Coming in Under the Radar: When Intention Questions Change Behavior

PATTI WILLIAMS
GAVAN J. FITZSIMONS
LAUREN G. BLOCK*

Revision currently in process for Journal of Consumer Research.
*Patti Williams (pattiw@wharton.upenn.edu) is the James G. Campbell, Jr. Memorial Term Assistant Professor of Marketing and Gavan J. Fitzsimons (gavan@wharton.upenn.edu) is Assistant Professor of Marketing at the Wharton School, University of Pennsylvania, 1400 SHDH, Philadelphia, PA 19104-6371. Lauren G. Block is Associate Professor of Marketing at the Zicklin School of Business, Baruch College, 1 Bernard Baruch Way, Marketing Department, B12-240, New York, NY 10010 (Lauren_Block@baruch.cuny.edu).
Previous research has shown that the act of answering an intention question can lead to a greater likelihood of engaging in the target behavior. This mere-measurement effect is thought to occur because of the automatic increase in accessibility of the target attitude. However, we suggest that this presents only a partial picture of the mechanism at work. Many actions taken by marketers, including persuasion attempts (e.g., advertising) may lead to automatic attitude activation, but may not result in such overt behavioral changes. Over the course of three experiments, we test the hypothesis that mere-measurement effects occur because asking an intention question slips under the radar of our persuasion knowledge. In Experiments 1 and 2, we show that when manipulative intent is attributed to an intention question, people can adjust their behavior in response to the question, as long as they have sufficient cognitive capacity to permit conscious correction. In Experiment 3 we demonstrate that when respondents are educated that an intention question could be a persuasive attempt, the behavioral impact of those questions is attenuated.
How likely are you to buy a Starbuck’s coffee today? If in fact by the end of the day, you find yourself ordering a Tall Iced Skim Latte (hold the whipped cream), you might be able to shift some of the blame onto us. A considerable amount of research in consumer behavior and psychology has demonstrated that the simple act of asking questions leads to biased responses on the part of the respondents (e.g., Simmons, Bickart and Lynch 1993; Feldman and Lynch 1988). Moreover, research has found that not only does asking questions lead to biased responses, but it can often change the underlying behavior itself (e.g., Morwitz, Johnson and Schmittlein 1993; Sherman 1980). Thus the act of answering a simple intention question can lead not only to an over-prediction of the respondent’s likelihood to engage in the target behavior, but ultimately a greater likelihood to engage in the behavior itself. A variety of terms have been used to describe this phenomenon: the mere-measurement effect (e.g., Fitzsimons and Morwitz 1996; Chapman 2001); the self-erasing error of prediction (Sherman 1980); and the self-prophecy effect (Spangenberg and Greenwald 1999). Hereafter, we refer to it as the mere-measurement effect. This effect has been documented in a wide variety of behavioral domains including purchase of automobiles, voting, volunteering for charities and becoming a cable television subscriber.

Questions remain regarding why the simple act of answering an intention question actually causes behavioral change in such a significant and consistent manner. This is all the more puzzling when more overt efforts to change behavior, such as advertising persuasion attempts, often do not result in substantial, observable behavioral changes. The present research suggests that answering a seemingly innocuous question regarding future intentions slips below the level of our defenses, causing automatic or non-conscious
changes in cognitive structure that lead to behavioral changes of which the respondent is often not aware. In contrast, overt persuasion attempts are more likely to be filtered by targets through the lens of persuasion knowledge (Friestad and Wright 1994), which leads respondents to engage in effortful processing of the persuasion episode in an attempt to cope with it and adjust their behaviors accordingly. In a series of three studies, we test the premise that intention questions can unknowingly influence our behavior because they come in under our radar for persuasive tactics. We first demonstrate that mere-measurement effects occur for personal health behaviors, a domain typically regarded as one in which consumers’ defenses are most rigorously employed. Moreover, we show that when intention questions are perceived as manipulative, the mere-measurement effect is reduced (Experiment 1). Consistent with prior suggestions that using persuasion knowledge is an effortful strategy while the mere-measurement effect is an automatic one, we next demonstrate that the mere-measurement effect is only attenuated when consumers have full cognitive capacity (Experiment 2). Finally, we demonstrate the “change of meaning principle” (Friestad and Wright 1994) by teaching consumers about the mere-measurement effect, enabling them to defend themselves against the potential manipulative impact of intention questions (Experiment 3). The final experiment also clearly demonstrates the mediating role persuasion knowledge plays in the mere-measurement effect.

We begin with a review of the mere-measurement effect, followed by a discussion of the literature on consumer persuasion knowledge.

THE MERE-MEASUREMENT EFFECT
What is the Mere-Measurement Effect and Where Does It Occur?

In his research examining the link between stated intentions and actual behavior, Sherman (1980) found that for socially desirable behaviors, respondents systematically over-predicted their likelihood to perform the behaviors, relative to a control group not asked to predict, while for socially undesirable behaviors, systematic under-prediction was observed. Moreover, these biased predictions ultimately turned out to be “self-erasing,” as respondents were more likely to behave in accordance with their biased responses. For example, while only 4% of control group participants (not asked about their intentions) volunteered 3 hours to the American Cancer Society, 48% (22 of 46) of those who were asked about their willingness to volunteer said they would do so, over-predicting their likelihood relative to the control group. But, amazingly, 31% (14 of 45) of this group actually did agree to volunteer, far outstripping the 4% volunteer rate among their peers. Thus, the errors of over-prediction became self-erasing, as respondents ultimately acted in accordance with their biased stated intentions.

Similar effects have been found in a variety of other behavioral domains. For example, registered voters asked about their intentions to vote indicated they were more likely to vote than control group subjects, and ultimately did vote with greater probability relative to their peers (Greenwald, Carnot, Beach and Young 1987). Mere-measurement effects have also been found to increase long-term use of a health club among club members who had not attended the club during the last six months, relative to a similar group not asked about expected attendance (Spangenberg 1997), and to reduce cheating
among college students asked whether or not they would engage in cheating (again, relative to a control group; Spangenberg and Obermiller 1996).

Asking consumers their purchase intentions has also been shown to increase purchases of consumer goods and services. Consumers asked to imagine themselves as cable television subscribers increased the rate at which they subscribed relative to a control group (Gregory, Cialdini and Carpenter 1982). Morwitz, Johnson and Schmittlein (1993) demonstrated that the act of measuring intentions to purchase both automobiles and personal computers led to increased purchase rates among those asked. For example, 3.3% of consumers who were asked a purchase intention question regarding the automobile category made a purchase of a new car within the next 6 months, versus a purchase rate of 2.4% in a control group not asked intent.

The Mere-Measurement Effect: How Does It Work?

Some work has addressed the process through which the mere-measurement effect actually occurs. In his original work, Sherman (1980) argued that the act of giving a prediction evokes a cognitive representation of a script or sequence of actions that then re-emerges when the imagined situation is at hand. Due to its increased accessibility, this cognitive representation then directs behavior in accordance with the script.

A study by Fitzsimons and Morwitz (1996) supports Sherman’s suggestion that increased accessibility in part underlies the mere-measurement effect. Fitzsimons and Morwitz (1996) argued that asking a purchase intention question about a product category leads to activation of that category in memory. This activation then spreads to brands in
the category, in proportion to the prior accessibility of existing cognitions about the brands. Thus, when people are asked about category purchase intention, a previously highly accessible brand is most likely to be activated in memory, relative to a brand with low accessibility. Experimental results were consistent with this, showing that among current car owners, the increase in choice incidence associated with answering an intention question accrued to the brand of car they currently used. Non-car owners asked about intentions to buy a car, by contrast, were more likely to purchase a large market share brand. Thus, both owners and non-owners asked about purchase intentions more often bought the brand of car most readily accessible in memory prior to assessment of their intentions in the category.

Moreover, the direction of the mere-measurement effect (behavioral increases versus behavioral decreases relative to a control group) has been found to depend on the valence of the underlying attitude toward the target behavior. Thus, asking a question regarding intentions to choose a brand with an accessible and positive attitude increases choice of that brand relative to a control group, while asking the same question about a brand with an accessible and negative attitude decreases choice incidence relative to a control (Morwitz, Johnson and Schmittlein 1993; Morwitz and Fitzsimons 2002).

These explanations suggest that the mere-measurement effect is driven by the heightened accessibility of attitudes associated with the target behavior due to responding to an intention question. This, in turn, is suggestive of an automatic, rather than thoughtful and deliberative, process underlying the effect. Consistent with this, Fitzsimons and Williams (2001) used process dissociation procedures (Jacoby 1991, Pham and Johar 1997) to isolate the automatic versus effortful components of the mere-measurement
effect. Across several experiments, the effect of asking intentions on subsequent behavior was found to be primarily an automatic process. This finding suggests that the mere-measurement effect occurs not because respondents to intention questions deliberate upon the question, form a conscious intention and then thoughtfully carry out that intention at some later point. Rather, it suggests that the effect is primarily due to the automatic activation of an intention-related behavioral script. Fitzsimons and Shiv (2001) provide convergent evidence of a largely non-conscious effect of asking questions on subsequent behaviors. They found that the behavior of respondents to hypothetical questions was strongly biased by the content of the questions, despite conscious denials by the respondents that this could be the case. The automaticity of the mere-measurement effect suggests that it may be difficult to avoid, unless respondents choose to override the effect by invoking relevant effortful processes.

The Mere-measurement Effect and Persuasion Knowledge: Coming in Under the Radar

The literature to date on the process underlying the mere-measurement effect strongly implicates an automatic, non-conscious increase in accessibility of the target attitude. However, we suggest that this presents only a partial picture of the mechanism at work. Many actions taken by marketers, including persuasion attempts (e.g., advertising) may lead to automatic attitude activation, but may not result in such overt behavioral changes. What then can account for the differential effects between asking an intention question and presenting a consumer with an advertisement?
We suggest that asking an intention question comes in under the defensive radar consumers use when confronted with an overt persuasive attempt. In other words, asking an intention question is simply not perceived as an intent to manipulate, therefore people are not suspect and do not know how to adjust their interpretation and coping processes accordingly. The idea that people use their knowledge of persuasion motives and tactics to interpret, evaluate and respond to influence attempts from marketers and others was first introduced in a seminal paper by Friestad and Wright (1994). Coining it the “Persuasion Knowledge Model” (PKM), Friestad and Wright offer a broadened view of persuasion that emphasizes a consumer’s capacity to learn about persuasion over time, including how consumers manage their own psychological activities in persuasion episodes.

The model argues that consumers possess knowledge about persuasion attempts, which includes causal explanatory beliefs about the psychological states and processes thought to mediate the effect of a persuasion agent’s attempts to influence another person’s behaviors. In addition, consumers hold topic knowledge (beliefs about the topic of the persuasion message) and agent knowledge (beliefs about the traits, competencies and goals of the persuasion agent). These knowledge structures allow consumers to “recognize, analyze, interpret, evaluate and remember persuasion attempts and to select and execute coping tactics believed to be effective and appropriate” (Friestad and Wright 1994, pg. 3), as well as to determine when persuasion tactics are appropriate or are manipulative.

These knowledge structures develop over time as consumers are exposed to tactics and come to recognize them as such. The process by which a tactic comes to be perceived as having persuasive intent is termed the “change of meaning principle” and offers
important implications for how consumers interpret actions by persuasion agents. For example, an action that is not perceived by consumers to be a tactic may result in greater compliance, as the action itself does not evoke persuasion knowledge or accompanying coping behaviors. In contrast, once an agent’s action has been recognized as a tactic, consumers are more likely to attempt to cope and perhaps protect themselves from what they perceive the tactic and its potential impact upon their behavior to be. In the present research we argue that typical questions regarding future behavioral intentions are not perceived as a persuasion tactic and thus have a greater impact upon consumer behavior than are other actions that consumers do view as having clear persuasive intent.

In support of our theorizing, Sherman (1980) originally suggested that the self-prophecy effect would not occur if respondents suspected a connection between the prediction phase and the behavior phase. This suggests that if respondents perceive a questioner to have a stake in the future behavior under question, they are likely to view the intention question with some suspicion, perhaps treating it more like a persuasion episode rather than an innocuous question about future behavior. Interestingly, one published paper that did not find a mere-measurement effect after measuring intent occurred in a context in which consumers might have inferred some persuasive intent behind the intention question (Obermiller, Spangenberg and Atwood 1992). In this case, alumni of a university asked by its representatives about intentions to donate money to the school did not contribute at a rate greater or lesser than a control group not asked intent. The lack of effect was attributed to reactance (Brehm 1966) among the intentions group to perceived manipulative intent by their alma mater’s development office. Though not discussed in that paper, these results suggest that the mere-measurement effect may be diminished or
eliminated if respondents perceive the question itself to be part of a persuasion attempt, and thus are motivated to activate their persuasion knowledge to understand and cope with the question.

In the current paper we present a series of three experiments designed to explore whether the mere-measurement effect occurs because answering a presumably innocuous question slips below our level of defenses. In other words, consumers do not activate persuasion knowledge when asked intent because such intention questions are not interpreted or recognized as a persuasion tactic.

**EXPERIMENTAL OVERVIEW**

Over the course of three experiments, we test the hypothesis that mere-measurement effects occur because asking an intention question slips under the radar of our persuasion knowledge. In Experiment 1 we examine potential mere-measurement effects when respondents perceive an intention question to have persuasive (manipulative) intent due to sponsorship of the question by a self-interested (profit seeking) sponsor. We expect that in such circumstances, respondents’ persuasion knowledge will be activated and will result in the attenuation of the mere-measurement effect relative to the same question when un-sponsored or when posed by an objective sponsor.

We further explore the implication that an inability to access and apply persuasion knowledge partially accounts for the mere-measurement effect in a second study. In Experiment 2, we manipulate the availability of cognitive resources among respondents at the time of answering an intention question. Prior studies have shown that use of persuasion knowledge is most likely when consumers have adequate cognitive resources
(Campbell and Kirmani 2000). In other words, interpreting information through the lens of persuasion knowledge is often an effortful rather than an automatic process. As the typical intention question is unlikely to be perceived to have persuasive intent, it is unlikely then, that consumers would activate their persuasion knowledge when asked such a question. And, without adequate cognitive resources to sustain the effortful use of persuasion knowledge, they may not be able to rely upon that knowledge even when the question might be perceived to have persuasive intent. Therefore, under reduced cognitive resources, we expect that a mere-measurement effect will occur even when a self-interested sponsor asks the intention question.

Finally, in Experiment 3 we seek to educate respondents that an intention question could be used as a persuasive tactic. Development of persuasion knowledge accrues over time, depending on the maturation of cognitive skills, accumulated experience, and socialization with respect to persuasive attempts like advertising and personal selling (Friestad and Wright 1994). When a person begins to perceive some new tactic as a persuasive attempt, a “change of meaning” occurs. No longer innocent, the person can then respond accordingly, altering his/her reaction to the persuasive attempt. In Experiment 3, we teach subjects about the mere-measurement effect. We expect that when respondents are aware of the mere-measurement effect, they will view even un-sponsored intention questions as persuasion attempts, and any behavioral impact of those questions will be attenuated.

Experiment 3 is also designed to demonstrate the mediating role of persuasive knowledge on the question-behavior link. In experiments 1 and 2 the behavior respondents were asked intentions about occurred over a one-week period following the intention
question. As a result of the time elapsed between intention question and behavior, we were unable to collect reliable measures of perceived manipulative intent from respondents. In Experiment 3 we changed the target behavior from a naturally occurring health behavior to a behavior that we could immediately measure in the laboratory: propensity to volunteer for a charity. As a result we were able to collect direct measures of the perceived manipulative intent of the intention question within only a few minutes of the question. These direct measures provide an opportunity to determine whether perceived manipulative intent mediates the intention question-behavior link.

EXPERIMENT 1: MANIPULATING PERCEIVED PERSUASIVE INTENT

Participants

Participants were 232 undergraduate students at a large northeastern university in the United States who took part in the experiment that was conducted as part of an in-class exercise on marketing research. Sixty-nine participants took part in a pretest and 232 participants took part in the main experiment.

Design and Procedure

In this experiment, the general behavior of interest was either a positive health related, socially desirable behavior (i.e., flossing teeth) or a negative, socially undesirable, health related behavior (i.e., eating fatty foods). Previous research on the mere-measurement effect has shown that the valence of the attitude toward the target behavior predicts the direction of behavioral change – positive, socially desirable behaviors
increase as a result of an intention question while negative, socially undesirable behaviors decrease. Our expectation was that asking an intention question would increase flossing and decrease consumption of fatty foods. For each of the two behaviors there were four conditions of interest: control, intent-only, sponsored-objective source and sponsored-self-interested source. In the control condition, participants were not asked an intention question about the target behavior (the control condition for the positive behavior was asked intent for the negative, and vice versa). Participants in the intent-only condition were asked a simple intention question either about the positive or negative behavior (“How likely are you to floss your teeth in the next week?” or “How likely are you to eat fatty foods in the next week?”; where 1 = definitely will and 7 = definitely will not). The two other conditions received the same intention question, and in addition had a small tag in the lower right hand corner of the page underneath the intention question that identified the apparent sponsor of the research. In the sponsored-self-interested source condition, the sponsor of the research had a direct and readily apparent self-interest and profit motive in the results of the question and the behavior itself. In this condition, the self-interested sponsor of the flossing behavior question was the Association of Dental Products Manufacturers, while the self-interested sponsor of the fatty food behavior question was the American Fruit Growers Association, both fictitious organizations. For the sponsored-objective source condition, we sought sponsors who would be equally credible, but less clearly profit motivated. Thus, we used two organizations with a clear interest in the research question, but a much less direct immediate benefit from the research: the American Dental Association (for flossing) and the American Medical Association (for fatty foods).
Note however, that the direction of the motivation varied across the two self-interested sponsors. Presumably the Association of Dental Products Manufacturers would be motivated to encourage additional flossing while the American Fruit Growers Association would prefer less eating of fatty foods in favor of additional fruit consumption. Thus, if respondents make adjustments in their target behaviors based upon the perceived manipulative intent of the questioner, these adjustments would not be in the same direction across the conditions. While an intention question about a socially desirable behavior should increase the rate of that behavior, if the respondent perceives the question to have a persuasive goal, we would expect respondents to respond to the manipulative intent by exhibiting less of an increase in flossing behavior (than if they had not perceived manipulative intent in the question). Similarly, while an intention question about a socially undesirable behavior should decrease the rate of that behavior, if persuasive intent is detected, respondents may decrease their rate of fatty food consumption to a lesser degree than do respondents that do not perceive manipulative intent.

One week after completion of the intention question in the first portion of the experiment, respondents were given a follow up questionnaire that measured the number of times they had eaten fatty food and flossed their teeth over the past week.

In addition to the main experiment, a pretest was conducted to measure the degree to which respondents’ persuasion knowledge would be activated as they responded to each of the three different types of intention questions. Participants read either an intention question about flossing or eating fatty foods that was either (i) just the intent question, (ii) the question sponsored by an objective source, and (iii) the question sponsored by a self-
interested source (see above for the specific questions and sponsors). They then answered two questions (adapted from Campbell and Kirmani 2000) designed to measure whether persuasion knowledge was activated by the intention question: “The purpose of the question on the previous page was to change my behavior,” and “While I read the question I thought it was pretty obvious that the author of the question was attempting to persuade me.” Respondents answered each question on a 1-7 strongly disagree to strongly agree scale.

**Results**

The pretest results demonstrated that our manipulation of persuasion knowledge was successful. The two items had high internal reliability ($\alpha = .82$) and were thus averaged to form a persuasive intent index. As analysis showed no difference due to behavior type ($F(1, 68) < 1$) nor an interaction between behavior type and sponsor type ($F(1, 68) < 1$), the positive (flossing) and negative (eating fatty foods) behaviors were collapsed. There was a significant main effect of sponsor type ($F(1, 68) = 66.33, p < .001$). In the intent question only conditions, perception of perceived intent was $m = 2.43$. This was significantly lower than perceived persuasive intent in the objective source sponsored intent question ($m = 3.20, F(1, 68) = 5.82, p < .02$) which was in turn significantly lower than perceived persuasive intent in the self-interested sponsor intent question conditions ($m = 5.89; F(1,68) = 73.05, p < .001$).

Of the original 232 respondents in the main experiment only 205 were present in class one week later – thus the other 28 participants were not included in the following
analyses. For the socially undesirable behavior, eating fatty foods, the control group reported a base rate of eating fatty foods of 4.33 times over the one-week period. Consistent with our expectations, those in the intent-only condition (asked about intention to eat fatty food) reported a significantly reduced rate of fatty food consumption (1.95 times, $F(1, 204) = 16.60, p < .001$). Respondents asked an intention question that had been sponsored by an objective source also reported a significantly reduced rate of fatty food consumption (2.85 times, $F(1, 204) = 5.02, p < .05$). By contrast, respondents who were asked an intention question that had been sponsored by a self-interested source reported fatty food consumption that was not significantly lower than the control group (3.74 times, $F(1, 204) = 1.25, p = .31$). This pattern of data is strongly supportive of our hypothesis, namely that the mere-measurement effect will occur unless the respondents’ persuasion knowledge is activated as it would be when a self-interested source is asking the intention question. Note that those asked a self-interested sponsored question had significantly more fatty food consumption than those in the intent-only condition (3.74 vs. 1.95, $F(1, 204) = 6.45, p < .01$). No other contrasts were significant. See Table 1 for means for each condition.

Table 1 about here

Analogous results were obtained for the socially desirable behavior, flossing. The control group not asked a flossing intention question reported baseline flossing of 6.41 times over the one-week period. Those asked simply a flossing intention question reported flossing 10.00 times over the same period ($F(1,204) = 23.82, p < .001$). Those asked flossing intent by an objective sponsor reported flossing behavior that was still
significantly greater than the control group (8.30 times, $F(1, 204) = 4.33, p < .05$). When respondents were asked a flossing intention question by a self-interested sponsor however, not only was a significant increase not observed, but the rate of flossing observed was actually substantially lower than the control group (1.75 times, $F(1, 204) = 40.21, p < .001$). This suggests, as did the results for the socially undesirable behavior, that when a self-interested sponsor asks the intention question, the respondent’s persuasion knowledge is activated, and in this case actually led to a backlash effect in which people flossed less than they would have as a result of the perceived persuasion attempt. Again we find that respondents’ behavior after having been asked an intention question by a self-interested sponsored was significantly different from those in the question-only condition (1.75 vs. 10.00, $F(1, 204) = 87.67, p < .001$) and objective-sponsor condition (1.75 vs. 8.30, $F(1, 204) = 40.40, p < .001$). No other contrasts were significant.

Discussion

Consistent with our expectations, we observed the basic mere-measurement effect in both socially desirable and undesirable behavioral domains. For a socially desirable behavior, flossing, asking a question led to an increase in flossing behavior while for a socially undesirable behavior, eating fatty food, asking an intention question led to a decrease in the behavior. These changes as a result of being asked an intention question were not significantly attenuated when the intention question was sponsored by an objective source. However, when the question was asked by a self-interested source respondents perceived higher levels of persuasive intent in the question, and this led to an
attenuation of the effects for the negative behavior, and a reversal of the intent-behavior effect for the positive behavior.

Results of Experiment 1 demonstrate that a correction of the mere-measurement effect can occur when respondents’ persuasion knowledge is activated. Using persuasion knowledge to interpret a persuasive attempt is an effortful process, and is most likely to occur when consumers have adequate cognitive resources available for processing. Without adequate cognitive resources to sustain the effortful use of persuasion knowledge, consumers may not be able to apply their knowledge of a persuasion tactic despite recognizing it as such. In the next experiment, we manipulate respondents’ cognitive resources at the time of answering an intention question. We expect that under reduced cognitive resources a mere-measurement effect will occur even when a self-interested sponsor asks the intention question.

EXPERIMENT 2: REDUCING COGNITIVE RESOURCES

Participants

Participants were 221 undergraduate students at a large northeastern university in the United States who took part in the experiment that was conducted as part of an in-class exercise on marketing research.

Design and Procedure

The design of experiment 2 was identical to that of experiment 1, with the addition of another factor – whether or not cognitive capacity was constrained. We employed a
divided attention task to constrain cognitive capacity as respondents were reading and responding to the intention question. Respondents under constrained cognitive capacity conditions were asked to keep track of the number of times they blinked as they read and responded to the intention question. This manipulation is one that has been shown to successfully reduce cognitive capacity, also in the context of the mere-measurement effect (Fitzsimons and Williams 2000). Respondents not in the constrained capacity condition also performed a blink counting task but completed it prior to reading and responding to the intention question.

Results

Of the 221 participants who completed the first phase of the experiment, 172 were present for the second phase one week later. The remaining observations were set aside.

As a check that participants followed our divided attention instructions, a comparison was made between the number of reported blinks for those in constrained versus unconstrained cognitive capacity conditions. Participants in unconstrained cognitive capacity conditions completed the blink counting task prior to being asked the category intent condition. By contrast, participants in the constrained cognitive capacity conditions continued the blink counting task for a considerably longer period. Thus we would expect that if participants were accurately tracking the number of times they blinked we would observe greater blink counts for the constrained capacity participants than for the unconstrained capacity participants. As expected, participants in the
unconstrained conditions reported a mean number of blinks of 3.93 versus a mean of 8.84 for the constrained capacity conditions ($t(171) = 6.89, p < .001$).

The pattern of results obtained in Experiment 2 largely replicates that of Experiment 1. For those respondents whose cognitive capacity was not constrained, control group fatty food consumption was 6.78 times. This was reduced to 3.78 for those in the question only condition ($F(1,164) = 8.25, p < .001$) and to 3.46 for those asked a question by an objective sponsor ($F(1,164) = 13.55, p < .001$). As in Experiment 2 those asked the intention question by a self-interested sponsor reported a level of fatty food consumption that did not differ from the control group (7.13 times; $F(1,164) < 1$).

Examining flossing behavior for those respondents whose cognitive capacity was not constrained found that the control group (not asked intent) flossed their teeth 2.78 times in one week. If simply asked an intention question this rate rose to 5.26 times ($F(1,164) = 23.05, p < .001$). If asked an intention question by an objective sponsor this rate was also significantly greater than the control flossing rate (4.80 times; $F(1,164) = 11.28, p < .001$). However, as in Experiment 1, if asked intent by a self-interested sponsor the rate of flossing did not increase – in fact, once again it decreased, although not significantly in this case (2.00 times; $F(1,164) = 1.51, p = .22$). Overall, the pattern of results for participants that did not receive a divided attention task largely replicates that observed in Experiment 1. Table 2 presents means for each condition.

Of more interest in the current study is the degree to which constraining cognitive capacity had an impact on the effect of asking an intention question on behavior. For each
of the behaviors studied, constraining the respondent’s cognitive capacity did not significantly change the number of times respondents engaged in the behavior in three of the four question conditions: the control group not asked an intention question, those that received simply an intention question, or those that received an intention question from an objective sponsor (all planned contrasts between constrained and unconstrained capacity were non-significant for these forms of intention question; all \( p \)’s > .10). Table 2 reports all of these means by condition. For example, participants asked about intent to floss their teeth by an objective sponsor under no cognitive load flossed 4.8 times, while those asked intent by an objective sponsor under cognitive load flossed 5.1 times \( (F(1,164) < 1) \). This result suggests that in each of these conditions no conscious awareness was required for the effect of an intention question on behavior to be observed. Had conscious awareness been necessary, we should have observed a difference between behavior rates in the constrained versus unconstrained capacity conditions.

By contrast however, for both positive and negative behaviors, when participants were asked an intention question by a self-interested source, constraining cognitive capacity led to different behavior rates. For eating fatty food, when no constraint was applied, respondents consumed fatty foods 7.13 times when a self-interested source asked the intention question. This was reduced to 4.39 times if they responded to the question under cognitive load \( (F(1,164) = 7.53, p < .01) \). Similarly, for flossing, under no cognitive load, respondents flossed 1.89 times when a self-interested source asked the intention question but flossed 4.94 times if their cognitive capacity was constrained \( (F(1,164) = 14.26, p < .001) \). Comparing participants in the constrained cognitive capacity/self-interested sponsor conditions to the appropriate control groups shows that the imposition
of a cognitive load leads to the reemergence of a significant mere-measurement effect. When capacity is constrained, asking an intent question from a self-interested source leads to both a significant increase in flossing behavior (4.94 times) versus a control group (2.67 times, $F(1,164) = 16.28, p < .001$), and a significant decrease in fatty food consumption (4.39 times) versus a control group not asked intent (6.96 times; $F(1,164) = 10.42, p < .001$).

**Discussion**

The results of Experiment 2 provide strong support for our argument that the degree to which respondents to an intention question can correct for its potential effect upon subsequent behavior depends critically on availability of cognitive resources. Respondents must have adequate cognitive resources to activate their persuasion knowledge in order to both recognize the potential persuasion attempt and to respond to it. If cognitive resources are not available, respondents will behave very much like respondents that had been asked a simple intention question or one that was not perceived to be a persuasive attempt. In Experiment 3 we try to encourage the activation of persuasion knowledge in response to an intention question by explicitly describing the mere-measurement effect to respondents, effectively turning intention questions into a tactic, thereby providing “change of meaning.” To the degree that this technique reduces the mere-measurement effect it may provide an important inoculating tool to battle the effect in contexts in which it is undesirable, or has detrimental effects on the respondent.
EXPERIMENT 3: STIMULATING A CHANGE OF MEANING

Participants

Participants were 108 undergraduate students at a large northeastern university in the United States who took part in the experiment in return for partial course credit.

Design and Procedure

In this experiment, only a socially desirable behavior, volunteering for a charitable organization, was examined. All participants were asked to read what was presented as a research abstract from the *Journal of Consumer Research*. Those participants in the change of meaning condition read an abstract about the mere measurement effect while the remainder read about attitude stability. The mere measurement abstract was adapted for this purpose from a previously published paper (Fitzsimons and Morwitz 1996) while the control abstract was taken from a second, unrelated article prepared by one of the current paper's authors. Both abstracts were matched as closely as possible for length (word count) familiarity to an undergraduate student; see Appendix A for the complete abstracts. All participants were then told that they would be learning about a real charitable organization, the Teach for America Program, and subsequently asked questions regarding their opinions of this organization. Subjects then read a short overview of the organization’s mission and short quotations from several volunteers about their experiences (see Appendix A). All participants who were given change of meaning
information were asked the intention question: how likely or unlikely they would be to participate in the Teach for America program upon their graduation from college (1 = Definitely would participate; 7 = Definitely would not participate). Some of the participants who read the unrelated attitude information were also asked the same question; this condition mirrors the standard mere-measurement condition (intention question provided with no change of meaning information). Those in the control condition received the unrelated abstract but were not asked an intention question.

All participants were asked questions about their past volunteering behaviors (frequency of volunteer activities, which specific organizations they had volunteered for and amount of time per week they typically volunteer). Participants were then told that if they were interested in learning more about the Teach for America organization, they should provide their email or mailing address and they would be contacted with additional information. Participants in the two conditions in which intentions were measured also completed the questions used in previous experiments designed to measure the degree to which their persuasion knowledge had been activated.

Results

The manipulation check data showed a successful change of meaning – ability to interpret an intention question as a persuasive tactic -- among those who received an intention question (n = 73). After reading an abstract describing the mere-measurement effect, the change of meaning condition participants believed the intention question they answered had a high degree of persuasion intent (m = 5.24). Those in the control group
who read an unrelated research abstract perceived a lesser degree of persuasive intent behind the intention question ($m = 1.77$; $F(1, 72) = 218.8$, $p < .001$).

In addition, the mere-measurement effect was observed as expected. Because of the binary nature of the dependent variable, analysis was conducted using a logistical regression procedure. Results show a significant main effect of question condition ($\chi^2(2, N = 108) = 11.38$, $p < .01$). Among those not asked intent, 11% (4 of 35) provided their email or mailing addresses, asking for additional information about volunteering for Teach For America. In contrast, among those asked an intention question, but who received no change of meaning information about the mere-measurement effect, 34% (12 of 35) requested additional information, significantly more than in the control condition ($\chi^2 (1, N = 108) = 4.77$, $p < .03$). After receiving information about the potential impact of answering intention questions on subsequent behavior, the mere-measurement effect was attenuated such that just 13% (5 of 38) of participants that were asked about intent to volunteer requested additional information about Teach For America. This proportion was significantly different from that among the group asked intentions but who did not receive change of meaning information ($\chi^2(1, N = 108) = 4.28$, $p < .04$), but is not significantly different from the control condition ($\chi^2 (1, N = 108) < 1$, $p = .81$). Thus, it appears that participants in the change of meaning condition were educated about the potential persuasive impact of intention questions and raised their defenses in response.

More direct evidence of this process explanation is provided by testing for the mediating role of perceived persuasive intent in the mere-measurement effect. This analysis was performed on the 73 participants who responded to an intention question – both those who were asked an intention question and received no change of meaning
information \((n = 35)\) and those who did receive change of meaning information \((n = 38)\).

For perceived persuasive intent to mediate the effect of asking intentions on subsequent behavior four criterion must be satisfied (Baron and Kenny 1986). First, the independent variable must be significantly related to the dependent variable. Whether change of meaning information was provided or not was significantly related to whether participants requested additional information about Teach For America \((\chi^2(1, N = 73) = 4.50, p < .05)\). Second, the independent variable must be significantly related to the proposed mediator. Whether change of meaning information was provided or not was significantly related to perceived persuasive intent \((F(1, 72) = 298.0, p < .001)\). Third, the proposed mediator must be significantly related to the dependent variable. Perceived persuasive intent was significantly related to whether participants requested additional information about Teach for America \((\chi^2(1, N = 73) = 8.53, p < .01)\). Finally, including the proposed mediator in the first regression should show a significant relationship between the mediator and the dependent variable and lead the previously significant independent variable – dependent variable relationship to drop from significance. In this logistic regression, while perceived persuasive intent was significantly related to whether participants requested additional information about Teach For America \((\chi^2(1, N = 73) = 8.17, p < .01)\), whether change of meaning information was provided or not was no longer significantly related to requests for additional information \((\chi^2(1, N = 73) = 2.10, p = .15)\).

In summary, the degree to which providing change of meaning information to respondents reduces the effect of asking intent on subsequent behavior is mediated by whether they perceived the question to be an attempt to persuade them.
Discussion

Results from this experiment suggest that consumers can be educated about the potential impact of intention questions on their subsequent behavior, and can come to view such questions as persuasion tactics. Thus, “change of meaning” for intention questions can be obtained, resulting in an attenuation of the mere-measurement effect similar to that observed in the previous experiments when intention questions were sponsored by self-interested organizations. In addition, in Experiment 3 we were able to collect individual level measures of perceived persuasive intent as the behavior measure occurred in the same session as the intent question was asked (permitting following up with persuasive intent measures while the intent question was relatively fresh in the respondents’ minds). This permitted us to demonstrate the mediating role of perceived persuasive intent in attenuating the mere-measurement effect: only when respondents felt the question was attempting to persuade them did they adjust their behavior and reduce the magnitude of the mere-measurement effect.

GENERAL DISCUSSION

The results obtained from the series of three studies presented here provide compelling support that mere-measurement effects occur, at least in part, because people do not think that being asked an intention question is a persuasive attempt. Unlike advertising or a direct sales call, for which our guard is up against an unwanted persuasive appeal, intention questions are perceived as benign queries, incapable of influencing our
behavior. In Experiments 1 and 2, we show that when manipulative intent is attributed to an intention question, people can adjust their behavior in response to the question, as long as they have sufficient cognitive capacity to permit conscious correction. In the case of the flossing behavior, this correction was so strong it resulted in a backlash effect (whereby flossing not only did not increase, but decreased below control levels if the intention question was asked by a self-interested sponsor). Finally, in Experiment 3, results demonstrate that consumers can be educated about the potential impact of intention questions on subsequent behavior and that “change of meaning” can occur. Once this process has taken place, participants are able to cope with these questions and adjust their behaviors accordingly.

These results provide further evidence that consumers’ subsequent behavior is automatically affected by being asked an intent question. It is only when substantial cognitive capacity is available that respondents are able to adjust for the perceived persuasive intent of the question. This process is analogous to results observed in a consumer advertising context. When presented with a series of advertising claims of unknown truthfulness, Hawkins and Hoch (1992) found that consumers increased the degree to which they believed the advertising claims as a simple function of whether they had been previously exposed to the claims or not. They found that this “truth effect” (Hasher, Goldstein and Toppino 1977) was found to be much more prevalent when consumers were in low involvement learning settings (e.g., when cognitive capacity devoted to learning and evaluating the claims was low). In other words, just as with the mere-measurement effect, to avoid falling prey to the truth effect consumers needed to
have sufficient cognitive resources to correct an otherwise automatic increase in the felt truthfulness of the advertising claims.

Previous research has suggested that mere-measurement effects will occur only when socially desirable behaviors are the focus of attention (Spangenberg and Greenwald 1999). The experiments reported here suggest that is not the case. Eating fatty foods is unlikely to be perceived as socially desirable, even among those who have positive attitudes toward such behaviors. In fact, the behaviors examined in this research are likely to have complex underlying attitudinal structures, such that individuals may simultaneously hold both positive and negative (ambivalent) attitudes toward them. Thus consumers may know that consuming fatty foods is bad for them, yet nonetheless find such consumption enjoyable (e.g., a “guilty pleasure”; Giner-Sorolla 2001), or know that flossing their teeth is a good thing, yet find doing so unpleasant (e.g., a “grim necessity” Giner-Sorolla 2001). The ambivalent attitudinal structures underlying the behaviors examined in this paper may give rise to the backlash effects observed, particularly in Experiment 1. Since consumers feel both positively and negatively toward the behavior they can easily adjust their behaviors in either direction, toward or away from the behavior, depending upon what they perceive the goal of the persuasion agent to be. Such behaviors offer a rich domain for future investigations. Further complicating this picture is that the ambivalent attitude structures may be a combination of implicit and explicit attitudes. Fitzsimons, Nunes and Williams (2002) explore this issue in more depth in their study of vice behaviors – those for which society and often the user themselves hold negative explicit attitudes and the user holds an implicit positive attitude.
It is interesting to speculate further on Sherman’s (1980) original thinking that measuring intentions appears to activate an entire behavioral schema associated with the behavior. If this is true, then other means of activating the same behavioral schema ought to produce similar results, assuming these other means are also not perceived as persuasive attempts. Dhalokia and Morwitz (2002) provide initial support for this idea by measuring satisfaction with consumers’ financial services providers. The mere-measurement of satisfaction was found to increase aggregate customer product use and profitability, and to decrease rates of defection, all relative to customers not asked about their satisfaction levels. Moreover, these effects were found to have considerable persistence, resulting in effects that appeared for months after the original point of measurement and which lasted even a year later.

While nearly every paper on the topic of mere-measurement has offered warnings regarding the unintended impact of survey questions on respondents’ future behavior, the present results seem to make that warning even more compelling. The results from the experiments reported here offer convincing evidence regarding the potential unintended hazards of measurement. For example, at-risk populations are often identified based upon their answers to questions regarding their likelihood to engage in the target at-risk behaviors. This research suggests that very well-meaning organizations may be having serious and negative impacts upon those they intend to help, inadvertently provoking more of these risky behaviors by asking those most at risk about their future behavioral intentions with respect to those behaviors. It is only when respondents perceive intent questions to have manipulative intent, and have sufficient cognitive capacity to correct for this fact, that the intent question does not result in unwanted changes in behavior. This
may describe only a small portion of the situations respondents find themselves in when responding to intent questions from consumer or public policy researchers. Researchers and practitioners must be mindful of the potential effects of measurement on future behavior and should begin to address methods of dealing with, or overcoming these potential biasing effects.
APPENDIX A

Change of meaning condition:
A number of studies have demonstrated that the act of forming and reporting a response to an intentions question can alter respondents’ subsequent behavior, a phenomenon coined the mere-measurement effect. This mere-measurement effect has been demonstrated in contexts involving both specific (e.g., donating time to a specific charity) and general behaviors (donating time to any charity). Sherman (1980) and Greenwald et al. (1987) demonstrated that measuring intentions to perform a specific behavior (e.g., donating time to a specific charity, voting in a specific election) changed participants’ likelihood of engaging in that specific behavior, compared to a control group whose intentions were not measured. Morwitz et al. (1993) demonstrated a comparable effect occurs when intentions to engage in a general behavior are measured. Specifically they showed that measuring intentions to perform a general behavior (e.g., buy any brand of automobile, buy any brand of personal computer) changed participants’ likelihood of engaging in that general behavior, compared to a control group whose intentions were not measured. For example, those individuals asked if they intended to purchase a car were more likely to actually buy a car than were those not asked about their intentions to purchase. Thus, the simple act of answering a question about future behavioral intentions can actually change future behavior.

Control condition:
This research draws on two competing models of the attitude response process to formulate and test an overall framework for attitude stability – i.e., the degree of correspondence between initial and delayed attitudes. According to the construction model of attitudinal response, attitudes are always constructed afresh; and are therefore susceptible to contextual influences, leading to a low degree of attitude stability across time. On the other hand, the retrieval-based model argues that a delayed attitude is formed by simply retrieving an earlier attitude, and mapping it on to the given response scale. This viewpoint dictates a high degree of stability, irrespective of the contextual cues salient at the point of delay. Our framework combines these ideas and suggests that factors that encourage a retrieval-based process at delay (e.g., high accessibility of the initial attitude) should promote attitude stability, regardless of the contextual cues available at delay. Alternatively, under conditions that encourage a construction-based process (e.g., low accessibility of the initial attitude), stability can be increased by reinstating the particular informational cues that were responsible for initial attitude formation. These ideas are tested in terms of the relative influence of visual and verbal cues on initial and delayed brand attitudes. The obtained findings are strongly supportive of the predictions derived from our theoretical framework. Implications of our findings for the attitude stability area, as well as the construction/retrieval debate, are discussed.
Appendix A continued

We are the national corps of outstanding and diverse recent college graduates of all academic majors who commit two years to teach in urban and rural public schools. Since 1990, we have focused the leadership and passion of some of our nation's most talented graduating seniors on expanding educational opportunity for our nation's most disadvantaged students. During their two-year commitments, corps members have a powerful impact on their students' lives and on the schools in which they teach. After their two years, Teach For America alumni bring their unique perspective and experience to every sector of professional life, where they remain lifelong advocates for making an excellent education available to all children.

What is Teach for America like? The experience varies, of course, but two themes are constant: the corps years are extremely challenging...and incredibly rewarding.

Many corps members say that their first semester of teaching was the most daunting task they ever faced. Yet looking back on the experience, 95% would join Teach For America all over again. Why? Their impact in the classroom. The support of fellow corps members. The unequaled opportunity to learn.

Alan Guiliani '92, taught algebra and calculus at Central High School in West Helena Arkansas. After majoring in math at New Mexico State University. "I view my students as extraordinary, so they must know this material with world-class accuracy."

Michael Johnston '97, taught English at Greenville High School in Greenville, Mississippi after graduating from Yale with a degree in English and Philosophy. "[My student] composed a two-page essay comparing the tenets of mythology and Christianity that was an unmitigated triumph."

Jen Averill '95, taught seventh and eighth grade writing and reading at Fort Rosa Middle School in Tunica, Mississippi after graduating from Tufts University. "The thirty-sixth time I kneel by my student's desk and tell him I still believe he has so much to share, he writes, 'How can I be a lover in this place?'"

Source: www.teachforamerica.org
REFERENCES


Greenwald, Anthony G., Catherine G. Carnot, Rebecca Beach and Barbara Young (1987), “Increasing Voting Behavior by Asking People if They Expect to Vote,” *Journal of Applied Psychology*, 72 (May), 315-318.


Table 1: Experiment 1 Results

Rate of Flossing or Consuming Fatty Foods by Intent Condition

<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>Intention question only</th>
<th>Intention question by objective sponsor</th>
<th>Intention question by self-interested sponsor</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fatty Food Consumption</td>
<td>4.33</td>
<td>1.95</td>
<td>2.85</td>
<td>3.74</td>
</tr>
<tr>
<td>Flossing</td>
<td>6.41</td>
<td>10.00</td>
<td>8.30</td>
<td>1.75</td>
</tr>
</tbody>
</table>
Table 2: Experiment 2 Results

Rate of Flossing or Consuming Fatty Foods by Intent Condition and Cognitive Capacity

<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>Intention question only</th>
<th>Intention question by objective sponsor</th>
<th>Intention question by self-interested sponsor</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Table 2a – Fatty food consumption</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Normal cognitive capacity</td>
<td>6.78</td>
<td>3.78</td>
<td>3.46</td>
<td>7.13</td>
</tr>
<tr>
<td>Constrained cognitive capacity</td>
<td>6.96</td>
<td>4.18</td>
<td>3.80</td>
<td>4.45</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>Intention question only</th>
<th>Intention question by objective sponsor</th>
<th>Intention question by self-interested sponsor</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Table 2b – Flossing rate</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Normal cognitive capacity</td>
<td>2.78</td>
<td>5.26</td>
<td>4.80</td>
<td>2.00</td>
</tr>
<tr>
<td>Constrained cognitive capacity</td>
<td>2.66</td>
<td>6.25</td>
<td>5.08</td>
<td>4.94</td>
</tr>
</tbody>
</table>
Figure 2: Experiment 3 Results

<table>
<thead>
<tr>
<th>Percent Requesting Additional Information</th>
<th>Control</th>
<th>Intent-Measured, No Change of Meaning</th>
<th>Intent-Measured, Change of Meaning</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>11%</td>
<td>34%</td>
<td>13%</td>
</tr>
</tbody>
</table>