certainly countless other variables that have had some impact on generational changes in perceptions of control, the

relatively common necessity of highly complex decision making may still have an important impact on those perceptions.

Is a large amount of control and agency in decision making a good thing? Since Seligman's (1975) research on learned helplessness, much of traditional psychological research has supported the notion that having no control (or the perception of no control) over the outcomes in one's life can lead to lower motivation, well-being, confidence, and other negative outcomes (e.g., Cordova & Lepper, 1996; Deci, 1981; Deci & Ryan, 1985; Deci, Spiegel, Ryan, Koestner, & Kaufman, 1982; Langer, 1975; Langer & Rodin, 1976; Perlmuter & Monty, 1977; Schulz, 1976; Taylor & Brown, 1988; Zuckerman, Porac, Latin, Smith, & Deci, 1978). More specifically, decision-making researchers have found that a lack of control over important, self-relevant decisions leads to negative affect. For example, Ackerman and Gross (2006) found that, relative to students with no control, business students who were asked to choose their classes for a hypothetical marketing minor (as opposed to having their classes chosen for them) reported more satisfaction, desire for the option, and happiness, and they reported less stress and regret.

On the other hand, a smaller collection of research has shown that, although some control usually creates better outcomes than no control, it is not without its consequences. Burger (1989), for example, argued that negative consequences of control have been all but ignored in the literature, and he identified several variables that may be negatively affected by increased control, such as health, performance, and affect. Since Burger's review, empirical studies have confirmed many of his predictions. For example, illusions of control have been negatively associated with trading performance in professional stock traders (Fenton-O'Creevy, Nicholson, Soane, & Willman, 2003), and more job control has been associated with worse somatic health in customer-service representatives with low self-efficacy regarding their job duties (Jimmieson,

2000). Increasing control in general appears to be a mixed bag of consequences which vary according to moderating variables. Burger correctly predicted that a variety of negative outcomes can result in situations where extensive perceptions of control are not warranted (e.g., stock trading) or more control means more accountability in a situation of low self-efficacy (e.g., unconfident customer service reps).

Up to this point in this literature review, decision-making control has been described in fairly vague terms, although there are many ways to define and operationalize control. For example, one specific way to increase control is to increase the number of decisions a person can make along the way to achieving some end. Someone who gets to choose the destination, airline, seat numbers, and hotel when planning a vacation should logically be said to have more control than someone who only chooses the destination. Interestingly, irrespective of knowledge regarding the positive impacts of control, people generally appear to associate more choice with more positive outcomes, and, if given a choice, they generally prefer the path that leads to as many decisions as possible. In fact, even animals prefer the option of making more decisions. Rats (e.g., Voss and Homvie, 1970) and pigeons (e.g., Ono, 2000) prefer to make more decisions even when the outcome is exactly the same as with fewer decisions, and primates (e.g., Suzuki, 1999) also prefer to make more decisions, as long their apparent chances of success are at least as good as with fewer decisions.

Humans also have inefficient preferences regarding choice, preferring to take the path of more decisions when the odds of success are the same (Beattie, Baron, Hershey, & Spranca, 1994) or even when they are worse (Bown, Reed, & Summers, 2003). Ultimately, contrary to people's expectations when choosing how many decisions they want to make, making more decisions leads to less satisfaction before the outcome of the decisions are even known (Botti &

Iyengar, 2004). In summary, people prefer having more control in their ability to make decisions for themselves, and they prefer maximizing the number of decisions they get to make even when it results in no advantage over fewer decisions.

Although there are certainly many other ways to define control when referring to decision making (e.g., the total number of decisions made, the total number of attributes or qualities of decisions, the social pressures associated with making certain decisions), I will only focus on one—the number of options when making a choice. The primary reason for this focus is that option number is the clearest operationalization of control that can be said to have increased along with excuse making at a societal level. Although it is relatively clear that people in American society currently have more options from which to choose when making typical decisions (e.g., buying products, choosing educational institutions, pursing career options, choosing leisure-time activities, etc.) than ever before, it is much less clear that people have more control in other ways (e.g., the number of discrete decisions that people make each day). One purpose of this research is to determine a potentially important causal variable that can explain the increase in excuse making that has occurred in recent decades, and option number is the only control-related variable that can be quantifiably determined to covary with excuse making in society in recent decades.

There is less research on the topic of option number than there is on the topic of general control in decision making (i.e., the difference between having some control vs. no control), but the results seem generally consistent. Initial research on the topic showed that, similar to general preferences for personal decisions and the largest number of decisions possible, people seem to prefer more options (Kahn & Lehmann, 1991) and more thoroughly enjoy the decision-making process when more options are offered in relation to fewer (Iyengar & Lepper, 2000), and more

recent studies confirmed this pattern (Kahn & Wansink, 2004). However, although people seem to prefer as many options as possible, having more options actually causes people to be less likely to make a decision at all (Chernev 2003a; 2003b; 2006; Iyengar & Lepper, 2000; Tversky & Shafir, 1992). For example, Boatwright and Nunes (2001) showed in a field study that people were more likely to buy products in a grocery store as the number of options increased, but that trend flattened out and eventually reversed as the number of options became too large. Chernev (2003a; 2003b) showed that this effect of too many options reducing preferences is moderated by the salience of a premeditated ideal option. In essence, although it appears that people enjoy having variety, excessive variety also makes the decision process more difficult and often leads people to avoid making a final decision at all.

Bown et al. (2003) described and compared three similar characteristics of decision-making control that could be described as eliciting the "lure of choice": (1) number of options, (2) number of independent decisions, and (3) number of decisions about each attribute of a given group of options. The authors made the case that the lure of choice resulting from having a relatively larger number of separate decisions to make is different from the lure of choice that results from having a relatively larger number of separate options to choose from when making a single decision. For example, it is possible that a person could be lured by a store display with a large number of alternatives (e.g., a display of 50 different pre-made greeting cards) because of the opportunity to exactly match his/her exact, preferred specifications. However, that same person might not be lured by a store display that required a sequence of decisions that led to a previously undefined product (e.g., a make-your-own greeting card station that requires sequential decisions about paper type, paper size, paper color, font, ink color, graphics, etc.). This difference in preferences could exist even if the sequence of decisions in the second

example and the comparison of attributes in the first example each required the same number of separate considerations, mental effort, and distinct decisions. The third type of characteristic (i.e., the degree to which attribute decisions are divided into smaller and smaller units rather than single, holistic judgments) will not be addressed in detail here, because it not as relevant to the purpose of this research.

The lure of choice is demonstrated in each of the three types of situations, because people tend to prefer more choice in all three (Bown et al., 2003), but because they do not necessarily reflect the same processes, each deserves independent testing. This research will focus on effects related to large numbers of options in a given decision rather than other manifestations of decision-making control for these reasons: there is less research on the relationship between option number and responsibility than between general decision making and responsibility, and the option-number manifestation is most relevant to the increase in decision making in society at large.

Option Number and Information Overload

Information

When considering how an increase in options could lead to any psychological outcome, one must determine what characteristic(s) of larger choice sets makes them different from smaller choice sets. The most noticeable characteristic of larger choice sets is that they contain more options and, consequently, necessitate more considerations, comparisons, and cognitive load than smaller choice sets. For the purposes of this research, the aspects of a choice set that result in this additional cognitive load will be loosely referred to as information. There are many ways to define and quantify the amount of information in a choice set, such as the actual or perceived number of options, attributes of the options, and the distribution (i.e., relative frequency) of attributes among the options. Researchers have used these and other aspects of

choice sets both separately and in combination (e.g., Kahn & Wansink, 2004; Keller & Staelin, 1987; Lurie, 2004; Malhotra, 1982), but it is unclear which, if any, is the most important. However, before dealing with which method of measuring information is the best, it is important to emphasize why information is important.

Researchers have used the term "information overload" to describe what happens when a decision maker must make a decision in the presence of more information (in whatever form) than he/she can effectively process. Jacoby and his colleagues (Jacoby, Speller, & Berning, 1974; Jacoby, Speller, & Kohn, 1974) were the first to study the idea of information overload in consumer-choice situations. They found that people made worse decisions when there was too much information to process adequately. Their initial results were criticized for methodological reasons, but subsequent researchers (e.g., Diehl, 2005; Lurie, 2004; Keller & Staelin, 1987; Malhotra, 1982) used a variety of different methods and operationalizations of decision quality to replicate the finding that more information beyond some ideal amount of information leads to worse decisions. In general, researchers have found that decision quality is positively correlated with information up to a level of information that is not cognitively overwhelming, but, once the amount of information exceeds the amount that can be easily processed, decision quality declines as more information is added.

Although the objective quality of decisions is not of interest in the studies presented, it is reasonable to assume that objective quality and subjective experience (e.g., regret, responsibility, satisfaction, decision confidence, etc.) would be correlated. It is therefore important to examine the effects of information on choice quality, because there is so little research on the subjective, psychological effects of decisions made from too much information in the form of too many

options. So, given that decision quality decreases beyond some critical amount of information, what exactly is the best way to define and quantify information?

Information quantity. There are at least three ways that information in a choice set can be increased: (1) increasing the number of options (e.g., 5 vs. 10 shirts in a choice set), (2) increasing the number of attributes (e.g., 10 shirts that differ on size and color vs. 10 shirts that differ on size, color, brand, and fabric), and (3) increasing the homogeneity of attributes such that each option in the set is less distinct from the others (e.g., 10 shirts that are each clearly different from all other shirts in the choice set vs. 10 shirts that are all relatively equivalent to all the other shirts in the choice set). However, research indicates that all three dimensions are relatively independent measures of information and thus do not need to be combined in studies. For example, studies that included factors for number of options and number of attributes did not find significant interactions between those factors (e.g., Jacoby, Speller, & Berning, 1974; Jacoby, Speller, & Kohn, 1974; Lussier & Olshavsky, 1979; Olshavsky, 1979; Malhotra, 1982), and studies that included factors for number of options and distribution of attributes also found no interactions (e.g., Lurie, 2004). Therefore, because it makes sense to treat the three types of information as independent causes of decision effectiveness, the first (i.e., number of options) was chosen as the factor of interest for this research. Future research should include the other two factors to determine if they produce different changes in the kinds of subjective psychological variables that were measured in the current studies.

There is a distinction that is missing in almost every study on option number in choice sets. Studies that compare large and small choice sets do not typically attempt to determine cognitive differences between those conditions that go beyond the number of options, and it is easy to understand how these unidentified confounds could be problematic. When manipulating

the number of options in an experiment with two choice-set conditions (i.e., one condition with a large number of options and one with a small number of options), researchers typically make sure to keep the number, variety, and distribution of each option's attributes the same in both conditions. Although this is a good thing because it assures that the two conditions have options that differ only in number, it confounds information in the choice set and number of options in the choice set, because a small group of options is obviously going to contain less information than a larger group of equivalently complex options. It is therefore impossible to tell if between-condition effects are a result of having more variety, having more information to sift through, or some combination of the two.

Lurie (2004) attempted to address this issue as it relates to decision quality. He computed a measure of uncertainty in "bits," which are the unit of measurement for uncertainty in a choice set as defined by Shannon (1949) in his theory of information structure. Generally, uncertainty is a measure of the average likelihood of an attribute occurring. It can also be thought of as the inverse of information (Garner, 1962). In the case of choice sets and decision making, uncertainty is determined by the number of options, the number of attributes that each option possesses, the number of values or levels of each attribute, and the relative frequency of the levels of those attributes among options across the entire choice set (Lurie, 2004). Lurie used Garner's (1962) equation in an unknown way to control for the amount of information in a group of options by measuring the degree to which attributes of the options were evenly distributed. In short, more evenly distributed options have more uncertainty associated with them, because distinguishing between them is more difficult. For example, if only one car in a choice set of 10 had leather upholstery (a rare occurrence), that attribute would provide more information (i.e., less uncertainty) than if five cars had leather upholstery (a common occurrence) and would

clearly distinguish that car from others in the choice set, meaning that the cars would not have to be examined as closely to differentiate them. In other words, it is cognitively easier to choose from very different options than it is to choose from very similar options, so it is theoretically possible to have one choice set with 10 options that is as easy to process as a choice set with 15 options as long as the second set has fewer or less evenly distributed attributes.

Lurie found that distribution of attributes among options and the number of options both independently affected decision quality, and, when uncertainty (measured in bits) was controlled in a regression equation that included number of options, uncertainty remained significant and number of options did not. Thus, he argued that uncertainty (i.e., information) was the best predictor of decision quality, and information structure should be clearly defined in terms of bits rather than simply counting attributes. However, there are a number of reasons why a similar approach was not used in this research. First, Lurie did not clearly define how he computed his uncertainty coefficients, so it is unclear how option number is relevant to the computation. Also, other authors only use Garner's original equation to study uncertainty as it relates to binary probability of attributes (i.e., the probability that an attribute does or does not occur), whereas Lurie somehow used the equation (or some unknown adaptation of it) to account for attributes with three levels, one of which always occurs. These ambiguities make it unclear how to replicate his method for calculating bits, which, to my knowledge, has not been used by anyone else.

Second, and more importantly, a measure of uncertainty defines the occurrence of each attribute the same way, thereby treating all attributes and their levels as equal, interchangeable parts. Although this manner of standardization may be straightforward and parsimonious in a mathematical sense, treating the levels of each attribute the same across attributes leaves out

critical information contained in the units of the attributes themselves. In other words, the exact levels of each attribute and the units of those attributes are equally important for decision making in the real world, so standardizing them takes away a very important piece of information for decision makers. For example, consider the task of choosing a new cell phone from a group of other cell phones that are the same on every attribute except one—talk-time battery life. Suppose that the phones to be considered have three possible levels of the battery-life attribute: 2, 3, and 4 hours. Those options for battery life are pretty low by today's standards, and some are lower than the maximum amount of time a phone conversation could conceivably take, meaning that the 4-hour version would be the only one that was acceptable to many choosers. Now, suppose the phones have these levels of battery life: 8, 12, and 16 hours. Mathematically, these two scenarios are equivalent, because the battery lives differ by the same proportion. However, battery life is a less important attribute in the second situation, because all three phones have beyond the industry's average battery life for a phone, and it unlikely that anyone would need to talk on a phone for 8 or more hours without recharging. Therefore, taking attributes out of their context to compute uncertainty leads to the loss of a critical piece of the puzzle for determining the importance of attributes. This problem has been cited by some as the reason why the calculation of information-structure coefficients has never become popular in psychology in the way that it has in engineering, biology, physics, and statistics (Luce, 2003). In summary, measuring information with finite coefficients is a strategy that is especially problematic when measuring the effects of those coefficients on subjective psychological variables, so that strategy was not used in the studies presented. Rather, counts of option number in each choice set will be the only measure of information, and further unique manipulations of information can be used in future research as needed.

Information quality. The general quantity of information is obviously important, but what about the quality? Quality can be defined as the information's inherent usefulness in assessing the utility of an option (Keller & Staelin, 1987). For example, suppose one is choosing a new desktop computer for work and has access to three attributes about potential computers in the choice set. The information would be higher quality if those attributes were processing speed, memory, and hard-drive size than it would be if the attributes were color, texture, and shape, because color, texture, and shape are less informative about the utility of a computer than processing speed, memory, and hard-drive size. Keller and Staelin (1987) showed that decision quality is increased with higher quality information (holding quantity constant) and decreased with higher quantity information (holding quality constant), but more importantly for this research, decision confidence was increased with higher quality information (holding quantity constant) and decreased with higher quantity information (holding quality constant). However, higher information quality only increased decision quality to a point, and then the increase in decision quality leveled off, although increases in decision confidence did not level off. This pattern indicates that, as long as the attributes of a choice set are of sufficiently informative quality to make a good decision, they stop making a difference in decision quality. Thus, it is conceivable that, if a researcher provided attributes that were high-quality enough, he/she could measure differences in information quantity without concern for any main effects or interactions involving information quality. The goal of the presented studies was to do just that: provide enough quality information about all of the options that quantity of options could be manipulated without interference from quality-related issues.

Option Number and Excuse Making

In his aptly titled *The Paradox of Choice: Why More is Less*, Barry Schwartz (2004) argued that people with virtually limitless options in life may feel overwhelmed at times and

experience a host of negative outcomes as a result. He gave several examples of the kinds of variety people encounter when making even the simplest decisions. On a trip to the grocery store, for example, Schwartz noticed that there were 285 different varieties of cookies, 360 kinds of shampoo, and 275 different breakfast cereals. He went on to point out that the huge increase in choices and personal freedom goes far beyond the consumption of material goods. For example, the American higher-education experience has shifted away from being mostly required courses to being mostly courses from which students can choose a huge variety of options. Even the most traditional institutions (e.g., Harvard, Princeton) now offer as many as 350 class options for fulfilling general-education requirements, whereas a student's options for general education were very limited in the past (Schwartz, 2004). This trend is perhaps not surprising, given that people generally tend to prefer having as many options as possible when making choices (Broniarczyk, Hoyer, & McAlister, 1998; Chernev, 2006; Kahn & Lehman, 1991). After citing an impressive list of other ways in which society must deal with more options now than ever before (e.g., hundreds of cable channels, complex health care and retirement plans, multiple utilities options, career options, etc.) and the ways that current values and advertising strategies encourage the quest for more choice (e.g., the recent allowance of direct marketing of pharmaceutical companies to consumers puts the burden of drug choice on patients), Schwartz argues that having too many choices can be problematic for psychological outcomes such as satisfaction and regret.

Recent empirical research provides support for the notion that increasing a person's freedom to choose is only beneficial up to a point, and more choice beyond that can lead to negative affective states. For example, having a large number of options to choose from when making a decision leads to feelings of increased regret, decreased satisfaction, and increased

frustration (Haynes & Olson, 2007; Iyengar & Lepper, 2000). Moreover, larger numbers of options also cause people to be less confident in their choices (Chernev 2003a; 2003b; Iyengar & Lepper, 2000) and less likely to be willing to make a choice at all (Chernev 2003a; 2003b; Iyengar & Lepper, 2000; Tversky & Shafir, 1992). However, although there have been a small handful of studies regarding the effects of a large number of options on regret (Haynes & Olson, 2007; Iyengar & Lepper, 2000) and "self-blame" (Botti & McGill, 2006), there has been no research on the effects of excessive options on excuse making and responsibility, and it is that relationship that this research addresses.

Regret and Responsibility

At first glance, a casual reader might ask why it is necessary to determine the relationship between option number and excuse making. The relationship between option number and regret has already been established; doesn't that pretty much sum it up? The answer, which is no, is based on the fact that theoretical and empirical links between regret and responsibility are somewhat unclear, and the relationship between regret and excuse making is virtually unexplored.

Research on regret and responsibility is somewhat divided, because researchers disagree on the extent to which feelings of regret necessitate feelings of personal responsibility. On the one hand, most researchers regard regret as something that cannot exist without the perception of responsibility (Gilovich & Medvec, 1994; van Dijk, van der Pligt, & Zeelenberg, 1999; Zeelenberg, van Dijk, & Manstead, 1998). According to this perspective, regret is a negative affective state resulting from the belief that one should have acted differently to change a negative outcome over which one had control. Embedded in this perspective is the notion that some decisions are "wrong" in retrospect, meaning that the actor should have known better and is thus responsible for the negative outcome (Zeelenberg, van Dijk, & Manstead, 1998). Moreover,

other negative affective states that may occur following a negative outcome (e.g., disappointment) are not the same as regret and thus do not require the presence of responsibility.

On the other hand, other researchers have argued that feeling responsible for a bad outcome is not necessarily a requirement of regret (e.g., Connolly, Ordóñez, & Coughlan, 1997; Simonson, 1992), because it is possible to regret an action without feeling very responsible for it. For example, regret and responsibility can diverge when making a decision between a risky but potentially very valuable option and a safe but much less valuable option (Simonson, 1992). If one chooses the risky, valuable option and it doesn't work out, then one would regret the decision and feel responsible for it. On the other hand, if one chooses the safe option and it doesn't work out, then one would regret the decision because of the missed opportunity, and one would not feel as responsible for it, because going with the safe option was obviously the best decision (Simonson, 1992). The issue has continued to be debated by philosophers and scientists with some even changing sides, such as when Ordóñez and Connolly (2000) essentially retracted their argument that responsibility and regret have no relationship. The inconsistency of these results stems from three problems: (1) the definition of regret determines whether or not responsibility must play a role, (2) the focus of the questions given to participants (e.g., the behavior that lead to the outcome vs. the outcome itself) greatly influences responses, and (3) participants simply cannot (or do not bother to) distinguish well between related affective states such as disappointment, regret, and sadness. The first two problems are difficult to remedy, because no one has developed a reliable, valid measurement of context-specific (i.e., state) regret (Schwartz et al. [2002] developed a measure of trait tendency for regret). However, the third problem makes the first two essentially irrelevant. Connolly and Butler (2006) demonstrated that there were high correlations between common measures of regret, disappointment and sadness that

people predicted they would feel and actual regret, disappointment, and sadness that people later reported feeling. The same high correlations were found between positively-valenced predictions and actual experiences of rejoicing, elation, and happiness. Their conclusion was that people do not differentiate their emotions clearly enough to effectively distinguish between the related concepts of regret, disappointment, and sadness. The implication is that a general affective measure would be more useful.

Researchers generally agree that regret requires perceptions of responsibility, whereas disappointment does not (Zeelenberg et al. 1998). However, if it is the case that regret, disappointment, and sadness are so closely related, then minor methodological differences (i.e., the nature of the questions being asked or the wording of the items) could account for differences between them, when, in fact, they are essentially all highly correlated components of a general, negative experience. Therefore, although disappointment, regret, and sadness differ notably when carefully defined as hypothetical constructs by social scientists, they may not differ in consistently meaningful ways for lay people—at least, not until researchers develop reliable, valid scales that differentiate them.

This definitional issue is relevant to the research being presented because, although a small handful of studies involve option number and regret, those studies do not necessarily inform excuse making, and the primary purpose of the proposed studies was to determine the relationship between excuse making and option number. Botti and McGill (2006) conducted the only study in the literature that contains references to responsibility and information, but the manipulations of information (i.e., more or less differentiation of option attributes rather than more or fewer options), the characteristics of the tasks (e.g., all possible outcomes were known to participants to be unpleasant before the task even started), and the measurements of

"responsibility" (responsibility defined as "self-blame"—an operationalization that is in a conceptual grey area between excuse making and regret) were dramatically different than anything presented in this research. Therefore, the presented studies involving different-sized choice sets and excuse making will provide useful information about a behavior that cannot be inferred from existing research.

That said, it is likely that excuse making would be at least somewhat related to regret, disappointment, or some composite measurement of post-decisional affect. As such, Study 1 examined this potential relationship by measuring how excuse making is affected by post-decisional affect, number of options, and the interaction of the two factors. All research on the effects of excessive options on regret (or similar post-decisional affective measures) have used regret as an outcome variable in combination with other outcome variables. However, these studies did not attempt to determine the direct effects of regret on other outcome variables, but, rather, considered the effects on each outcome in isolation. It is possible that post-decisional affect is an important causal force that leads to a variety of outcomes including excuse making, but post-decisional affect must be manipulated through random assignment to most accurately determine if that is the case (Spencer, Zanna, & Fong, 2005).

Persistence of Excuse Making Over Time

Up until this point, this literature review has implied a relationship that has not yet been explicitly stated. Namely, it is possible that getting into an excuse-making attributional style as a result of negative post-decisional affect and excessive options could cause one to maintain that excuse-making attributional style over time. In other words, a person who feels he/she must make excuses to account for an ostensibly bad decision may then be more likely to make excuses about other, unrelated bad decisions in the future. Therefore, it is possible that the simultaneous increase in both option number and excuse making over the past several decades may be more

than just a correlational relationship. It could be that the pervasive presence of excessive options and excessive decisions causes people to make more excuses, and making more excuses causes a more permanent shift toward an excuse-making attributional style.

Research on excuses typically has examined the construct by addressing three issues: (1) what causes people to make more excuses (e.g., Major, Kaiser, & McCoy, 2003; Schlenker, 1980), (2) what interpersonal and intrapersonal outcomes covary with making more excuses in a specific context (e.g., Brown & Josephs, 1999; Crocker & Major, 1989; Melhman & Snyder, 1985), and (3) what variables covary with chronic excuse-making attributional styles across contexts (e.g., Follette & Jacobson, 1987). However, no one has attempted to determine experimentally if external influences can facilitate excuse making that then persists on subsequent, unrelated tasks. This effect is not difficult to imagine, because research has shown that there are many ways in which exhibiting a behavior can increase that behavior's frequency in the future. Research on catharsis, for example, shows that acting aggressively has a priming effect that promotes further aggressive thoughts and actions in the future (Anderson, Carnagey, & Eubancks, 2003), and increasing perceptions of public acceptance of catharsis also increases later aggression (Bushman, Baumeister, & Stack, 1999). More generally, people are usually expected to be consistent in their self-presentations, so public commitments of any kind (e.g., behaviors, expressed attitudes, attributional style, etc.) tend increase the probability of similar actions and cognitions recurring in the future (Schlenker, 2003; Schlenker, Dlugolecki, & Doherty, 1994; Tice, 1992). This issue of chronic excuse making resulting from external influence was addressed in Study 2 by manipulating the presence of a believable, socially acceptable excuse (thereby mimicking social norms of excuse availability and acceptance) and the number of options in a choice set.

Goals and Hypotheses

Until this point, my review of the literature has implied an inverted-U relationship between option number and psychological outcomes such as regret, satisfaction, and stress. The implied relationship suggests that psychological outcomes improve as one's control increases from nothing to something, but after some variant level of control, adding additional control actually starts to harm those outcomes. This relationship between information overload and detrimental psychological outcomes follows from the finding that more information leads to worse decisions and worse psychological outcomes past the point of information overload (Diehl, 2005; Lurie, 2004; Keller & Staelin, 1987; Malhotra, 1982). For example, Ackerman and Gross (2006) showed that students who were asked about foundation courses in marketing reported the lowest regret and stress regarding their courses when they got to choose one third of their courses relative to those students who got to choose none or all of their courses. However, the ascending part of the curvilinear relationship was not tested in the current studies, because there has already been a great deal of research on the beneficial effects of control on psychological outcomes when conditions of no control have been compared to conditions of some control (e.g., Cordova & Lepper, 1996; Deci, 1981; Deci & Ryan, 1985; Langer & Rodin, 1976; Perlmuter & Monty, 1977; Schulz, 1976; Taylor & Brown, 1988; Zuckerman, Porac, Latin, Smith, & Deci, 1978). More research on this same issue is not really necessary.

At this point, one might argue that I contradict myself with this statement, because the previously cited research does not define control as option number, and I made a point of arguing that research on other operationalizations of control does not necessarily generalize to option number. However, this criticism does not present a problem, because there are clear, conceptual differences between studies that compare choice sets of various sizes and studies that compare conditions of some choice vs. conditions of no choice. More specifically, a study that compares

a small-option condition with a large-option condition is comparing degrees of control, whereas a study that compares a small-option condition with a no-option condition is comparing some control and no control. Research is well established on the comparison between some control and no control, but research is not well established on the comparison between some control and too much control (i.e., information overload). Therefore, although adding a no-option condition to the proposed studies would serve to replicate findings on the control vs. no control issue, the findings would not be likely to add anything meaningful to existing literature.

If a no-option control condition is omitted from the design, and a curvilinear relationship is hypothesized, then it critical to establish the small-option condition at or past the peak of the information-overload curve. If one chooses a number of options for the small-option condition that is on the ascending part of the curve and a number of options for the large-option condition that is on the descending part of the curve, then one might find no difference between the two conditions, thereby missing an important relationship. Ideally, one must therefore choose option numbers for all conditions that will fall on the same side of the curve to provide an opportunity to detect any linear pattern that may exist. In the information-overload section of this proposal, I postulated that, because people make worse decisions when overloaded with information, certain negative psychological outcomes such as regret and disappointment would also be more likely. Therefore, to adequately test relative differences in negative psychological outcomes (and related outcomes such as excuses), all conditions needed to include at least enough options to cause information overload. In the case of the current studies, information overload was defined as more options and/or attributes than participants could adequately process in the allotted time, even if they did have time to actually see all the information. Failing to achieve information overload in the condition with the smallest number of options would mean failing to choose a

condition at or past the peak of the curve, because the top of the curve should occur at the point of information overload. Research on decision making as it relates to option and attribute number has suggested that people cannot adequately process more than approximately six (Bettman, 1979; Wright, 1975) to 10 (Malhotra, 1982; Streufert, Driver, & Haun, 1967) pieces of information (attributes or separate options) at once when choosing between options. All conditions in both studies therefore had more than this minimum requirement to ensure information overload, because the condition of the smallest information contained six options with three attributes each, thereby equaling 18 pieces of information.

In summary, replicating the ascending part of the curvilinear relationship between option number and excuses (i.e., the difference between having no options and having a small number of options) would not add much to extant literature, whereas the descending part of the curvilinear relationship is much less established and does necessitate further research. There is scant literature on the effects of small-option-number vs. large-option-number conditions on any variables, so any additional research would be informative. The next section includes hypotheses that expand on the notion that large numbers of options may actually lead to feelings of less responsibility.

Study 1

Study 1 was a 3 (moderate-arousal feedback, low-arousal feedback, or no feedback) × 2 (option number: 6 or 18 options) completely between-subjects design. Arousal feedback was manipulated to facilitate a manipulation of post-decisional affect. All participants were given a limited amount of time to choose one car that was as close as possible to a given standard, and all participants were given feedback indicating that they did a relatively poor job. If all participants receive negative feedback, then participants on average should be encouraged by that failure to make some sort of excuse to protect their public image and self-image, and differences in their

excuse making were thus examined. All participants were assumed to be feeling somewhat negative affect after the failure feedback (college students typically do not enjoy failure feedback on intellectual tasks), and the affect manipulation was intended to manipulate the intensity of this negative affect. Negative-affect intensity was manipulated through false physiological feedback regarding participants' arousal during and after the task.

Specifically, Study 1 was intended to answer two questions: (1) do excessive options cause more excuse making, and (2) does the intensity of negative post-decisional affect cause differences in excuse making independently of and in combination with different-sized choice sets? Hypotheses follow for each factor in isolation as well as their interaction.

Post-decisional affect. Research indicates that participants who receive false physiological feedback often internalize and interpret that feedback such that their actual feelings end up in the same direction as the bogus feedback. For example, Batson, Engell, and Fridell (1999) showed that participants who were given false physiological feedback indicating that they were very physiologically aroused ended up feeling more aroused than participants who were given feedback indicating that they were less aroused. Moreover, similar false physiological feedback has been shown to affect feelings of empathy (Batson, Turk, Shaw, & Klein, 1995), judgments of attractiveness (Valins, 1966), ratings of emotionality (Wilkins, 1971), and attitudes regarding objectionable pictures (Parkinson & Manstead, 1981). These studies collectively indicate that manipulating arousal feedback not only affects participants' perceptions of their arousal but also leads participants to look for some cause to which they can attribute that arousal.

In Study 1, participants were given failure feedback, so it was assumed that the most salient attribution they could make for a particular level of arousal would be that the failure feedback was responsible. Thus, it was expected that participants who received physiological

feedback indicating higher levels of arousal immediately after their performance feedback would be more likely to interpret that physiological feedback as an indication that they became relatively upset upon learning of their failure. Alternatively, participants who received physiological feedback that indicated relatively low, unchanging arousal after receiving their performance feedback would interpret that physiological feedback as an indication that they were not particularly bothered by the failure. A no-feedback (i.e., control) condition was also included to get a baseline measure of how participants would respond in the absence of any physiological feedback.

When a failure occurs, people generally account for that failure using four strategies (Schlenker, 1980), including excuses (a denial of responsibility), justifications (acceptance of responsibility along with an explanation that the transgression was not actually a bad thing), apologies (acceptance of responsibility along with signs of remorse), and denials (a denial of responsibility and a denial that anything wrong took place at all). Reasonable excuses have the unique advantage of facilitating an image of effectiveness after a failure (Schlenker et al., 2001). Also, in addition to a negative impression, negative affect is also likely to be present after a failure, and an excuse would be an effective way to manage that as well. Research on excuse making and affect shows that excuse-making (i.e., external) attributional styles can often reduce negative affect following a failure (e.g., McFarland & Ross, 1982; Mehlman & Snyder, 1985; Ouwerkerk & Ellemers, 2002). Given that excuses can reduce negative affect, one can make the following hypothesis:

- **Hypothesis 1:** More negative post-decisional affect will lead to more excuses.

Number of options. There are a number of reasons to expect more excuse making under conditions of excessive options. As previously stated, excuses provide a way to reduce negative

affect resulting from a mistake, so assuming the decision doesn't work out, an excuse would help people feel less negative affect and less ineffective in their decision-making abilities, and negative affect should be highest in the condition with the most options (Iyengar & Lepper, 2000). In addition, believable excuses can reduce the negative opinions of others following a mistake (Schlenker et al., 2001), and excuses become more believable as the difficulty of the task (e.g., the more options one must consider) increases. In other words, excuses reduce several types of negative outcomes following a poor decision, and excuses may be more believable under conditions of an excessive number of options than under conditions of a more reasonable number of options, thereby increasing their effectiveness for intrapersonal and interpersonal goals.

Research on retroactive pessimism (i.e., the tendency to assess one's chances for success more pessimistically after a poor decision than before the decision) provides more insight into why more options may lead to more excuses. Although it is described in a relatively different context, retroactive pessimism basically achieves the same end as an excuse when accounting for a poor decision—to make a disappointing outcome more palatable (Tykocinski, 2001). For example, Tykocinski (2001) showed that students who incorrectly chose the winner of an upcoming election later increased their estimate of the winner's likelihood of victory, whereas students who correctly chose the winner showed no such shift. Similar research (Tykocinski & Steinberg, 2005) shows patterns of retroactive pessimism that correspond in many ways to lack-of-control excuses (Schlenker et al., 2001), because saying that one's probability of success was relatively low in hindsight is very similar to saying that one did not have a great deal of control over the outcome. Some have even classified certain types of excuses as "counterfactual

excuses," thereby linking the notion of counterfactual thinking and excuse making (Markman & Tetlock, 2000).

On the other hand, one could at first glance potentially generate reasons to expect fewer excuses with more options, but this reverse pattern is unlikely to actually occur. For example, having more options could be argued to be synonymous with having more responsibility in an objective sense, because having more options means a greater ability to choose exactly what one wants. However, although this may be true in an objective sense (and even that is arguable), people in these situations could just as easily argue that having more options actually lowers control over the ultimate outcome, as long as there are enough options to lower one's chances of adequately processing all the information, thereby lowering the probability of a good decision. This type of lack-of-control excuse would work nicely in a sufficiently large choice set, because excuses that cast the actor as failing because of circumstances out of his/her control are generally effective at reducing negative impressions (Schlenker et al., 2001). Similarly, excuses related to low prescription clarity (i.e., the clarity of the goals, rules, or scripts for success in a situation; Schlenker et al., 2001) would also be effective, because one could easily argue that the large amount of information made it unclear how to find a successful strategy for assimilating it.

One might instead argue that regret has been shown to increase in the presence of a large number of options, and regret has been linked to more responsibility by some researchers (Gilovich & Medvec, 1994; van Dijk, van der Pligt, & Zeelenberg, 1999; Zeelenberg, van Dijk, & Manstead, 1998). If people are feeling more responsibility as the number of options goes up, then one might predict fewer excuses. However, as stated in the previous section, the link between regret and responsibility is still unclear, and it is possible that people can feel regret for a poor decision while still making an excuse to improve their mood and impression on others,

because lay people do not think of regret as necessitating responsibility in the same way as many social scientists (Connolly & Butler, 2006). Moreover, people who can objectively be identified as having the most responsibility are sometimes motivated to make the most excuses (Schlenker & Miller, 1977). Ultimately, because excessive options provide several convenient excuses (e.g., lack of control, lack of prescription clarity, retroactive pessimism) that can serve to reduce the negative affect and interpersonal impressions created by failure, the following hypothesis is warranted:

- **Hypothesis 2:** A larger number of options will lead to more excuses.

Combination. The factors of option number and post-decisional affect were included together in the study to test for an interaction between them. The presence of an interaction will be informative because of the hypothesized processes behind the effect of option number on excuses and the function that excuses serve. If it is true that excuses reduce the negative affect associated with a poor decision from a large number of options, then that effect should be strongest when the negative affect is the highest. Therefore, the following hypothesis is warranted:

- **Hypothesis 3:** There will be an option number × arousal interaction such that excuse making will be highest in the moderate-arousal, 18-option condition.

Study 2

Study 2 was a 2 (excuse or no excuse) × 2 (option number: 6 or 18 options) between-subjects design with task (task 1 and task 2) as a within-subjects factor. As in Study 1, the first task involved choosing a car in a limited amount of time that best represented a given standard, and all participants were given the same failure feedback as in Study 1. The availability of a reasonable excuse was manipulated in the description of the time limit for the task. Some participants were told that they were in a condition with very little time relative to others (excuse

condition), whereas other were told that they were in a condition with a lot of time relative to others (no-excuse condition). In actuality, all participants received the same amount of time for the task. Measurements of excuse making and affect were taken following the first task. Finally, a working-memory task was completed that was the same for all participants. Participants were given failure feedback after that task as well, and more measurements of excuse making and affect were taken.

Study 2 had two goals: (1) determine the effects on excuse making and post-decisional affect of the availability of an acceptable excuse and the number of options and (2) measure the conditions under which an experimentally-induced, excuse-making attributional style persists over time. The first goal reflected an attempt to better understand the process behind any effects of option number on excuse making. The second goal represented an attempt to determine if excuses resulting from excessive options facilitated later excuse making regarding an unrelated task. The overall design was an attempt to reflect relevant real-world conditions in the laboratory (i.e., conditions under which excuses were permissible or not, option number was overwhelming or not, and there were opportunities to make excuses for poor outcomes on more than one task in a given context).

The study necessarily required two tasks to establish two times at which to measure excuses for the within-subjects measurement. Hypotheses follow for the excuse factor alone and in combination with the option-number factor.

Excuses. Recall that in Study 1, post-decisional affect was manipulated through arousal feedback to determine if more intense negative affect caused more excuse making. The hypothesized process in Study 1 was that people who experience more negative affect would shun responsibility for a poor decision as a way of reducing that negative affect. In Study 2, the

presence of a good excuse was manipulated in part to determine its effects on affect. If the hypothesized process and motivation in Study 1 were correct (i.e., that people feel negative affect following a failure, and making a excuses for their failure would reduce that negative affect), then conditions under which excuses are more acceptable should lead to more positive affect and more excuse making in Study 2. In other words, because external attributions increase positive affect following failure (McFarland & Ross, 1982), a believable excuse already present in the decision environment should have reduced negative affect preceding and following the decision, because the excuse should take off some of the pressure before the decision is made and provide a reasonable self-esteem defense after the decision in the event of a failure, which all participants experienced. In addition, because the participants were accountable to others as well as themselves for their decision, the presence of the excuse in the excuse condition should have promoted more excuse making (Johnson & Schlenker, 2008). The following hypotheses are thus warranted:

- **Hypothesis 4:** The presence of an acceptable external excuse will cause less negative affect.
- **Hypothesis 5:** The presence of an acceptable external excuse will cause more excuse making.
- **Hypothesis 6:** More excuse making will be associated with less negative affect.

Excuses (over time) and option number. The presence of a reasonable, socially validated excuse was expected to establish a norm of acceptability regarding excuses, thereby leading to a general climate of more excuses in the future on all tasks in the same experimental context. This was expected to be true even on a second task that was totally unrelated to the first, because a general social context in which excuses are permissible should apply to all events in that context. Making more excuses in the past facilitates making more excuses in the future

for a variety of self-presentational reasons (Schlenker, 2003; Schlenker, et al., 1994; Tice, 1992). Moreover, the creation of a climate of excuses was expected to be amplified by the presence of an excessive number of options, because more options should have led to more excuses in general, and more excuses on one task should have led to more on the next. Following that logic, the hypotheses below are warranted:

- **Hypothesis 7:** Excuse making will be higher after the second task in the excuse condition when compared to the no-excuse condition
- **Hypothesis 8:** Excuse making will be higher after the second task in the 18-option condition when compared to the 6-option condition
- **Hypothesis 9:** There will be an excuse × option number interaction such that excuse making will be highest after both tasks in the excuse/ 18-option condition

CHAPTER 2
STUDY 1

Method

Participants

Two-hundred fifty six undergraduate students (182 females; 74 males) received partial credit toward fulfillment of a research requirement for their introductory psychology course. Students did not know the nature of the study when they signed up for it. One participant was deleted for responding with the maximum value for every excuse-related question, including those that were reverse scored, which indicated that he was not reading the questions.

Instruments

Excuse making. No one has clearly established the best technique for operationalizing or measuring excuse making behavior, so the scales used in the presented studies contained several different types of items that operationalized excuse making in different ways. All items can be found in Appendix A.

First, a list of relatively independent items gave participants a chance to make several different types of excuses using a variety of scale points (e.g., "how easy was it for you to complete the task given the amount of time you had?"—1 = "Not at all easy" to 9 = "Extremely easy"—and "how much personal responsibility do you feel for your performance on the test?"—1 = "No responsibility" to 9 = "A great deal of responsibility"). The advantage of these items was that each could be answered without as much of a risk that participants would "anchor" their responses according to a scale that was common to all items, and that would make it feasible to analyze each item independently to look at different types of excuse making. For example, the item regarding the clarity of the instructions was designed specifically to tap "prescription clarity excuses." Moreover, because the scale points were so different for each type of item, and each

item assessed relatively independent characteristics, the items did not group together well in factors (exploratory factor analysis of the items across both studies revealed inconsistent, relatively unreliable factors that did not always make clear conceptual sense), so combining the items did not seem to be the best option in analysis of the data.

Second, participants completed a list of items that gave them a chance to reduce responsibility on a variety of dimensions, but all items in this section used the same scale (i.e., 1 = "Hurt my performance a lot" to 9 = "Helped my performance a lot"). In the first grouping of items mentioned above, the benefit of being able to analyze each item independently is offset by the disadvantage of measurement error associated with each item. In the second grouping of items, the common scale allowed items to be analyzed independently or grouped together into meaningful clusters that represented different latent variables of excuse making. These groupings were determined through exploratory factor analysis that revealed three reliable categories across both studies (see Results section). It did not make sense to include the initial set of individual items in these factors, because there was a clear conceptual difference between using a scale that ranged from helping to hurting performance and a scale representing degrees of responsibility from none to a great deal (exploratory factor analysis supported this contention).

Affect. Affect was measured by using a series of 10 semantic differentials (see Appendix A for items). Participants were asked to pick a point along the 7-point continuum that represented how they felt at that particular moment regarding the decision task (as opposed to how they remembered feeling during the task).

Maximization scale. The Maximization Scale (13 items; Schwartz et al., 2002) was included for exploratory purposes, although strong effects were not expected. Some research indicates that maximizers have a tendency to experience a variety of more negative outcomes

after making decisions than satisficers (Haynes & Olson, 2007; Iyengar, Wells, & Schwartz, 2006; Schwartz, 2004). The Maximization Scale measures the extent to which people pursue the "perfect" option when making decisions (Schwartz et al., 2002). People range on a continuum from weak maximizing tendencies (satisficers) to strong maximizing tendencies (maximizers). When shopping for a sweater, for example, maximizers (relative to satisficers) will check more stores, compare prices and other attributes of the sweaters more carefully, spend more cognitive energy trying to get the "best deal," and, ultimately, regret the final purchase more (Iyengar, Wells, & Schwartz, 2006; Schwartz et al., 2002). It is ironic that that maximizers feel less satisfied about their purchases than satisficers, because maximizers are actually better at getting the best deals (Iyengar, Wells, & Schwartz, 2006). Satisficers, on the other hand, shop with a level of quality in mind that is sufficient, and they will purchase the first sweater they see that seems adequate rather than continuing to shop for a sweater that might be slightly better. Given the research on the negative affect experienced by chronic maximizers in the presence of a large number of options (e.g., Haynes & Olson, 2007), it is hypothesized that maximizers will experience more negative affect after a poor decision than satisficers, and that negative affect may lead to more excuses. The pattern of negative affect is especially likely to be evident in the 18-option condition, because the maximizers will have more options from which to choose, and, thus, more opportunities for indecision and counterfactual thinking after the decision. However, the relationship between negative affect and excuse making may not be the same for maximizers as it is for satisficers when the task involves choosing the best option, because that skill is more important to the identity of maximizers. The importance of decision-making skills to the identity of the participants is one of several factors that covary with maximizing tendencies, and those make predictions for excuse making difficult. Still, given that the scale is useful in predicting a

variety of reactions to decisions, any results involving maximization scores should be useful in dissecting the processes behind other effects in the study. See Appendix A for scale items.

Procedure

The design was a 3 (moderate-arousal feedback, low-arousal feedback, or no feedback) × 2 (option number: 6 or 18 options) completely between-subjects design. Maximization was controlled as an individual difference measure, because research indicates that it may moderate the effects of the manipulated independent variables (see Instruments section for hypotheses). Participants completed the study in groups of between one and four. Participants had their own cubicles from which they could not see or interact with anyone else in the room.

When participants arrived, they were asked to sit at a desk with a computer. After they signed the informed consent form, the experimenter handed out instructions explaining the purpose of the study. What follows is a brief summary of how the study was described to participants: "In marketing, researchers often gather information about a certain demographic of people to whom they want to sell products. Through surveys, interviews, and experimental studies, market researchers can even determine exactly what characteristics of products are most important to people. However, even with all this information, market researchers still sometimes make mistakes and recommend the development of products that are not the ideal choice for consumers. We are interested in testing why market researchers sometimes make these mistakes, and that is the purpose of today's study. You will be asked to play the role of a market researcher in which you must choose the best option from a list of hypothetical cars. You will be given a limited amount of time, because market researchers typically work under strict deadlines. The car design you choose will be the one that is built and sold to the public. You will be given information about the relative importance of different attributes of cars to consumers from a

certain group, and you will try to choose the car with the attributes that most closely match the ideal attributes of the consumer population."

The instructions also indicated that the researchers were interested in how physiological changes affect decision making. When participants finished reading the instructions, the experimenter hooked them up to bogus physiological equipment by taping electrodes to their index and middle fingers. They were told that the equipment would be measuring skin temperature, galvanic skin response, and heart rate, and those variables would be averaged together to create an overall measure of physiological arousal. Also, they were told that the measurements would begin as soon as they started the task and would continue until after the task was over. Finally, participants were told that the task had been shown to be a "good predictor of decision-making ability, cognitive speed, general intelligence, and success in business (e.g., business savvy)." This was done to ensure that participants would have a reason to care about their performance.

After the participants were hooked up to the equipment, they began the decision-making task. The task and all dependent measures were administered on a computer using Microsoft Visual Basic 6.0 (see Figure 2-1 for screen shot of car task). The participants saw an instruction screen that reiterated the main points of the study. Then, the computer instructions indicated that the physiological equipment needed to calibrate a baseline level of arousal so that later levels could be compared to the baseline. A clock appeared on the screen and participants were instructed to sit still for 20 seconds while the equipment established a baseline. This was done to make subsequent physiological feedback more meaningful by ostensibly comparing participants' later arousal to their own baseline, thus indicating change rather than abstract, meaningless values.

After this calibration period, participants were given two minutes to compare all the options of all the cars available on the screen. Participants chose from either six or 18 cars, and their condition was randomly assigned by the computer. The time interval (two minutes) was pilot tested to ensure that virtually all participants had time to look at all the options at least once in the 18-option condition. Participants were only able to look at the information for one car at a time, and the program recorded if and how many times they looked at each car as well as how long they looked at each car.[1]

At the top of the screen, there was a list of the three attributes on which the cars could differ and the relative importance of each to the average consumer. The attributes were (range in parentheses) high gas mileage (20 mpg – 30 mpg), high horsepower (150hp – 200hp), and high safety rating (3 stars – 5 stars [.1 star intervals]). The values of each attribute were randomly determined by the computer for each participant. The relative importance of each attribute was ostensibly rated on a 1 (not at all important) to 100 scale (extremely important), but no value was actually be lower than 41 or higher than 65, because having extreme numbers might make choosing too easy. The importance of the attributes was defined as 49, 55, and 61. The

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[1]Time is a crucial issue in any design involving choice sets of various sizes, because bigger choice sets necessarily take longer to process than smaller ones. It is thus impossible to control for time in a way that eliminates all potential confounds. There are three way to deal with the issue of time in studies involving different-sized choice sets: (1) allow the participants as much time as they want, (2) control time proportionally such that larger choice sets have proportionally more time than smaller choice sets (e.g., 2 minutes for 6-option condition and 6 minutes for the 18-option condition), and (3) keep time constant in all conditions. In the current studies, the third option was the most appropriate. The third option most closely approximates the real world, because decision makers cannot usually spend time considering options that is proportional to the number of options. For example, if one needs to purchase a new television because the previous unit broke, one likely will buy a new television within a couple of days regardless of the size of the choice set. Although time spent analyzing options is likely correlated with option number, only in rare circumstances is choice-set size exactly proportionally related to time spent considering options. The first option is inadequate, because it would not eliminate the confound of the 18-item condition taking more time, it would vastly increase the variability between participants in time and effort, and it would make the failure feedback less believable for participants who spend a great deal of time analyzing the problem. The second option is also inadequate, because although it holds time-per-option constant, it confounds total time and option number, and it is less reflective of reality.

computer randomly determined which value of importance went with which attribute for each participant.

Participants were able to see the importance values throughout the task, but they were only able to see the actual values of the attributes for one car at a time. The screen contained either six or 18 car-shaped buttons labeled only by number, and the participant had to mouse over each car to see its attributes. This made quick comparisons and good decisions very difficult. After two minutes, the program stopped allowing participants to see the attributes, and it indicated that they must make a choice to continue with the study.

Once the choice was made, the program appeared to be processing the results for a moment, and then it reported that the participant finished in the 44th percentile (i.e., 56% of other participants had chosen a car that was closer to the ideal car). The actual quality of the choice was possible to compute, but participants could not have computed this themselves, because it is necessary to know the range of values for each attribute, and participants were not privy to that information. The actual quality of the choice is irrelevant to the hypotheses of this research, and it was thus not computed.

After being given their relative performance, some participants were told that they would get a chance to see a graph of their arousal that covered the time period from the calibration at the beginning of the study all the way until they clicked the button to continue. Once the button was clicked, the computer appeared to process the data for a few moments, and it then showed a graph that indicated one of two things. In the moderate-arousal condition, the graph indicated a relatively stable amount of arousal throughout the task followed by a moderate spike once the feedback was received. The x-axis of the graph was labeled with these events to help

participants interpret the cause of the spike. In the low-arousal condition, the graph indicated relatively stable arousal during the task that was identical to that in the moderate-arousal condition, and it also indicated a very small spike in arousal after the feedback was received. In the control condition, no feedback was given.

In both feedback conditions, a bogus scale was provided to help participants understand what the values of arousal on the graph actually meant. The scale ranged from 0 ("your baseline level of arousal [e.g., arousal resulting from sitting calmly"]) to 200 ("overwhelming arousal [e.g., arousal resulting from overwhelmingly exciting or disturbing information or activity"]). Participants in the moderate-arousal condition had a level of arousal that did not vary much from baseline (it was slightly above baseline throughout the task, rising slowly near the end), but it spiked to approximately 75 after the feedback was received (i.e., halfway between the levels of slight arousal and moderate arousal). Their condition was labeled "moderate arousal" instead of "high arousal," because the graph they received indicated moderate rather than high arousal. The feedback given to the moderate-arousal participants was high compared to the feedback given to the low-arousal participants, but the feedback was not high in an absolute sense when compared to the participants' own baselines. Arousal feedback that was too high may not have been believable, so a more moderate level was chosen for the moderate-arousal condition. Participants in the low-arousal condition had a level of arousal after the feedback of approximately 25 (i.e., halfway between baseline and moderate arousal). Thus, if they read the graph correctly, all participants within each condition should have interpreted the arousal on their respective graphs as representing similar levels of absolute arousal. Participants in the no-feedback condition saw no graph.

After the feedback, participants completed a questionnaire that included all the primary dependent measures, personality measures, and manipulation checks (see Appendix A for all items). First, participants completed close-ended items that measure their perceptions of responsibility for their decision. These questions represented some of the different kinds of excuses that are identified in the literature, such as the three types of excuses identified by Schlenker et al. (2001) (lack-of-control, lack-of-prescription-clarity, and lack-of-personal-commitment excuses) and the counterfactual types identified by Markman and Tetlock (2000). In addition, participants completed items related to regret, disappointment, sadness, and a variety of other negative emotions that could result from the task. Personality measures and manipulation checks were completed last. Finally, participants were probed for suspicion, debriefed, and dismissed.

Results

Manipulation Checks

Arousal. The most straightforward manipulation-check item for the arousal manipulation asked "On average, how aroused did you feel AFTER the decision task?" (1 = "Not at all aroused" to 5 = "Extremely aroused"). There was a significant main effect of arousal, F (2, 248) = 23.10, $p < .0001$, $MSE = .75$. Participants in the moderate arousal condition ($M = 2.69$, $SD = .95$) reported feeling significantly more arousal following the task than did participants in the low-arousal condition ($M = 1.99$, $SD = .75$), F (1, 248) = 28.48, $p < .0001$, and participants in the no-feedback condition ($M = 1.85$, $SD = .90$), F (1, 248) = 39.84, $p < .0001$. The low-arousal and no-feedback conditions did not differ, $F < 1.14$. There were no other significant effects, Fs < 1.81.

Two other items assessed participants' understanding of the arousal manipulation. The first item asked "According to the graph you saw of your own arousal, how aroused were you on

average?" (1 = "Not at all aroused" to 5 = "Extremely aroused"; 6 = "I did not receive arousal feedback"). A 2 (6 cars or 18 cars) × 2 (moderate arousal or low arousal) ANOVA revealed a significant main effect of arousal, F (1, 167) = 32.10, $p < .0001$, $MSE = .73$. The no-feedback condition was not included in this ANOVA, because the variance in responding was necessarily different for them (i.e., they should all have put "6" as a response, whereas the low-arousal and moderate-arousal participants should have put between "1" and "5"). Participants in the moderate-arousal condition ($M = 2.41$, $SD = .64$) reported significantly more arousal than participants in the low-arousal condition ($M = 1.86$, $SD = .62$). There was no effect of the car factor or the interaction in the 2 × 2 ANOVA, Fs < 1.13. An analysis of descriptive statistics for the no-feedback participants revealed that the vast majority correctly reported not receiving any feedback ($M = 5.56$, $SD = 1.20$).

The next item asked "On average, how aroused did you feel DURING the decision task?" (1 = "Not at all aroused" to 5 = "Extremely aroused"). The part of the feedback graph during the task was not different for people in the two feedback conditions, and their perceptions of their actual arousal during the task were similarly not different, F (1, 248) = 1.75, $p = .27$, $MSE = .69$. However, there was a marginal main effect (omnibus F-test) of the arousal factor on arousal felt during the task, F (2, 248) = 2.82, $p = .06$, such that participants in the no-feedback condition ($M = 2.03$, $SD = .96$) reported feeling more arousal during the task than those in the low-arousal condition ($M = 1.73$, $SD = .77$), F (1, 248) = 5.61, $p < .02$, but were not different from those in the moderate-arousal condition ($M = 1.89$, $SD = .75$), F (1, 248) = 1.13, $p = .29$. There were no other significant effects, Fs < 1.19.

In summary, the arousal manipulation did have the intended effect on arousal. Participants in the moderate arousal condition reported feeling more aroused than participants in

the low-arousal and no-feedback conditions. However, manipulating arousal was only a means to an end, because the true purpose of the manipulation was to manipulate the intensity of negative affect.[2]

Affect. Although the manipulations of arousal did generally cause the intended effects, arousal was only manipulated for the purpose of manipulating affect. Thus, the affect-related items were the most telling manipulation checks regarding the effectiveness of the arousal manipulation. Participants completed a series of 11 semantic differential items, 10 of which related to cognitive and emotional states that were designed to assess participants' affective reactions to the result of the decision task. When the 10 items regarding reactions to the test were combined (the "alert—tired" dimension was not included), they showed high internal consistency, Cronbach's alpha = .91, suggesting that the mean score on those items represented an overall measure of how negative participants felt after the task. When a 2 (6 cars or 18 cars) × 3 (moderate arousal, low arousal, or no feedback) ANOVA was performed, a significant main effect of arousal emerged, F (2, 250) = 4.31, p = .01, MSE = .59. Participants in the no-feedback condition (M = 3.76, SD = .82) were significantly less happy than participants in the low-arousal condition (M = 4.11, SD = .74), F (2, 250) = 8.49, p < .01, and marginally less happy than participants in the moderate-arousal condition (M = 3.98, SD = .75), F (2, 250) = 3.27, p = .07. The low-arousal and moderate-arousal conditions did not differ, F (2, 250) = 1.22, p = .28.

These effects were not consistent with what was expected from the manipulation of arousal. If the manipulation had the intended effect, the moderate-arousal condition should have

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[2]Two pilot studies were conducted in which affect was manipulated through the use of directional questions. The wording of the questions was designed to encourage participants to focus on either positive or negative affect experienced during and after the task, thereby manipulating their affect. The results from these studies indicated that this approach was not likely to be successful, so the approach used in this study was used instead, because it had a greater theoretical and empirical background.

led to the worst affect when compared with the low-arousal and no-feedback conditions. As it turned out, affect in the moderate-arousal condition was in the middle of the scale, and the no-feedback and low-arousal conditions were slightly on the negative side.

Three affect-related items were measured immediately after the arousal feedback as an additional manipulation check for the arousal factor. After receiving two relatively affect-irrelevant items ("How much patience did you feel?" and "How much excitement did you feel?"), participants were asked (a) "How many positive feelings did you experience?" (1 = "None at all" to 5 = "A great deal"), (b) "How many negative feelings did you experience?" (1 = "None at all" to 5 = "A great deal"), and (c) "How did you feel overall about your performance on the test" (1 = "Extremely bad" to 9 = "extremely good"). The main effect of arousal was not significant for (a), F (2, 247) = 1.58, p = .21, (b), $F < 1$, or (c), $F < 1$.

In summary, the manipulation checks revealed inconsistent relationships between the arousal manipulation and subsequent affect. The participants in the moderate-arousal and low-arousal conditions did not differ in meaningful ways in terms of affect. This was unexpected, because research shows that people need only to believe that they were more aroused to attribute the differences to salient sources (e.g., Batson, Engell, & Fridell, 1999; Valins, 1966; Wilkins, 1971). However, most studies on this topic use real-time indicators of arousal rather than a one-time indicator as used in this study, so the impact of the one-time indicator may not have been powerful enough to elicit the expected effects on affect.

Excuse Making

Although including gender in the analyses for Study 1 did result in a few main effects in which males made more excuses than females (a frequently replicated finding in the literature, e.g., Brown & Josephs, 1999; Warner & Moore, 2004), there were no interactions and no substantive contributions from including gender as a factor. Therefore, gender was excluded

from all analyses reported in this section to make for more straightforward analyses. Maximization scores also failed to predict anything meaningful in the data, so they were omitted as well.

As stated in the method section, the survey contained two different categories of excuse items that were analyzed in two different ways: as individual items and as clusters of items. The individual items were too distinct conceptually and statistically from each other and the other excuse items later in the scale (referred to as "combined items" below) to be included in factors. Those items were thus analyzed individually. However, items that shared the same scale were factor analyzed (see "combined items" section) into meaningful categories and analyzed as such.

Individual items. A 2 (6 cars or 18 cars) × 3 (moderate arousal, low arousal, or no feedback) ANOVA was conducted on the 10 individual excuse items near the beginning of the survey. Any differences in degrees of freedom resulted from the fact that participants were allowed to skip questions, so not all participants answered every question.

The analyses revealed several main effects of option number (see Table 2-1 for means and standard deviations). Relative to participants in the 6-option condition, participants in the 18-option condition reported feeling the following: the task was more difficult to complete in time, F (1, 249) = 10.08, $p < .01$, $MSE = 4.35$; the instructions were less clear, F (1, 249) = 3.92, $p < .05$, $MSE = 3.12$; the task was more difficult, F (1, 250) = 3.84, $p = .05$, $MSE = 3.43$. Also, one item from the second set of excuse questions did not fit well into any of the three factors (factors defined in next section), so that item was analyzed in isolation. When asked to rate the degree to which "the number of attributes for each car" helped or hurt performance, participants in the 18-option condition ($M = 4.70$, $SD = 1.42$) reported significantly less help than those in the 6-option condition ($M = 5.14$, $SD = 1.40$), F (1, 247) = 6.18, $p = .02$, $MSE = 1.98$. Some excuse

items in this list may seem more "accurate" (i.e., reflecting actual differences in conditions) than others. However, whether accurate or not, excuses are still excuses as long as they serve their intended function—reducing responsibility and facilitating self-presentational and self-protective goals.

Two items that were expected to be affected by the cars manipulation were not affected: (1) "how much personal responsibility do you feel for your performance on the test" and (2) "how much do you feel that factors beyond your personal control (e.g., background noise, testing conditions) were responsible for your performance on the test?". These items were designed to tap feelings of responsibility, but they did not differ by condition, *p*s = .13 and .72, respectively. When correlated with the individual items and excuse factors (see "combined items" section of Results) that were significantly affected by the cars manipulation, the two responsibility items showed very low to no correlation, *r*s = .00 to .19. This pattern of correlations suggests that asking specifically about feelings of responsibility is not the same as asking about more specific characteristics.

Two main effects of arousal were significant. There was a significant main effect for the item that asked "how hard did you try on the task," F (2, 250) = 3.92, p = .02, MSE = 3.78. Participants in the moderate-arousal condition (M = 5.21, SD = 2.11) reported that they exerted significantly less effort than those in the low-arousal condition (M = 6.01, SD = 1.81), F (1, 250) = 7.37, $p < .01$, and marginally less effort than those in the no-feedback condition, (M = 5.79, SD = 1.88), F (1, 250), p = .05. The low-arousal and no-feedback conditions did not differ, $F < 1$. There was also a significant main effect for the item that asked "how interesting was the test," F (2, 250) = 3.13, $p < .05$, MSE = 3.65. Participants in the moderate-arousal condition (M = 5.89, SD = 1.74) reported that the task was significantly more interesting than participants in the no-

feedback condition (M = 5.16, SD = 2.14), F (1, 250) = 6.26, p < .01, but not more interesting than reported by participants in the low-arousal condition (M = 5.55, SD = 1.85), F (1, 250) = 1.79, p = .18. The low- and moderate-arousal conditions did not differ, F (1, 250) = 1.41, p = .23. There were no significant interactions between the arousal and option-number factors on any item, ps > .09.

Taken together, the findings from the analyses of individual items suggested that participants in the 18-option condition made more excuses than participants in the 6-option condition. There were also two main effects of the arousal factor, but these effects were inconsistent and difficult to interpret given the ambiguity surrounding the effects of the manipulation on affect. There were no interactions between the factors.

Combined items. Although the items in the second part of the excuse questionnaire could be analyzed individually, it also made sense to combine them into meaningful, statistically reliable categories to get a measure of excuse making that was not as likely to be biased by the wording of a single item. Moreover, because there were significant main effects of the option-number factor on almost all items in each factor, combining the items made for a more efficient results section. To that end, three categories were compiled from the 14 items in the second excuse questionnaire. These categories were created through both a priori hypotheses and principal components analysis. Principal components analysis using oblique varimax rotation revealed three factors with Eigenvalues greater than one (see Table 2-2 for groupings and factor loadings) that were generally consistent with a priori categories (the same three factors came out in a similar analysis on the same items from Study 2). One item ("number of attributes for each car") did not consistently load well on any factor across Study 1 and Study 2, so it was left out of the factors and analyzed individually (see previous section for that analysis).

The first factor generally represented attributions relevant to characteristics of the testing environment (e.g., the testing environment; the influence of the research assistant). Lower numbers on this factor (i.e., reporting that the characteristics hurt more or helped less) reduced responsibility by implying that aspects of the testing situation interfered with performance. This factor will henceforth be referred to as the environment factor. One item in the group did not perfectly fit this description of the factor, however: the item labeled "your skill with computer software." That item loaded only moderately well on the environment factor, but its inclusion in the environment factor seemed to facilitate internal consistency, and it loaded much more strongly on that same factor when the same analysis was performed in Study 2. This pattern suggested that participants interpreted the item more in line with environmental influences than the relevance of the task to them personally. The presence of the word "software" may have been enough to enhance the salience of the situation rather than a more personal evaluation. The environment factor had an acceptable level of internal consistency, Cronbach's alpha = .71.

The second factor represented attributions that were relevant to participants' interest in the task and its personal relevance to them (e.g., "your abstract reasoning ability"; "your interest in the task"). This factor will henceforth be referred to as the interest factor. Lower number on this factor reduced responsibility for poor performance by implying that the participants lacked the ability, motivation, or interest necessary to succeed. In other words, if a task does not feel personally relevant to a person because he or she lacks a key component necessary for success, then he or she is not required to feel responsible for any failures that may occur. The interest factor had a moderate level of internal consistency, Cronbach's alpha = .67.

The third factor was perhaps the most straightforward, because it was composed of items that related specifically to the nature of the software and task itself (e.g., "number of car

options"; "time limit imposed by the software"). This factor will henceforth be referred to as the task factor. Items in this factor provided a very specific type of environmental excuse to participants. Lower numbers on this factor reduced responsibility by suggesting that aspects of the task specifically (as opposed to the testing environment in general) interfered with performance, thereby reducing personal responsibility. One could not be expected to succeed if aspects of the task itself were flawed or otherwise interfered with performance. The task factor had a moderate level of internal consistency, Cronbach's alpha = .57.

The three factors were positively correlated with each other, suggesting that all three factors shared something in common regarding excuse-making tendencies. The environment factor correlated significantly with the interest factor, r (254) = .34, $p < .0001$, and the task factor, r (254) = .22, $p < .001$. The interest and task factors were also positively correlated, r (254) = .28, $p < .0001$.

There were significant main effects of option number on all three factors (there were significant main effects on almost all the items within each factor as well). For the environment factor, participants in the 18-option condition (M = 5.20, SD = .80) reported being less helped by the listed influences than those in the 6-option condition (M = 5.45, SD = .88), F (1, 250) = 5.39, p = .02, MSE = .70. For the interest factor, participants in the 18-option condition (M = 4.81, SD = 1.07) reported being less helped by the listed influences than those in the 6-option condition (M = 5.17, SD = 1.01), F (1, 250) = 7.81, $p < .01$, MSE = 1.06. For the task factor, participants in the 18-option condition (M = 3.74, SD = 1.03) reported being less helped by the listed influences than those in the 6-option condition (M = 4.29, SD = 1.03), F (1, 250) = 17.86, $p < .0001$, MSE = 1.07. As a whole, these effects indicated that participants in the 18-option condition made more excuses of several varieties than participants in the 6-option condition.

There were no significant main effects of arousal feedback or option number × arousal feedback interactions, *p*s > .10.

As with the individual items, analyses of the factors revealed that participants in the 18-option condition made more excuses than participants in the 6-option condition. Using factors provided the advantage of creating more reliable categories of excuses than could be found by using individual items, and the analysis of those factors suggested that the increase in excuses found among 18-option participants included several categories of excuses that ranged from very task specific to generally situation specific to personal.

Affect

As mentioned in the manipulation-checks section, affect was measured as the mean score for 10 semantic differential items, and there was a main effect of arousal feedback. However, there were no significant effects of option number or the option number × arousal feedback interaction, *p*s > .24. At first glance, this lack of a significant main effect would seem to suggest that there was not a direct effect of option number on affect. This lack of an effect could have been problematic, because research shows that too many options should lead to worse affect (e.g., Iyengar & Lepper, 2000). Thus, for exploratory purposes, individual items were examined to ensure consistency with studies showing that too many options leads to worse affect. Analyses revealed four main effects. Participants in the 18-option condition ($M = 3.94$, $SD = 1.28$) felt less content than those in the 6-option condition ($M = 4.28$, $SD = 1.28$), F (1, 250) = 4.90, $p < .03$, $MSE = 1.58$. Similarly, participants in the 18-option condition ($M = 4.33$, $SD = 1.16$) felt more dissatisfied than participants in the 6-option condition ($M = 4.04$, $SD = 1.12$), F (1, 250) = 4.07, $p = .04$, $MSE = 1.30$. In addition, two items that were presented very early in the survey as manipulation checks for the arousal-feedback factor (i.e., "how many negative feelings did you experience" and "how did you feel overall about your performance on the test") also

showed a main effect of option number. Participants in the 18-option condition (M = 2.29, SD = 1.09) experienced more negative feelings after the task than participants in the 6-option condition (M = 2.03, SD = .96), F (1, 247) = 3.87, p = .05, MSE = 1.06. Participants in the 18-option condition (M = 4.39, SD = 1.63) also felt less good about their performance than participants in the 6-option condition (M = 4.82, SD = 1.72), F (1, 250) = 4.10, p = .04, MSE = 2.85. Although there were fewer effects than expected of option number on negative affect, the effects that did surface were all consistent with previous research on the relationship between option number and affect (e.g., Iyengar & Lepper, 2000).

Excuse Making and Affect

One of the primary purposes of this research was to determine the relationship between excuses and affect as it relates to decisions from variously sized choice sets.

The most straightforward way to determine this relationship was to examine the main effects and interactions involving a factor that manipulated affect. However, given that the arousal factor made very little difference in affect or excuse making (and the differences were difficult to interpret where they did exist), other strategies were explored to expand on the relationship between affect and excuse making.

Affect and excuse making were clearly correlated such that worse affect was associated with more excuses. This correlation was quite consistent throughout the data, but perhaps the most straightforward tests involved correlating the primary affect measure (i.e., the mean of the 10 semantic differentials) and measures of excuse making (reverse scored here for purposes of interpretation). For example, there were significant positive correlations between affect and the environment, r (254) = -.32, $p < .0001$, interest, r (254) = -.30, $p < .0001$, and task, r (254) = -.22, $p < .001$, excuse-making factors. Worse affect was associated with more excuses in all cases. Conceptually identical correlations between affect and excuses were found almost

regardless of the variables involved. Thus, although it was clear that affect and excuses are correlated, determining why was not so easy with the data from Study 1. However, this relationship was examined in more detail in Study 2.

Discussion

In Study 1, it was hypothesized that (a) more options would lead to more excuses, (b) worse affect would lead to more excuses, and (c) option number and affect would interact to produce the most excuses in the moderate-arousal, 18-option condition. The first hypothesis was supported: Participants who had the most options also made the most excuses on a variety of dimensions. In addition, participants made excuses in a way that relates back to the real-world context in which this research was embedded. Some excuses made perfect sense in the context of too many options. For example, it was perfectly logical for participants in the 18-option condition to report that the task was more difficult to complete given the time restraints, because it actually was. Similarly, the restrictions on seeing information from more than one car at a time really did hurt the 18-option participants more, because they had less time to waste clicking from one car to the next. However, the 18-option participants didn't just stop there; they also made excuses in a variety of domains that should not have been affected by the difficulty of the task. For example, relative to 6-option participants, 18-option participants reported that the task instructions were less clear and the research assistant was less helpful. In fact, the environment factor that emerged from the principal components analysis might be described as representing a class of excuses that was irrelevant to the difference in conditions. Participants who were already making more excuses for legitimate reasons also seemed to make more excuses for illegitimate reasons. In other words, it seemed that participants in the 18-option condition disengaged responsibility more than participants in the 6-option condition, but that disengagement spread beyond legitimate "reasons" to include categories of excuses that were not

legitimate. It seemed that 18-option participants generalized their disengagement of responsibility to multiple categories of excuses, some of which were not particularly justifiable in the experimental context.

Only two excuse-related items that should have been affected by the option-number manipulation were not: the items regarding how much personal responsibility the participants felt for their performance and how much they felt other, situational influences were responsible. Given the low or nonexistent correlations between these items and other measures of excuses used in the study, however, it seems that asking people about how responsible they feel is not the same as providing them with the opportunity to make more specific excuses. Research suggests that social scientists sometimes parse psychological states into more distinct dimensions than average people are capable of identifying on a questionnaire (e.g., Connelly & Butler's [2006] work showing that participants do not always distinguish well between affective states that researchers define very carefully as distinct). Thus, it is possible that although people who engage in more excuse-making behaviors probably feel less responsible than people who make fewer excuses, they can't necessarily label their state of responsibility as effectively when asked directly. Moreover, people sometimes try to benefit from "taking responsibility" by claiming publicly that they are responsible for some outcome while simultaneously citing specific excuses designed to reduce that responsibility in a more discrete way (Schlenker, 1980). This strategy can maintain some of the social benefits of excuse making while avoiding some of the costs associated with overtly claiming a lack of responsibility.

The second hypothesis (i.e., worse affect would lead to more excuses) received inconsistent support. Excuse making was associated with worse affect correlationally, but this effect needed to be interpreted with caution due to its correlational nature. The ideal support for

the second hypothesis would have come in the form of a main effect of the arousal factor, assuming the factor produced the kind of changes in affect for which it was intended. However, although the arousal factor manipulated arousal in the intended way, the manipulation of arousal did not cause a change in affect in the hypothesized way. Moreover, any effects of the arousal factor on excuse making were weak, inconsistent, and difficult to interpret in any meaningful way. Effects regarding that factor thus will not be interpreted in detail here. Also, possibly because of the ineffectiveness of the arousal manipulation, the third hypothesis (i.e., an interaction between affect and option number) was not supported.

The positive correlation between excuses and negative affect was consistent with the hypotheses, but it didn't actually say much about the relationship between excuses and affect. It made sense that people who felt worse would make more excuses, but the design of the study did not provide a way to interpret whether or not making more excuses would eventually help people feel better. In other words, there was not enough time between the opportunity to make excuses and the measurement of affect to see if participants' excuses actually worked. Study 2 did provide this opportunity, however, because it included two questionnaires separated by a distracting, difficult task.

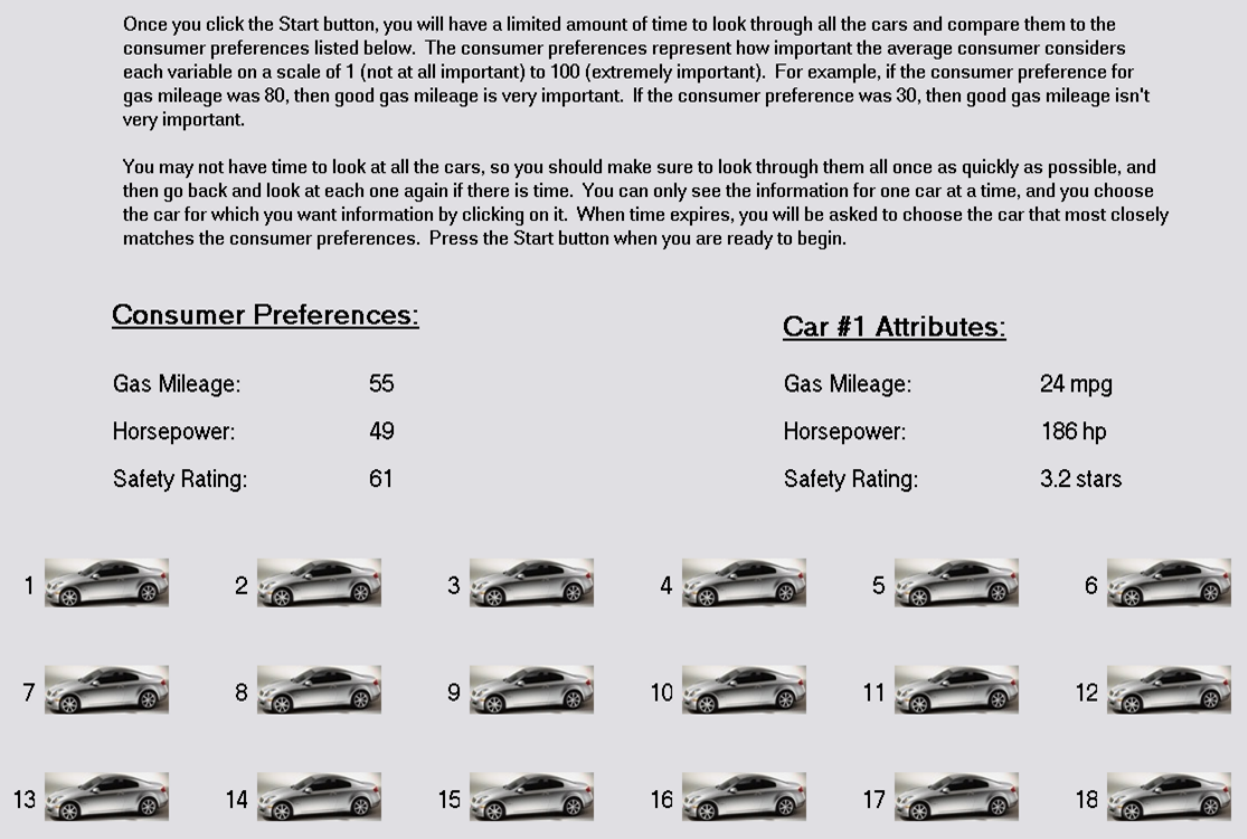

Figure 2-1.Screen shot for car task that participants experienced

Table 2-1. Main effects of option number on individual excuse items in Study 1

Item	18 Cars		6 Cars	
	M	*SD*	*M*	*SD*
Task difficulty in terms of time	4.70	2.15	3.77	2.01
Clarity of instructions	6.28	1.93	6.72	1.59
Overall task difficulty	4.41	1.89	3.96	1.80

Note. First and third rows reverse scored such that higher numbers for all three items equal more agreement with statement.

Table 2-2. Factor loadings for excuse factors (Study 2 loadings in parentheses)

Item Number	Item Wording	Factor 1 Testing Environment	Factor 2 Personal Interest	Factor 3 Task Characteristics
1.	User-friendliness of the software	**.74 (.69)**	-.16 (-.07)	.01 (.22)
2.	Restrictions on seeing attributes for more than one car at a time	.00 (.08)	-.03 (-.21)	**.64 (.80)**
3.	Number of car options	-.05 (-.00)	.03 (.28)	**.74 (.63)**
4.	Your general knowledge of cars	-.26 (-.09)	**.70 (.61)**	-.02 (.12)
5.	Your skill with computer software	**.52 (.67)**	.37 (.04)	-.06 (-.09)
6.	Number of attributes for each car	.17 (.24)	.30 (.47)	.44 (.26)
7.	Your memory for details	.20 (.19)	**.47 (.53)**	.33 (.24)
8.	The testing environment (e.g., lighting, background noise, room temperature, etc.)	**.68 (.62)**	.25 (-.16)	-.24 (.22)
9.	Your abstract reasoning ability	.08 (-.02)	**.66 (.74)**	.15 (.00)
10.	Your ability to solve math problems	.02 (.05)	**.70 (.73)**	.00 (-.04)
11.	Influence (e.g., clarity, ambiguity) of the computer instructions	**.65 (.56)**	-.06 (.36)	.19 (-.16)
12.	Influence (e.g., instructions, appearance) of the research assistant	**.64 (.55)**	-.07 (.14)	.21 (-.18)
13.	Time limit imposed by the software	.01 (-.11)	-.01 (.12)	**.72 (.75)**
14.	Your interest in the task	.12 (.24)	**.50 (.46)**	.02 (.07)

Note. Factor loadings are standardized regression coefficients. Item 6 did not consistently load well on any factor, and it reduced the internal consistency for each factor if included, so it was excluded and analyzed separately.

CHAPTER 3
STUDY 2

Method

Participants

One-hundred sixty two undergraduate students (124 females; 38 males) received partial credit toward fulfillment of a research requirement for their introductory psychology course. Students did not know the nature of the study when they signed up for it.

Instruments

Participants complete the Maximization Scale as in Study 1.

Procedure

The study was a 2 (excuse or no excuse) × 2 (option number: 6 or 18 options) × 2 (task) mixed-model design with task (task 1 and task 2) as a within-subjects factor. The beginning of Study 2 was the same as Study 1 through the end of the first task with two exceptions. First, there was no mention of arousal, no electrodes, and no arousal feedback given. Second, the written instructions given to participants explained that the researchers were most interested in the effects of limited amounts of time on complex decision making, and participants in different conditions ostensibly had different amounts of time to make their decisions. The instructions also indicated that the participants had been assigned to the "two-minute" condition, but this condition was described differently depending on the level of the excuse factor. In the excuse condition, the instructions described the participant's condition as being one of the shortest, most difficult conditions. Moreover, the instructions explained that research has shown that making the types of complex decisions that participants would be making is very difficult in only two minutes, but participants should still do their best to make the best decision possible despite the difficult conditions. In the no-excuse condition, participants were told that their time condition

was among one of the longest in the study, and research had shown that most people didn't have much trouble making a good decision in that amount of time. The actual time for the first task was two minutes for all participants.

The excuse manipulation could have been introduced in one of two places in the method: before the first task or after the first task. Each possibility had advantages and disadvantages, but given my goals, the most useful position of the manipulation was prior to the first task. Manipulating the excuse factor before the first task meant that decision-making strategies used during the task may have differed according to the excuse, but that apparent methodological problem could also be a benefit. As in Study 1, decision-making strategies and effort were operationalized as the number of times participants look at the attributes for each option and the time they spend looking at each attribute. If strategies and effort had differed according to levels of the excuse factor, then that pattern may have revealed important information about the mediating role played by decision-making strategies and effort on later excuse making. However, because strategies and effort did not differ by the excuse factor[1], any subsequent differences in excuse making could not be said to be related to actual differences in performance. In both cases, important information would be revealed about the effects that excuse presence has on decision-making strategies and effort. Conversely, if the excuse manipulation had occurred after task, then it could only have affected post-decisional attributions, and although the results would have been relatively straightforward to interpret, they would have provided less insight into what the presence of the excuse was actually doing.

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[1]A 2 (excuse or no excuse) × 2 (6 or 18 options) ANOVA showed that participants in the excuse condition did not differ from participants in the no-excuse condition on time elapsed before looking at all options or number of clicks per option, Fs < 1. This suggests that having the excuse in mind before starting the task did not actually change the way participants approached the task or the effort that they put forth toward finding the correct response.

The first task proceeded exactly like the task in Study 1, except there was no ostensible measurement of arousal or manipulation of arousal feedback. Once participants finished reading over their feedback, they were told that they would be given another task to complete before the study was over. However, before that second task was completed, participants were given a questionnaire containing the same items as Study 1, except they did not complete any personality measures until after the second task.

The purpose of the second task was explained by the experimenter as follows: "As you know, the purpose of this study is to examine the ways in which people make decisions and remember complex information that relates to market research. However, we are also interested in how these processes are affected by the kinds of work conditions that real market researchers experience. Therefore, you will complete the next task in the presence of the kind of distracting background noise that people often hear in office environments. Other participants in this study will be completing the task with more noise, less noise, or no noise at all." All participants heard the same description of the task. The description and the presence of the background noise were designed to offer a good potential excuse without clearly identifying it as such, thereby encouraging people to disengage responsibility. The second task was designed to be quite different from the first, because the hypotheses related to carry-over effects of excuse making on one task to a completely unrelated task.

The second task was similar to a 1-back task, but it required mental computation of randomly chosen multiplicative functions between 0×0 and 11×11. Participants saw a new function appear every two seconds, and they had to determine whether the result was more than, less than, or the same as the result from the previous function. Participants were told that they must answer as quickly and accurately as possible. Thus, participants were required to pay close

attention, make quick mental decisions, and remember the last thing they saw despite proactive interference from new information. The second task lasted three minutes and was preceded by a short practice session. The practice session was necessary to ensure that participants fully understood the nature of the task. After participants finished the task, the program appeared to be calculating their performance compared to other participants, and they eventually saw that they finished in the 37th percentile relative to other participants in the study.

After the completion of the second task, participants completed the same questionnaire that they completed after the first task with a few minor differences related to the differences between the tasks (e.g., questions related to the number of cars were dropped). Participants also completed the same personality measures as Study 1 and relevant manipulation-check items. Finally, they were probed for suspicion, debriefed, and released with credit for participating.

Results

Manipulation Check

The manipulation checks demonstrated that the excuse manipulation was effective. Two surveys (see Appendices B and C) were completed during the study. In the second survey, the last question assessed the degree to which participants recalled the excuse manipulation given at the beginning of the study. The question was "Given your time limit, how difficult should the first task have been according to the instructions you were given before the task?" (1 = "Not at all difficult" to 5 = "Extremely difficult"). A 2 (6 cars or 18 cars) × 2 (excuse or no excuse) ANOVA was performed on this item. There was a main effect of excuse, F (1, 156) = 61.44, p < .0001, MSE = 1.19. Participants in the excuse condition (M = 3.07, SD = 1.28) reported that the task should have been significantly harder than participants in the no-excuse condition (M = 1.71, SD = .87). Neither the main effect of cars or the interaction were significant, ps > .18.

Use of Excuses: Task 1

Like in Study 1, gender did not make significant contributions to any of the analyses (other than occasional main effects in which males made more excuses than females), so it was excluded as a factor in the analyses for Study 2. Also, Maximization scores again failed to predict anything meaningful in the data, so they were omitted from the analyses reported here.

Individual items. A 2 (6 cars or 18 cars) × 2 (excuse or no excuse) ANOVA was performed on the individual items presented after the first task (items were the same as those from Study 1). Participants in the 18-option condition reported that the task was more difficult to complete in time, F (1, 158) = 8.82, $p < .01$, $MSE = 4.79$; the instructions were less clear, F (1, 158) = 4.94, $p < .03$, $MSE = 3.36$; and the task was more difficult in general, F (1, 158) = 3.61, $p < .06$, $MSE = 3.88$ (see Table 3-1 for means and standard deviations). These were exactly the same items that were significantly affected by the option-number manipulation in Study 1.

The excuse factor also caused a main effect in two of the variables listed above. Participants in the excuse condition (reverse-scored Ms = 4.91 and 4.86, SDs = 2.20 and 1.99, respectively) reported that the task was more difficult to complete in time, F (1, 158) = 5.08, $p < .03$, and more difficult in general, F (1, 158) = 5.26, $p < .03$, than participants in the no-excuse condition (reverse-scored Ms = 4.13 and 4.15, SDs = 2.27 and 2.21, respectively). These were relatively accurate descriptions of the differences in the instructions across conditions. As in Study 1, there were no significant effects on the two individual items that mention the word "responsibility," ps > .16.

There was one significant option number × excuse interaction on the item that asked "how interesting was the test?" F (1, 158) = 4.20, $p < .05$, $MSE = 3.92$ (see Table 3-2 for means). The only significant simple effect was between no-excuse participants in the 6-option and 18-

option conditions, F (1, 158) = 6.93, $p < .01$. Participants in the no-excuse/18-option condition expressed the least interest in the task—significantly lower interest than participants in the no-excuse/6-option condition and marginally lower than participants in the excuse/18-option condition, $p = .08$. No other simple effects were significant, ps > .24. The overall pattern of this interaction implied that it took the combination of the denial of the most obvious excuse (i.e., the no-excuse condition) and the presence of a lot of options (i.e., the 18-option condition) to make a difference in reported interest. This effect could be the result of the fact that participants in the no-excuse condition had the most obvious excuse (i.e., difficulty of task completion given the time allotment) taken away from them by the instructions, and only participants in the 18-option condition felt a strong enough reduction of responsibility for precisely this reason that they pursued a more creative way to make an excuse. Denigrating interest in the task was an effective way to reduce responsibility in the no-excuse condition, because when participants said they had limited interest, they excused themselves somewhat from responsibility by implying a lack of obligation regarding good performance (e.g., "how could I be expected to put forth my best effort and do well in a task in which I had no interest?").

In summary, the main effects on the individual items from the option-number factor paralleled the effects found in Study 1: 18-option participants made more excuses than 6-option participants. Also, the excuse factor appeared to be effective, because participants in the excuse condition responded in ways that were consistent with the provided excuse (e.g., reporting that the task was more difficult), but they also denigrated the clarity of the instructions (i.e., a prescription-clarity excuse). The interaction on reported personal interest in the task suggested that, when task difficulty was specifically excluded as an excuse in the no-excuse condition, participants in the 18-option condition had to be a little more creative in their excuses by going

beyond blaming their failure on the difficulty of the task by instead blaming it on their own lack of interest.

Combined items. A principal components analysis revealed the same factors as Study 1 (see Table 2-2 for loadings). The three factors had generally acceptable internal consistency, Cronbach's alpha = .72, .65, and .65, respectively. Unfortunately, although the means for the factors differed by option number in the proper direction, only one of the effects was significant. On the task factor, participants in the 18-option condition (M = 3.73, SD = 1.18) reported less help from task characteristics than participants in the 6-option condition (M = 4.29, SD = 1.03), F (1, 158) = 11.06, $p < .01$, MSE = 1.15. This effect was consistent with the reality of the differences between the 18- and 6-option conditions. The main effects of option number on the other two factors did not approach significance, ps > .30.

There was also a main effect of the excuse manipulation on the task factor, F (1, 158) = 7.69, $p < .01$. Participants in the excuse condition (M = 3.78, SD = 1.19) reported less help from the task characteristics than participants in the no-excuse condition, (M = 4.25, SD = 1.05). The main effects of the excuse manipulation on the other two factors did not approach significance, ps > .29.

Finally, there was a significant option number × excuse interaction on the task factor, F (1, 158) = 4.81, $p < .03$ (see Table 3-3 for means). Participants in the excuse/18-option condition made the most excuses, differing significantly from those in the excuse/6-option condition, F (1, 158) = 15.01, $p < .001$, and the no-excuse/18-option condition, F (1, 158) = 12.67, $p < .001$. The other two simple effects did not approach significance, ps > .42.

The results from the combined items on the first questionnaire suggested that it was necessary to have both a large number of options and a salient excuse in order to facilitate the

most excuse making. Moreover, the task factor represented a dimension of the task that really did differ somewhat according to option number (i.e., those characteristics of the task probably really did seem more harmful to performance in the 18-option condition), but it did not represent a dimension that really differed according to the excuse factor (i.e., participants in both the excuse and no-excuse conditions experienced equivalent task difficulty). Thus, the interaction on the task factor that qualified the two main effects suggested that neither more options nor salient excuses were enough to cause more excuse making; rather, it took both to create the effect.

Affect: Task 1

There were no significant effects involving either of the manipulated factors on the combined affect measure (mean on same 10 items as in Study 1, Cronbach's alpha = .94), *p*s > .14. A similar exploratory analysis of individual items showed a similar pattern to that found in Study 1.[2] In addition, there was a significant main effect of the excuse factor on initial affect (first item on questionnaire: "how do you feel about your performance?"), F (1, 157) = 5.73, $p < .02$, $MSE = 3.29$. Unexpectedly, participants in the excuse condition ($M = 4.40$, $SD = 1.72$) were initially less happy about their performance than those in the no-excuse condition ($M = 5.09$, $SD = 1.89$). This effect was not significant on the combined affect measure later in the questionnaire, F (1, 158) = 2.14, $p < .15$, $MSE = .92$. This pattern suggested that having a viable

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[2]A similar exploratory analysis to the one performed in Study 1 was performed on the individual affect items for the purposes of replicating existing findings regarding the effects of option number on affect. The items in Study 1 that differed significantly (i.e., contentment and satisfaction) by option number did not differ in Study 2, *p*s > .26, but the adjusted means were very similar to those found in Study 1. In addition, there was a significant main effect of option number on the disappointment-fulfillment item, F (1, 155) = 4.34, $p < .04$, $MSE = 1.28$. Participants in the 18-option condition ($M = 3.61$, $SD = 1.15$) felt more fulfilled (i.e., less disappointed) than participants in the 6-option condition ($M = 3.96$, $SD = 1.13$). This effect is also consistent with research on option number and affect (e.g., Iyengar and Lepper, 2000). It is unclear why the manipulation did not affect the same items significantly in both studies, but all the effects that did occur were consistent with previous research.

excuse led participants to initially be less satisfied with their performance, but that effect was not as strong later in the same questionnaire. A possible explanations for this unexpected finding is examined in the discussion.

A similar analysis to Study 1 revealed similar findings regarding the relationship between excuse making and affect in the short term. Affect was significantly correlated with the environment factor, r (159) = -.42, $p < .0001$, the interest factor, r (159) = -.38, $p < .0001$, and the task factor, r (159) = -.35, $p < .0001$. Study 2 also contained a second questionnaire, and that allowed for an analysis of the effects of excuse making over a longer period of time. This analysis is discussed later in this section.

Use of Excuses: Task 2

The questionnaire that participants received after the second task (see Appendix C for items) was necessarily slightly different from the first questionnaire, because some items only made sense in light of the car task, and other items only made sense in light of the memory task. However, care was taken to make the questionnaires as similar as possible for comparison purposes.

Individual items. A 2 (6 cars or 18 cars) × 2 (excuse or no excuse) ANOVA was performed on the first nine excuse-related items after the second task. There were significant main effects of the option-number factor on two of the items. Participants in the 18-option condition (Ms = 4.92 and 6.00, SDs = 2.41 and 1.82, respectively) reported that the task was less interesting to them, F (1, 157) = 6.09, $p < .02$, MSE = 5.12, and that they tried less hard, F (1, 158) = 4.86, $p < .03$, MSE = 3.04, than participants in the 6-option condition (Ms = 5.80 and 6.60, SDs = 2.07 and 1.65, respectively). This effect is consistent with the hypothesis that conditions facilitating excuse making in one situation in the experimental context will facilitate them in another situation in that same context.

There was only one significant main effect of the excuse factor on the individual items. Participants in the excuse condition ($M = 4.27$, $SD = 1.96$) reported that they felt it was less important to them personally to do well than participants in the no-excuse condition ($M = 4.98$, $SD = 2.07$), F (1, 158) = 4.97, $p < .03$, $MSE = 4.09$. There was also a marginal effect that was supportive of hypotheses, so it is interpreted conservatively here. Participants in the excuse condition ($M = 6.52$, $SD = 1.83$) reported that the second task was more difficult than participants in the no-excuse condition ($M = 6.04$, $SD = 1.89$), F (1, 158) = 2.80, $p < .10$, MSE = 3.40 (means reverse scored). As in Study 1 and the first questionnaire of Study 2, the items in the second questionnaire that contained the word "responsibility" were not significantly affected by the manipulated factors, ps > .24.

In summary, main effects of the option-number and excuse factors showed that even on a second, irrelevant task, participants made more excuses if they were in the excuse condition or the 18-option condition in the first task. These excuses ranged from relatively legitimate (e.g., 18-option participants reported that the second task was more difficult, and that could be because they were more cognitively fatigued and really did perceive it to be more difficult) to relatively illegitimate (e.g., reporting that they did not feel the task was interesting or worthwhile). The lack of any option number × excuse interactions suggested that the two factors has relatively independent effects on individual excuse variables.

Combined items. In the questionnaire after the second task, it was inappropriate to designate three factors that conceptually paralleled the ones found in questionnaire from Study 1 and the first questionnaire from Study 2, because the items were not the same. Those previous questionnaires included four items that referred specifically to the car-selection task, and it did not make sense to include those irrelevant items after the second task. Three of the four items

that were not included were the items that made up the task factor in the previous factor analyses. Due to the absence of these items, in was inappropriate to have a third factor that described the car task. The remaining items were much more consistent with the environment and interest factors found in previous questionnaires.

A principal components analysis with oblique varimax rotation confirmed this reasoning by revealing two factors that paralleled the interest and environment factors present in Study 1 and in the first questionnaire in Study 2 (see Table 3-4 for loadings). The interest factor had acceptable internal consistency, Cronbach's alpha = .80, and the environmental factor had moderate internal consistency, Cronbach's alpha = .65. The item regarding the influence of the research assistant was excluded for being poorly correlated with both factors. If people made more excuses on the interest factor, they were essentially reporting that they lacked the ability or motivation to succeed. People who made more excuses on the environment factor were reporting that aspects of the experimental environment kept them from achieving positive outcomes. Excuses on either factor allowed people to appear less responsible for the negative performance.

A 2 (6 cars or 18 cars) × 2 (excuse or no excuse) ANOVA performed on the two factors revealed no significant main effects of either experimental factor on either excuse factor, ps > .23. The option-number × excuse interaction was not significant for the interest factor, $F < 1$, but it approached significance for the environment factor, $F(1,158) = 3.11$, $p < .08$, $MSE = .88$. None of the means differed significantly from each other, ps > .15, so the interaction will be interpreted cautiously here. However, although the differences were not significant, it is worth mentioning that the pattern of means was similar to those seen in the significant interaction on the third factor in the first questionnaire. In other words, participants in the excuse/18-item

condition made more excuses than any other condition, and the other conditions did not appear to differ much from each other. This marginal effect again supported the notion that having a lot of options and a salient excuse were necessary to generate the most excuse making.

Affect: Task 2

A 2 (6 cars or 18 cars) × 2 (excuse or no excuse) ANOVA was performed on the 10-item affect measure (Cronbach's alpha = .94). As in the first questionnaire, none of the effects were significant, *p*s > .16. A similar analysis on the first item in the questionnaire ("How do you feel about your performance on the test?") failed to reveal a significant main effect of the excuse factor, F (1, 158) = 2.77, p = .10, MSE = 2.58.

Although there were no obvious effects of the manipulated factors on affect, the relationship between excuse making and affect deserved more attention, because many different levels of excuse making occurred within all conditions, and the amount of excuses people made was related to their affect. The following section includes a closer examination of that pattern.

Path Model

When the relationship between excuse making and affect on the second questionnaire was examined, a pattern arose that was similar to the pattern found in both studies thus far. More excuses (means reverse scored to facilitate interpretation) were significantly correlated with worse affect when the interest factor, r (160) = -.27, p < .001, and environment factor, r (160) = -.37, p < .0001, of excuses were correlated with the 10-item affect measure. In other words, people who made more excuses on the second questionnaire also reported worse affect on the second questionnaire. In addition, more excuses were associated with worse affect when the environmental, r (160) = -.20, p < .01, interest, r (160) = -.25, p < .01, and task, r (160) = -.22, p < .01, excuse factors (means reverse scored) from the first questionnaire were correlated with the affect measure on the second questionnaire.

This persistent pattern of more excuses being associated with worse affect within the same questionnaire seemed reasonable, because it was probable that making excuses did not instantly improve affect. In other words, the failure probably caused most people to feel at least somewhat bad and make some excuses for it simultaneously, so a positive correlation between excuses and negative affect seemed reasonable. Conversely, given that extant research generally indicates that excuses should help affect, not hurt it (e.g., Snyder & Higgins, 1988), one would expect that more excuses should eventually lead to better affect after some time has passed, but there was still a significant positive correlation between excuses made on the first task and negative affect after the second task. However, although more excuses seemed to be associated with more negative affect throughout the study, it was possible that, controlling for individual differences in the tendency to make excuses and the tendency to report negative affect, one might see a different pattern between excuses made on the first questionnaire and affect reported on the second questionnaire. In other words, by controlling for given levels of affect and excuse making at all time points through the use of simultaneous equations, one can see how excuses after the first task affected affect after the second task, controlling for the other relevant relationships.

To address this possibility, structural equation modeling with latent variables was used. Two models were created (see Figure 3-2) that contained four latent variables: excuses made after the first task (time-1 excuses), excuses made after the second task (time-2 excuses), affect after the first task (time-1 affect), and affect after the second task (time-2 affect). Given that the different excuse factors that were found on the first and second questionnaires were highly correlated within each questionnaire, and there were slightly different items in the factors on the first and second questionnaires, it made sense to combine them into single excuse making

variables to avoid the problems associated with picking some types of excuses over others without theoretical justification. The newly generated excuse variables represented time-1 excuses for the first questionnaire (14 items; Cronbach's alpha = .79) and time-2 excuses (10 items; Cronbach's alpha = .76) for the second questionnaire. The time-1 and time-2 affect measures were the same 10-item measures used throughout the presented studies.

The most obvious, logical model given the order of presentation of the items in the questionnaires (i.e., time-1 excuses first, time-1 affect second, time-2 excuses third, and time-2 affect fourth) is the first model in Figure 3-2. This model tested whether time-1 excuses had a different pattern of effects on time-1 and time-2 affect. Conceptually, this model tested the hypothesis that excuses made earlier in the study caused worse affect in the short run and caused better affect in the long run. Setting up the model this way had the advantage of accounting for effects of earlier excuses on later excuses and earlier affect on later affect, thereby controlling somewhat for individual differences in responding tendencies. Moreover, the model also had the advantage of simultaneously comparing the direction of the relationship between excuses and affect for the short term of both questionnaires while also comparing the relationship between time-1 excuses and time-2 affect (i.e., the long-term relationship). Overall, using a structural equation model in this way provided a more complete picture of the data than other possible measurements of the relationship between time-1 excuses and time-2 affect (e.g., correlating time-1 excuses with a change-score of [time-2 affect − time-1 affect]; using regression to test effect of excuses at time 1 on affect at time 2 with affect at time 1 controlled).

A second, less intuitive model (see second model in Figure 3-2) was created to account for the possibility that it was actually affect that was driving the differences in excuse making. This model was consistent with the notion that the experience of negative affect might be what

drives excuse making in the first place, but it was more problematic than the first model in terms of the method of data collection, because the affect variables were collected after the excuse variables within each questionnaire. However, if one assumes that the initial reaction to the failure was negative affect, then it would make sense that negative affect would have been the true starting point of the process, and excuse making would follow as an attempt to alleviate that negative affect (Snyder & Higgins, 1988). Time-1 affect and time-1 excuses were functionally measured at the same time, so it is probably as sensible to think of them as occurring simultaneously as it is to think of them as being measured at two separate time points. Some structural equation modeling researchers (e.g., Kline, 2005) suggest that different variables that are measured within a very short amount of time can be said affect each other regardless of order in some circumstances.

The second model included affect as the first in the causal chain. The model tested paths from time-1 affect to time-1 excuses and from time-2 affect to time-2 excuses. Most importantly, like the first model, the second model tested the path between time-1 excuses and time-2 affect, thereby testing if more time-1 excuses actually caused better time-2 affect. Conceptually, the model tested the hypothesis that worse initial affect caused more excuse making in the short run, and more excuse making caused better affect in the long run. Like the first model, the second model also controlled for individual differences in responding by estimating the path from time-1 affect to time-2 affect and from time-1 excuses to time-2 excuses.

In order to compare the fit of the two models to the data, models that were less complex than the saturated models needed to be used. As such, the path of the least theoretical significance was left out of both models. That path went from time-1 affect to time-2 excuses.

Leaving that path out of both models allowed the models to be compared using the Akaike Information Criterion (AIC), which is considered to be one of the most useful goodness-of-fit statistics for comparing non-nested models (Kline, 2005).

Latent-variable path models were created in LISREL (v. 8.72) from the correlation matrix of these four variables (see Table 3-5 for zero-order correlations and Cronbachs' alphas) by setting the error variance for each latent variable to be equal to the observed variance (i.e., 1 − Cronbach's alpha) for that variable. Doing this created latent variables out of the observed variables and helped deal with the problem of measurement error associated with observed-variable models (J. Algina, personal communication, March 22, 2005).

The first model presented in Figure 3-2 fit the data very well. Several goodness-of-fit indices (computer through the "ftb" option in LISREL v. 8.72 to avoid the overly liberal estimates that can be found in versions of LISREL after 8.52) were chosen to illustrate this point: RMSEA = .00, NNFI = 1.01, CFI = 1.00, GFI = 1.00, SRMR = 0.015. All presented paths were significant at $p < .05$.

The structural coefficients in the model illuminated a pattern that was supportive of the hypothesis that excuses should improve affect over time. The significant paths from time-1 excuses to time-1 affect and from time-2 excuses to time-2 affect show that, in the short run, excuse making caused worse affect. The critical path, however, was the path from time-1 excuses to time-2 affect. That path was also significant, but in the opposite direction. Therefore, although more excuses appeared to cause worse affect in the short run, more excuses at the beginning of the study caused better affect later in the study. This effect is especially impressive when one considers that the significant zero-order correlation between time-1 excuses and time-2 affect indicated that more time-1 excuses were associated with worse time-2 affect when other

relevant influences (e.g., response biases) were not controlled. The advantage of the structural equation modeling approach was that it controlled for the often powerful effects of within-subject response biases on those items and allowed for a more accurate estimation of the relationship between excuses and affect in the long run.

The second model presented in Figure 3-2 did not fit the data particularly well according to some goodness-of-fit indices. Several goodness-of-fit indices were chosen to illustrate this point: RMSEA = .23, NNFI = .77, CFI = .96, GFI = .97, SRMR = .033. Although the CFI, GFI, and SRMR were acceptable, the RMSEA and NNFI were not acceptable. Given that the model was equally as complex as the first model (i.e., one degree of freedom), these goodness-of-fit indices indicated that the model was less appropriate. Most importantly, the AIC for this model was 27.72, whereas the AIC for the first model was 18.69. The difference in AIC indicated that the first model was a better fit to the data. However, although the path from time-1 excuses to time-2 affect was not significant for the second model, it did change signs just as in the first model.

The first model was the best fitting, and that was not particularly surprising given that the paths corresponded perfectly with the order of presentation of the variables. More interestingly, the first model implied that, although making more excuses may actually cause people to feel worse in the short run, those excuses cause them to feel better in the long run. The second model did not fit the data well, so the path of greatest interest will be interpreted cautiously here. The path from time-1 excuses to time-2 affect was not significant, but the sign did change when compared with the zero-order correlation, providing more support for the notion that more excuses helped affect in the long run.

Discussion

The primary goal of Study 2 was to determine if the presence of a good excuse, too many options when making a decision, or some combination of the two facilitated excuses following both a relevant and an irrelevant task. The secondary goal was to further examine the relationship between excuse making and affect. It was hypothesized that more options would lead to more excuses (hypothesis 2), and it was hypothesized that both the presence of a salient excuse (hypothesis 5) and the use of excuses (hypothesis 7) would both be associated with less negative affect.

The hypothesis regarding the primary goal of the study (hypothesis 2) was generally supported. Participants who had an excuse for poor performance on the first task and participants who had the highest number of options made more legitimate and illegitimate excuses after the first task. The number of items affected by option number was smaller than in Study 1, but this was not surprising given that Study 2 had considerably fewer participants, and it would have been necessary to have more participants than Study 1 to have the best chance at replicating all the same effects given comparable effect sizes (Abelson, 1995).

An interaction also qualified the main effects regarding one measure of excuse making. That interaction was supportive of the hypothesis that the presence of a good excuse and a large number of options would have an interactive effect such that the combination of conditions would produce more excuses than the individual conditions (hypothesis 10). Participants in the 18-option/excuse condition, which could be considered analogous to a real-world environment that frequently encourages excuses and contains excessive options, made more excuses than participants other conditions. This interaction was significant after the first task, and it also appeared to a lesser degree after the second task. The carry-over effect on the second questionnaire was particularly interesting, because there was no obvious reason for conditions

that applied to the first task (i.e., option number and the presence of a task-specific excuse) to have affected attributions for the second task. This same persistence outcome was found, albeit marginally in some cases, when measuring the main effects of the excuse and option-number factors on items in the second questionnaire. People were more likely to make excuses after their poor performance on the second task if they had been in the excuse condition or the 18-option condition in the first task. These main effects supported the hypotheses that excuse making would be higher when a salient excuse was available (hypothesis 8) and when the most options were present (hypothesis 9).

These patterns lend support to the idea that a pro-excuse environment developed in the experimental context in which the study took place. Conditions that made excuses more probable and socially acceptable in one situation continued to have that influence on an unrelated situation. For example, on the second questionnaire, participants in the 18-option condition reported that the task was less interesting to them and that they tried less hard—both items fall into the category of lack-of-obligation excuses (Schlenker et al., 2001). Participants in the excuse condition reported that the task was less personally important to them (another lack-of-obligation excuse), but they also (marginally) reported that the task was more difficult. Some of these excuses may have been somewhat legitimate (e.g., the 18-option participants may have been more cognitively fatigued than 6-option participants, so they really did perceive the second task as more difficult), but others were not. For example, the 18-option and 6-option participants differed in their description of their effort, but they did not differ on their actual performance. Analyses of the number of correct responses on the second task showed no differences in performance between 18-option and 6-option participants, $F < 1$. This pattern suggests that the

lack-of-effort excuse that was more heavily used by 18-option participants was not legitimate, because if they had really put forth less effort, then they should have performed more poorly.

People generally try to tailor excuses to fit the known facts about the situation to avoid seeming dishonest or unreliable, but they also are willing to go beyond the known facts when that departure will not be obvious (Schlenker, 1980). Participants seemed to generalize experiences and attributions they had during and immediately after the first task to attributions during and immediately after the second task. In summary, there was a carryover effect of too many options and a carryover effect of salient excuses present in the experimental context that led to participants making more excuses on unrelated tasks.

Social psychological research provides several explanations for the carryover effect of excuse making. For example, self-presentational research suggests that people who make excuses in a given situation establish a social identity (Schlenker, 1980; 1985) that they are motivated to maintain, in this case by being willing to make excuses in the future. Alternatively, research on catharsis suggests that acting on particular thoughts primes those thoughts in similar future behavioral contexts (Anderson, Carnagey, & Eubancks, 2003).

A secondary goal of Study 2 was to determine the relationship between excuse-making and affect regarding decision making, and although some data suggested that the relationship was consistent with hypothesis 7, the most straightforward relationship between excuses and affect (i.e., the relationship proposed in hypothesis 6) was not significant. If hypothesis 6 was correct, then people in the excuse condition should have felt more positive affect, because they knew going into the task that they could fail without having to take full responsibility for the failure, and they could look especially good if they ended up succeeding. However, this pattern did not present itself, as people in the excuse condition generally did not differ in affect from

people in the no-excuse condition, but when they did differ, people in the excuse condition were actually less happy.

This finding was contrary to other studies that have shown that experimentally manipulating the salience of an excuse generally causes people to be happier (e.g., McFarland & Ross, 1982; Mehlman & Snyder, 1985). For example, McFarland and Ross (1982) showed that by randomly assigning people to conditions in which excuses were either encouraged or not encouraged, participants in the pro-excuse condition showed more positive affect than those in the anti-excuse condition. However, my study differed in an important way from this previous study, because the excuse in my study was offered before the performance started, whereas the excuse in many excuse studies (e.g., McFarland & Ross, 1982; Mehlman & Snyder, 1985) was given to participants after their performance. Therefore, participants in the excuse condition in the current study were aware that they were disadvantaged relative to other groups in the study from the start, and receiving failure feedback after being stuck in a difficult condition may have been discouraging, thereby leading to negative affect. If one is told that one was randomly assigned to a very difficult situation, and then one does poorly, then what is there to be happy about?

Another possibility is that having the excuse in mind throughout the task increased the salience of excuse making in general. Being pressured to make excuses may feel unpleasant if one has any doubts that one's excuses are not legitimate, and it was possible that participants in Study 2 did feel that way. Mehlman and Snyder's (1985) study is relevant to this idea that making excuses only feels good if one is not highly aware of the strategy. Those authors demonstrated that the presence of an excuse only made participants feel better when they were not highly self-conscious (manipulated through the use of a bogus-pipeline—a technique that

convinces participants that their true attitudes are known by the experimenter). The presence of self-consciousness presumably increased the salience of any self-presentational and introspective concerns they may have had about excuse making. When highly self-conscious, participants with an excuse actually felt worse (Mehlman & Snyder, 1985). In my study, being told about the excuse and thinking about it throughout the task may have been similar to a self-consciousness manipulation in that participants could not ignore the possibility that they failed to overcome a disadvantage and then made excuses about it.

The notion that being aware of one's excuses might lead to worse affect is relevant to some correlational data that helped clarify the unexpected experimental effect that people in the excuse condition did not feel significantly better overall. The data in Study 2 indicated that making more excuses was associated with worse affect in the short run. However, because Study 2 had two questionnaires that were spaced out over a 15-minute period, it also allowed the luxury of a more long term examination. That examination showed that the people who made more excuses after the first task ended up feeling better after their failure in the second task. This pattern suggested that, when initially making excuses in this situation, people may have been aware at some level that they were trying to disengage responsibility for their failure, and that made them feel worse in the short run. However, once a little time and a few more distractions had passed, that disengagement of responsibility either began to feel less forced or perhaps a little more justifiable, and people ended up feeling better in the long run.

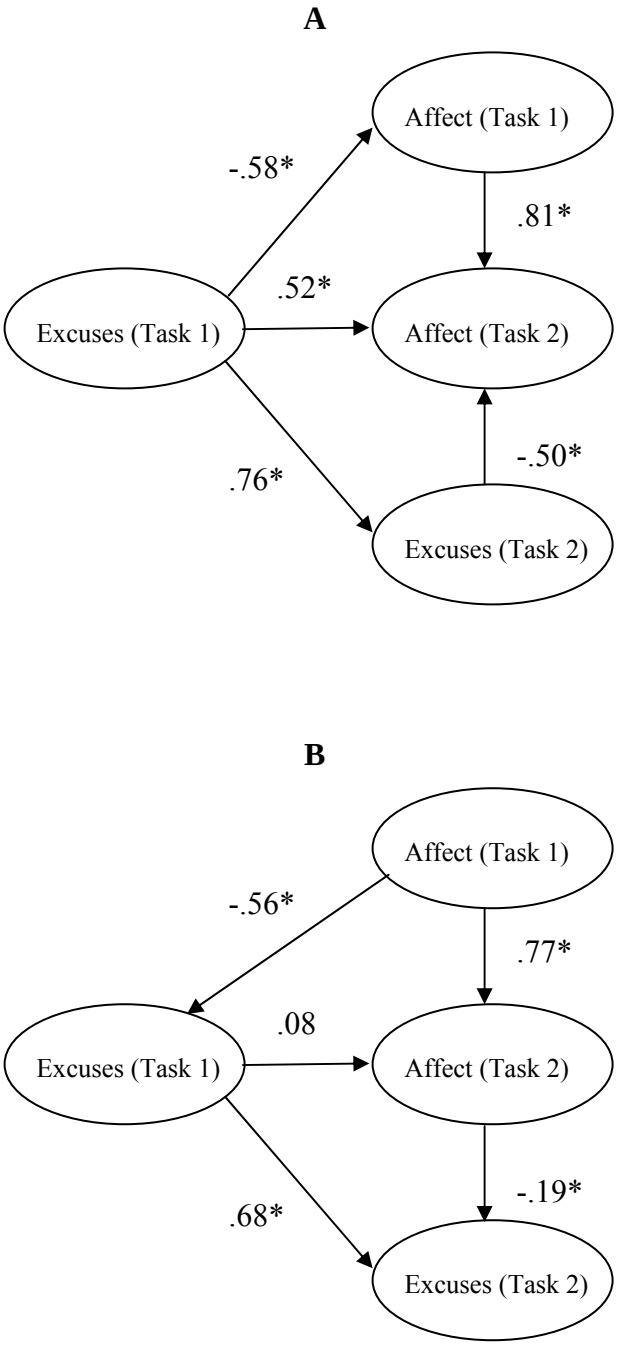

Figure 3-2. The SEM models linking excuse making (reverse-scored: higher numbers = more excuses) after first and second task and affect (higher numbers = better affect) after first and second task. Asterisks mark structural coefficients that were significant at $p < .05$. Standardized coefficients are reported.

Table 3-1. Main effects of option number on individual excuse items in Study 2

Item	18 Cars		6 Cars	
	M	*SD*	*M*	*SD*
Task difficulty in terms of time	5.03	2.21	4.01	2.21
Clarity of instructions	5.84	1.76	6.48	1.89
Overall task difficulty	4.80	2.10	4.21	1.90

Note. First and third rows reverse scored such that higher numbers for all three items equal more agreement with statement.

Table 3-2. Option number × excuse interaction on interest

	Excuse		No Excuse	
	M	SD	M	SD
6 Cars	5.43_{ad}	2.10	5.95_{cd}	1.75
18 Cars	5.56_{ac}	2.15	4.80_{ab}	1.90

Note. Values represent level of reported interest in task (higher number = more interest). Means not sharing subscripts differ at $p < .01$

Table 3-3. Option number × excuse interaction on task-interest excuses

	Excuse		No Excuse	
	M	SD	M	SD
6 Cars	5.76_{a}	1.09	5.66_{a}	.99
18 Cars	6.69_{b}	1.11	5.85_{a}	1.10

Note. Values (reverse-scored) represent excuse score on third excuse component (higher number = more excuses). Means not sharing subscripts differ at $p < .001$

Table 3-4. Factor loadings for excuse factors in second questionnaire of Study 2

Item Number	Item Wording	Factor 1 Dispositional	Factor 2 Situational
1.	User-friendliness of the software	-.14	**.80**
2.	Your skill with computer software	-.02	**.80**
3.	Speed of the presentation of each mathematical function	.05	**.62**
4.	Your memory for details	**.73**	.11
5.	The testing environment (e.g., lighting, background noise, room temperature, etc.)	.09	**.48**
6.	Your abstract reasoning ability	**.79**	.08
7.	Your ability to solve math problems	**.77**	.09
8.	Influence (e.g., clarity, ambiguity) of the computer instructions	.33	**.48**
9.	Influence (e.g., instructions, appearance) of the research assistant	.35	.09
10.	Your interest in the task	**.76**	-.18

Note. Factor loadings are standardized regression coefficients. Item 9 did not consistently load well on either factor, and it reduced the internal consistency for each factor if included, so it was excluded and analyzed separately.

Table 3-5. Zero-order correlations between excuses after tasks one and two and affect after tasks 1 and 2

	Variables			
	Excuses (1)	Excuses (2)	Affect (1)	Affect (2)
Excuses (1)	.79			
Excuses (2)	.60*	.76		
Affect (1)	-.51*	-.34*	.94	
Affect (2)	-.30*	-.37*	.68*	.94

Note. Asterisks mark significant correlations at $p < .0001$. Alphas are on diagonal. Excuses reverse-scored so that higher numbers = more excuses. Higher numbers = better affect.

CHAPTER 4
GENERAL DISCUSSION

This research was primarily designed to examine whether the increase in the size of choice sets that people must sift through on a daily basis affects excuse making. This is analogous to examining how a proliferation of choices may be related to the current climate of excuse permissiveness in western culture. This research was also designed to demonstrate that a social climate that is more permissive of excuse making in one context facilitates more excuse making in other contexts. I hypothesized that more options would lead to more excuse making and that providing a salient excuse to participants would produce more excuses even on a later, irrelevant task. Both hypotheses were supported. Thus, the primary goals of the presented studies were achieved, and the data painted an interesting picture that is deserving of future research. Together, the two studies suggested that a pro-excuse environment can develop even in a contrived, laboratory context, and once that environment has developed, it facilitates excuses that are both legitimate and illegitimate and that occur on relevant and irrelevant tasks. This implication is perhaps not surprising, because once norms for acceptable behavior are established, they can quickly take hold through the natural tendency people have to want to appear consistent when in front of a given audience (Schlenker, 2003; Schlenker, et al., 1994; Tice, 1992). These studies thus suggest that although a pro-excuse environment could develop from the pervasive experience of information overload that is more common today than ever before, a pro-excuse environment could also possibly perpetuate itself even without further influence from information overload.

There were also several hypotheses regarding the processes behind the effects of information overload on excuse making. It was hypothesized that people who felt poorly would make more excuses, and people who had salient excuses and who made more excuses would feel

better. Testing the mediating function of variables like affect and excuses is best done with experimental manipulation when possible rather than mediational analyses (Spencer, Zanna, & Fong, 2005), and that was the original goal of this research. However, the arousal manipulation in Study 1 did not succeed at manipulating affect in a significant and unambiguous manner, and the excuse manipulation in Study 2 actually resulted in worse affect in participants who were given an excuse. Thus, some of the hypotheses of interest were not supported experimentally, but they did receive some correlational support. The first path model in Study 2 provided support for the idea that excuses do help alleviate negative affect, but some time is necessary for the effect to occur. In the short run, making excuses may actually result in worse affect. However, given the correlational nature of the relationship of processes in the excuse-affect relationship, effects relevant to that relationship should be interpreted with caution, because successful experiments are the only way to draw confident conclusions about those effects.

The limitations of the presented studies existed primarily in the nature of the manipulations, but those limitations were necessary as a first step in this line of research. First of all, some difficult choices had to be made regarding how to manipulate option number while dealing the necessary choice between confounding option number with difficulty, time, or several other variables that covary with difficulty and time. The choice to confound option number with difficulty was made to most closely approximate the real world (i.e., situations in which people have a limited amount of time that is not constrained by option number to make a decision) and to maximize the possibility of seeing differences in excuse making if they did indeed exist. In the future, however, it will be important to employ other manipulations that keep difficulty constant while covarying other necessary confounds with option number. For example, a logical next step would be to give proportionally more time to conditions of higher

numbers of options. Another possibility would be to keep time constant but vary the number of attributes or the restrictions of the software such that people in the large- and small-option-number conditions perceive equivalent difficulty (and use equivalent cognitive resources) despite having the same amount of time. Regardless of the strategy, however, controlling for task difficulty and cognitive load will provide invaluable evidence regarding whether option number itself or some byproduct of option number is the critical component in determining excuse making.

Another limitation could be found in a lack of hypothesized effects on the two items that included variations of the word "responsibility." The items that addressed participants' perceptions of responsibility directly did not show differences by condition in either study, but that could be the result of participants' lack of insight into their attributional state. People are not always able to label their emotional states as accurately as researchers define them (Connelly & Butler's, 2006), and the same may be true for attributional states. In addition, participants may have claimed equal responsibility across conditions when the word itself was used in order to maintain the socially desirable image of "taking responsibility" for their failures, but they also took the opportunity to make specific excuses to lower responsibility in a more covert and thus socially acceptable manner.

Future research should examine the persistent effect on excuse making of large choice sets and pro-excuse environments. The data on this issue from Study 2 were encouraging, but they were not strong enough or complex enough to fully establish how pervasive the persistence of an externally influenced pattern of excuse making can be. Given the small effect sizes, more participants are obviously needed in future studies. In addition, a two-part study in which participants return to the lab at a later date (or to a different lab during the same session) with the

same experimenter present would be particularly informative, because it is possible that a pro-excuse environment is simply time-dependent and not context dependent (i.e., the effects observed in Study 2 would disappear if more time was allowed to pass). If participants from an excuse or large-option-number condition still made more excuses than participants in other conditions after more time had passed (or the environment changed), then that would say a lot about the robustness of the effect. It is not unreasonable to expect that this may occur, because research shows that norms that are established in a laboratory context can continue to influence participants after long periods outside of the lab (e.g., Sherif, 1936). This is likely to be especially true for participants who are made very aware that their responses have been seen by the experimenter and/or other participants in the study, and those audiences are supportive of the participants' responses.

Finally, the relationship between affect and excuses needs further attention, and this relationship could still be addressed satisfactorily with studies similar to the ones presented, but the manipulations would need to be changed. For example, the excuse manipulation could be moved to after the task, but that alone might not be enough to induce a positive effect on affect. Instead, the manipulation itself may need to differ in a way that allows participants more latitude for making excuses while simultaneously seeming less "unfair" to the participants in the excuse condition. Moreover, a measurement of "fairness" and believability would probably add something to the analysis by determining if the relationship between excuse availability and immediate affect could be explained in terms of these concepts. The affect manipulation would also need work, because a clean manipulation that really causes differences in affect is essential to determining if less negative affect truly causes fewer excuses. The correlational relationships present in the data of this research do little for firm conclusions to that end.

Barry Schwartz (2004) posited that having a huge variety of choices on a regular basis creates a "paradox of choice": people seem to be programmed to seek out options, but they are often worse off when they have too many. The presented studies add some relevant and important data to research on this topic. Although excuses are not inherently bad (they actually often seem to be effective in a variety of domains), the costs associated with chronic excuse making have not received adequate coverage in the excuse literature (Schlenker et al., 2001). One informative relationship can be found between locus of control and measures of cynicism, poor school outcomes, self-serving biases, depression, stress, and a host of other negative psychological outcomes. More external locus of control is associated with negative outcomes for all of these variables, and society's average locus of control seems to be getting more external over time (Twenge et al., 2004). Thus, more research on the topics in the presented studies is needed for at least two reasons. First, the basic processes behind and the limits of the effects need to be more clearly defined. Second, research is needed to suggest how people might overcome constant overexposure to too many options and avoid the obvious problems associated with sliding into the perception that their lives are controlled by influences that are more and more external.

APPENDIX A
STUDY 1: QUESTIONNAIRE

Note: The following questionnaires were presented in a program created in Visual Basic 6.0, so the format was slightly different, although the scale points were exactly the same. Participants could only see one item at a time, and they could not go back.

1. How much PATIENCE did you feel?

1…….....2…..……..3…….……4…….5
None At All A Little Some A Lot A Great Deal

2. How much EXCITEMENT did you feel?

1…….....2…..……..3…….……4…….5
None At All A Little Some A Lot A Great Deal

3. How many POSITIVE feelings did you experience?

1…….....2…..……..3…….……4…….5
None At All A Little Some A Lot A Great Deal

4. How many NEGATIVE feelings did you experience?

1…….....2…..……..3…….……4…….5
None At All A Little Some A Lot A Great Deal

5. How did you feel overall about your performance on the test?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
Extremely Bad Somewhat Bad Neutral Somewhat Good Extremely Good

6. How easy was it for you to complete the task given the amount of time you had?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
Not at all Easy A little Easy Somewhat Easy Very Easy Extremely Easy

7. How clear were the test instructions?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
Not at all Clear A little Clear Somewhat Clear Very Clear Extremely Clear

8. How well do you understand how your score was calculated?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
Not at all Well — Somewhat Well — Extremely Well

9. How interesting was the test?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
Highly Uninteresting — Neither Uninteresting Nor Interesting — Interesting — Highly Interesting

10. How much <u>personal responsibility</u> do you feel for your performance on the test?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
No Responsibility — A Little — Some — A lot — A Great Deal of Responsibility

11. How much do you feel that factors beyond your personal control (e.g., background noise, testing conditions) were responsible for your performance on the test?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
No Responsibility — A Little — Some — A lot — A Great Deal of Responsibility

12. How much do you think your score might improve if you took the test again under ideal testing conditions?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
None — A Little — Moderately — A lot — A Great Deal

13. How difficult was the test?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
Extremely Difficult — Difficult — Neither Difficult Nor Easy — Easy — Extremely Easy

14. How important was it to you personally to do well on the test?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
Not at all Important — A little Important — Somewhat Important — Very Important — Extremely Important

15. How hard did you try on the test?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9				
Not at all Hard	A little	Somewhat	Very	As Hard as I Could

Use the following scale for the next set of items:

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9				
Hurt my Performance A Lot	Hurt my Performance A Little	Had No Effect On My Performance	Helped my Performance A Little	Helped My Performance A Lot

16. User-friendliness of the software
17. Restrictions on seeing attributes for more than one car at a time
18. Number of car options
19. Your general knowledge of cars
20. Your skill with computer software
21. Number of attributes for each car
22. Your memory for details
23. The testing environment (e.g., lighting, background noise, room temperature, etc.)
24. Your abstract reasoning ability
25. Your ability to solve math problems
26. Influence (e.g., clarity, ambiguity) of the computer instructions
27. Influence (e.g., instructions, appearance) of the research assistant
28. Time limit imposed by the software
29. Your interest in the task

Answer the following questions by choosing a point along the continuum that represents how you feel about the decision task at this moment

30. Disappointment	1	2	3	4	5	6	7	Fulfillment
31. Discontent	1	2	3	4	5	6	7	Content
32. Alert	1	2	3	4	5	6	7	Tired
33. Satisfaction	1	2	3	4	5	6	7	Dissatisfaction
34. Happiness	1	2	3	4	5	6	7	Regret
35. Shame	1	2	3	4	5	6	7	Pride
36. Doubt	1	2	3	4	5	6	7	Confidence
37. Pleasure	1	2	3	4	5	6	7	Displeasure
38. Lucky	1	2	3	4	5	6	7	Unlucky
39. Elation	1	2	3	4	5	6	7	Depression
40. Joy	1	2	3	4	5	6	7	Sorrow

Please answer the following questions regarding your personal preferences

1……..…..2……..……3……..……4……5…..……..…6……..….7						
Completely Disagree	Strongly Disagree	Somewhat Disagree	Neutral	Somewhat Agree	Strongly Agree	Completely Agree

41. When I watch TV, I channel surf, often scanning through the available options even while attempting to watch one program.

42. When I am in the car listening to the radio, I often check other stations to see if something better is playing, even if I'm relatively satisfied with what I'm listening to.

43. I treat relationships like clothing: I expect to try a lot on before I get the perfect fit.

44. No matter how satisfied I am with my job, it's only right for me to be on the lookout for better opportunities.

45. I often fantasize about living in ways that are quite different from my actual life.

46. I'm a big fan of lists that attempt to rank things (the best movies, the best singers, the best athletes, the best novels, etc.).

47. I often find it difficult to shop for a gift for a friend.

48. When shopping, I have a hard time finding clothing that I really love.

49. Renting videos is really difficult. I'm always struggling to pick the best one.

50. I find that writing is very difficult, even if it's just writing a letter to a friend, because it's so hard to word things just right. I often do several drafts of even simple things.

51. No matter what I do, I have the highest standards for myself.

52. I never settle for second best.

53. Whenever I'm faced with a choice, I try to imagine what all the other possibilities are, even ones that aren't present at the moment.

54. Whenever I make a choice, I'm curious about what would have happened if I had chosen differently.

55. Whenever I make a choice, I try to get information about how the other alternatives turned out.

56. If I make a choice and it turns out well, I still feel like something of a failure if I find out that another choice would have turned out better

57. When I think about how I'm doing in life, I often assess opportunities I have passed up.

58. Once I make a decision, I don't look back.

Please answer the following questions regarding your perceptions of the test.

59. During the decision task, you were given a fixed time limit. What was your time limit?

1…….....2…..……..3…….……4…….5
1-2 minutes 3-4 minutes 5-6 minutes 7-8 minutes 9-10 minutes

60. How well did you perform on the test in terms of your percentile ranking?

1…….....2…..……..3…….……4…….5
0%-20% 21%-40% 41%-60% 61%-80% 81%-100%

61. How many different cars were there in the decision task?

1…….....2…..……..3…….……4…….5
1-4 5-8 9-12 13-16 17-20

62. Did you get feedback regarding your level of arousal after the task?

1…….....2…..……..3…….……4…….5
No, I received no arousal feedback — Yes, I received arousal feedback

63. According to the graph you saw of your own arousal, how aroused were you on average?

1…….....2…..……..3…….……4…….5
Not At All Aroused — A Little Aroused — Somewhat Aroused — Very Aroused — Extremely Aroused

…….6
I Did Not Receive Arousal Feedback

64. On average, how aroused did you feel DURING the decision task?

1…….....	2…..……..	3…….……..	4…….	5
Not At All Aroused	A Little Aroused	Somewhat Aroused	Very Aroused	Extremely Aroused

65. On average, how aroused did you feel AFTER the decision task?

1…….....	2…..……..	3…….……..	4…….	5
Not At All Aroused	A Little Aroused	Somewhat Aroused	Very Aroused	Extremely Aroused

APPENDIX B
STUDY 2: FIRST QUESTIONNAIRE

1. How did you feel overall about your performance on the test?

1	2	3	4	5	6	7	8	9
Extremely Bad		Somewhat Bad		Neutral		Somewhat Good		Extremely Good

2. How easy was it for you to complete the task given the amount of time you had?

1	2	3	4	5	6	7	8	9
Not at all Easy		A little Easy		Somewhat Easy		Very Easy		Extremely Easy

3. How clear were the test instructions?

1	2	3	4	5	6	7	8	9
Not at all Clear		A little Clear		Somewhat Clear		Very Clear		Extremely Clear

4. How well do you understand how your score was calculated?

1	2	3	4	5	6	7	8	9
Not at all Well				Somewhat Well				Extremely Well

5. How interesting was the test?

1	2	3	4	5	6	7	8	9
Highly Uninteresting		Uninteresting		Neither Uninteresting Nor Interesting		Interesting		Highly Interesting

6. How much <u>personal responsibility</u> do you feel for your performance on the test?

1	2	3	4	5	6	7	8	9
No Responsibility		A Little		Some		A lot		A Great Deal of Responsibility

7. How much do you feel that factors beyond your personal control (e.g., background noise, testing conditions) were responsible for your performance on the test?

1	2	3	4	5	6	7	8	9
No Responsibility		A Little		Some		A lot		A Great Deal of Responsibility

8. How much do you think your score might improve if you took the test again under ideal testing conditions?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
None A Little Moderately A lot A Great Deal

9. How difficult was the test?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
Extremely Difficult Neither Difficult Easy Extremely
Difficult Nor Easy Easy

10. How important was it to you personally to do well on the test?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
Not at all A little Somewhat Very Extremely
Important Important Important Important Important

11. How hard did you try on the test?

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
Not at all A little Somewhat Very As Hard as
Hard I Could

Use the following scale for the next set of items:

1..…….2…..…….3…..…….4…..…….5…..…….6…..…….7……..….8…..…….9
Hurt my Performance A Lot | Hurt my Performance A Little | Had No Effect On My Performance | Helped my Performance A Little | Helped My Performance A Lot

12. User-friendliness of the software
13. Restrictions on seeing attributes for more than one car at a time
14. Number of car options
15. Your general knowledge of cars
16. Your skill with computer software
17. Number of attributes for each car
18. Your memory for details
19. The testing environment (e.g., lighting, background noise, room temperature, etc.)
20. Your abstract reasoning ability
21. Your ability to solve math problems
22. Influence (e.g., clarity, ambiguity) of the computer instructions
23. Influence (e.g., instructions, appearance) of the research assistant
24. Time limit imposed by the software

25. Your interest in the task

Answer the following questions by choosing a point along the continuum that represents how you feel about the decision task at this moment

26.	Disappointment	1	2	3	4	5	6	7	Fulfillment
27.	Discontent	1	2	3	4	5	6	7	Content
28.	Alert	1	2	3	4	5	6	7	Tired
29.	Satisfaction	1	2	3	4	5	6	7	Dissatisfaction
30.	Happiness	1	2	3	4	5	6	7	Regret
31.	Shame	1	2	3	4	5	6	7	Pride
32.	Doubt	1	2	3	4	5	6	7	Confidence
33.	Pleasure	1	2	3	4	5	6	7	Displeasure
34.	Lucky	1	2	3	4	5	6	7	Unlucky
35.	Elation	1	2	3	4	5	6	7	Depression
36.	Joy	1	2	3	4	5	6	7	Sorrow

Please answer the following questions regarding your perceptions of the test.

60. How well did you perform on the test in terms of your percentile ranking?

1…….....2…..……..3…….……4…….5
0%-20% 21%-40% 41%-60% 61%-80% 81%-100%

61. How many different cars were there in the decision task?

1…….....2…..……..3…….……4…….5
1-4 5-8 9-12 13-16 17-20

APPENDIX C
STUDY 2: SECOND QUESTIONNAIRE

1. How did you feel overall about your performance on the test?

1	2	3	4	5	6	7	8	9
Extremely Bad		Somewhat Bad		Neutral		Somewhat Good		Extremely Good

2. How clear were the test instructions?

1	2	3	4	5	6	7	8	9
Not at all Clear		A little Clear		Somewhat Clear		Very Clear		Extremely Clear

3. How well do you understand how your score was calculated?

1	2	3	4	5	6	7	8	9
Not at all Well				Somewhat Well				Extremely Well

4. How interesting was the test?

1	2	3	4	5	6	7	8	9
Highly Uninteresting		Uninteresting		Neither Uninteresting Nor Interesting		Interesting		Highly Interesting

5. How much <u>personal responsibility</u> do you feel for your performance on the test?

1	2	3	4	5	6	7	8	9
No Responsibility		A Little		Some		A lot		A Great Deal of Responsibility

6. How much do you feel that factors beyond your personal control (e.g., background noise, testing conditions) were responsible for your performance on the test?

1	2	3	4	5	6	7	8	9
No Responsibility		A Little		Some		A lot		A Great Deal of Responsibility

7. How much do you think your score might improve if you took the test again under ideal testing conditions?

1	2	3	4	5	6	7	8	9
None		A Little		Moderately		A lot		A Great Deal

8. How difficult was the test?

1	2	3	4	5	6	7	8	9
Extremely Difficult		Difficult		Neither Difficult Nor Easy		Easy		Extremely Easy

9. How important was it to you personally to do well on the test?

1	2	3	4	5	6	7	8	9
Not at all Important		A little Important		Somewhat Important		Very Important		Extremely Important

10. How hard did you try on the test?

1	2	3	4	5	6	7	8	9
Not at all Hard		A little		Somewhat		Very		As Hard as I Could

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Use the following scale for the next set of items:

1	2	3	4	5	6	7	8	9
Hurt my Performance A Lot		Hurt my Performance A Little		Had No Effect On My Performance		Helped my Performance A Little		Helped My Performance A Lot

11. User-friendliness of the software
12. Your skill with computer software
13. Speed of the presentation of each mathematical function
14. Your memory for details
15. The testing environment (e.g., lighting, background noise, room temperature, etc.)
16. Your abstract reasoning ability
17. Your ability to solve math problems
18. Influence (e.g., clarity, ambiguity) of the computer instructions
19. Influence (e.g., instructions, appearance) of the research assistant
20. Your interest in the task

Answer the following questions by choosing a point along the continuum that represents how you feel about the decision task at this moment

21. Disappointment	1	2	3	4	5	6	7	Fulfillment
22. Discontent	1	2	3	4	5	6	7	Content

23. Alert	1	2	3	4	5	6	7	Tired
24. Satisfaction	1	2	3	4	5	6	7	Dissatisfaction
25. Happiness	1	2	3	4	5	6	7	Regret
26. Shame	1	2	3	4	5	6	7	Pride
27. Doubt	1	2	3	4	5	6	7	Confidence
28. Pleasure	1	2	3	4	5	6	7	Displeasure
29. Lucky	1	2	3	4	5	6	7	Unlucky
30. Elation	1	2	3	4	5	6	7	Depression
31. Joy	1	2	3	4	5	6	7	Sorrow

_____

_____

32. How well did you perform on the test in terms of your percentile ranking?

1	2	3	4	5
0%-20%	21%-40%	41%-60%	61%-80%	81%-100%

Please answer the following questions regarding your personal preferences

1	2	3	4	5	6	7
Completely Disagree	Strongly Disagree	Somewhat Disagree	Neutral	Somewhat Agree	Strongly Agree	Completely Agree

33. When I watch TV, I channel surf, often scanning through the available options even while attempting to watch one program.

34. When I am in the car listening to the radio, I often check other stations to see if something better is playing, even if I'm relatively satisfied with what I'm listening to.

35. I treat relationships like clothing: I expect to try a lot on before I get the perfect fit.

36. No matter how satisfied I am with my job, it's only right for me to be on the lookout for better opportunities.

37. I often fantasize about living in ways that are quite different from my actual life.

38. I'm a big fan of lists that attempt to rank things (the best movies, the best singers, the best athletes, the best novels, etc.).

39. I often find it difficult to shop for a gift for a friend.

40. When shopping, I have a hard time finding clothing that I really love.

41. Renting videos is really difficult. I'm always struggling to pick the best one.

42. I find that writing is very difficult, even if it's just writing a letter to a friend, because it's so hard to word things just right. I often do several drafts of even simple things.

43. No matter what I do, I have the highest standards for myself.

44. I never settle for second best.

45. Whenever I'm faced with a choice, I try to imagine what all the other possibilities are, even ones that aren't present at the moment.

46. Whenever I make a choice, I'm curious about what would have happened if I had chosen differently.

47. Whenever I make a choice, I try to get information about how the other alternatives turned out.

48. If I make a choice and it turns out well, I still feel like something of a failure if I find out that another choice would have turned out better

49. When I think about how I'm doing in life, I often assess opportunities I have passed up.

50. Once I make a decision, I don't look back.

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51. During the first task, you were given a fixed time limit. What was your time limit?

1…….....2…..……..3…….……4…….5
1-2 minutes 3-4 minutes 5-6 minutes 7-8 minutes 9-10 minutes

52. Given your time limit, how difficult should the first task have been according to the instructions you were given before the task?

1…….....2…..……..3…….……4…….5
Not At All Difficult | A Little Difficult | Somewhat Difficult | Very Difficult | Extremely Difficult

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BIOGRAPHICAL SKETCH

Ryan Johnson was born in Sacramento, California in 1980. He moved to Gainesville, Florida at age 6 and remained through graduate school. He received a B.S. in psychology in 2003, a M.S. in social psychology in 2005, and he plans to receive a Ph.D. in social psychology in May 2008, all from the University of Florida.