

# When Consumers Do Not Recognize “Benign” Intention Questions as Persuasion Attempts

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We demonstrate that the mere-measurement effect occurs because asking an intention question is not perceived as a persuasion attempt. In experiments 1 and 2, we show that when persuasive intent is attributed to an intention question, consumers adjust their behavior as long as they have sufficient cognitive capacity to permit conscious correction. In experiment 3 we demonstrate that this finding holds with product choice and consumption, and we find that persuasion knowledge mediates the effects. In experiment 4, we show that when respondents are educated that an intention question is a persuasive attempt, the behavioral impact of those questions is attenuated.

How likely are you to buy a Starbucks 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 has demonstrated that the simple act of asking questions leads to biased responses on the part of the respondents (e.g., Feldman and Lynch 1988; Simmons, Bickart, and Lynch 1993). 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 an intention question can lead not only to an overprediction of the respondent's likelihood to engage in the target behavior, but ultimately to a greater likelihood to engage in the behavior itself. We refer to this phenomenon as the “mere-measurement effect,” although it has also been referred to as the “self-erasing error of prediction” (Sherman 1980).

Questions remain regarding why the simple act of answering an intention question actually causes behavioral change in such a significant and consistent manner. The present research suggests that answering a seemingly innocuous question regarding future intentions slips under the radar of our defenses, causing automatic or nonconscious

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 through the lens of persuasion knowledge (Friestad and Wright 1994). In four studies, we test the premise that intention questions can unknowingly influence behavior because they are not perceived to be manipulative or to have persuasive intent.

By identifying the role of persuasive intent, our article contributes to the understanding of how and why the mere-measurement effect occurs. First, it allows us to distinguish conditions when and why certain automatic changes in cognitive structure result in increased intentions while others do not. Further, by understanding the conditions under which the mere-measurement effect occurs, we also identify how this effect can be overridden or attenuated. For example, our studies demonstrate that making the persuasive nature of the question salient results in attenuation of the mere-measurement effect. Likewise, educating people about the mere-measurement effect results in a similar attenuation by providing the “change of meaning” that alters consumers' knowledge structures about persuasive tactics. In addition, by replicating our findings on both socially desirable and undesirable behaviors, we resolve extant speculation that the mere-measurement effect would not occur for socially undesirable behaviors.

## THE MERE-MEASUREMENT EFFECT

In his research examining the link between stated intentions and actual behavior, Sherman (1980) found that for socially desirable behaviors, respondents systematically overpredicted their likelihood to perform the behaviors, relative to a control

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group not asked to predict, while for socially undesirable behaviors, systematic underprediction occurred. 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 volunteered 3 hr. to the American Cancer Society, 48% (22 of 46) of those who were asked about their willingness to volunteer said that they would do so, overpredicting 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 overprediction became self-erasing, as respondents 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 participants, and they ultimately did vote with greater probability relative to their peers (Greenwald et al. 1987). Morwitz et al. (1993) demonstrated that measuring intentions to purchase both automobiles and personal computers led to increased purchase rates among those asked.

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 reemerges when the imagined situation is at hand. Because of 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. They 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. Experimental results showed that both car owners and non-owners who were asked about purchase intentions more often bought the brand of car most readily accessible in memory. Moreover, the direction of the mere-measurement effect 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, while asking the same question about a brand with an accessible and negative attitude decreases choice incidence relative to a control (Morwitz et al. 1993).

This heightened accessibility of attitudes is suggestive of an automatic, rather than a thoughtful and deliberative, process underlying the effect. To test this, Fitzsimons and Williams (2000) used process dissociation procedures 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, suggesting that the mere-

measurement effect results from the automatic activation of an intention-related behavioral script, rather than a respondent’s conscious deliberation on the intention question and a determination to carry out that intention at some later point.

### THE MERE-MEASUREMENT EFFECT AND PERSUASION KNOWLEDGE

The extant studies on mere-measurement and attitude accessibility present only a partial picture of the mechanism at work. We suggest that intention questions do not prompt consumers to activate the coping processes on which they likely rely when confronted with an overt persuasive attempt. Asking an intention question is simply not perceived as an attempt to influence; therefore, consumers are not suspicious and do not know 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 this idea the Persuasion Knowledge Model, 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. 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 as the tactic and its potential impact on their behavior. 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 on consumer behavior than other actions that consumers do view as having clear persuasive intent.

In support of our theorizing, Sherman (1980) originally suggested that such effects 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 per-

suasive appeal than an innocuous question about future behavior.

In the current article we present a series of four 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. Participation in all experiments was part of an in-class exercise for undergraduate marketing students.

## EXPERIMENT 1: MANIPULATING PERCEIVED PERSUASIVE INTENT

### Method

Two hundred and thirty-two participants took part in the experiment. The general behavior of interest was either a positive health-related, socially desirable behavior (flossing teeth) or a negative socially undesirable, health-related behavior (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—thus, positive, socially desirable behaviors should increase as a result of an intention question, while negative, socially undesirable behaviors should decrease. Our expectation was that asking an intention question would increase flossing and decrease consumption of fatty foods. We utilized a one-way design with four levels (intention question form: control, intent-only, sponsored-objective source, and sponsored-self interested source) for each of two behaviors.

For each of the behaviors there were three forms of question asked: intent-only, sponsored-objective source, and sponsored-self-interested source. In addition, participants who were not asked an intention question about the target behavior served as control conditions (i.e., the control conditions for the positive behavior were 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, below which was a small tag identifying the apparent sponsor of the question. In the sponsored-self-interested source condition, the sponsor had a direct, readily apparent self interest and profit motive in the results of the question and the behavior itself. 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 re-

search question, but a much less direct immediate benefit from the research: the American Dental Association (flossing) and the American Medical Association (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 on 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 expect respondents to respond to the persuasive intent by exhibiting less of an increase in flossing behavior (than if they had not perceived persuasive 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 who do not perceive persuasive intent. One week after completion of the intention question, respondents were given a follow-up questionnaire that measured the number of times that they had eaten fatty food and flossed their teeth over the past week.

In addition to the main experiment, two pretests were conducted. The first measured the degree to which respondents' persuasion knowledge was activated in response to each of the three different types of intention questions. Sixty-nine participants read either an intention question about flossing or eating fatty foods that was either (i) just the intention question, (ii) the question sponsored by an objective source, or (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.

The second pretest, administered to a separate group of 22 participants, measured the degree to which they felt that the sponsor of the question in each of the sponsored question conditions was objective versus self-interested on the subject of either flossing teeth or eating fatty foods. They responded to three items about each of the four sponsors used in the main experiment indicating the degree to which they felt the sponsor was 1 = “Not at all self interested” to 7 = “Very self interested,” 1 = “Did not have direct profit motive” to 7 = “Had a very direct profit motive,” and 1 = “Not at all objective” to 7 = “Very objective.” The third item was reverse scaled, and question order was randomized.

TABLE 1

EXPERIMENT 1 RESULTS: RATE OF FLOSSING OR CONSUMING FATTY FOODS BY INTENT CONDITION

|                        | Control | Intention question only | Intention question by objective sponsor | Intention question by self-interested sponsor |
|------------------------|---------|-------------------------|---|---|
| Fatty food consumption | 4.33    | 1.95 <sup>a, b</sup>    | 2.85 <sup>a</sup>                       | 3.74 <sup>b</sup>                             |
| Flossing               | 6.41    | 10.00 <sup>a, b</sup>   | 8.30 <sup>a</sup>                       | 1.75 <sup>a, b</sup>                          |

<sup>a</sup>Indicates means that are significantly different from the relevant control condition mean at the  $p < .05$  level.

<sup>b</sup>Indicates means that are significantly different from one another at the  $p < .05$  level.

Results

*Pretest Results.* The results of the first pretest demonstrated that our manipulation of persuasion knowledge was successful. The two items were averaged to form a persuasive intent index ( $\alpha = .82$ ). As analysis showed no main effect of 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 intention-question-only conditions, perception of perceived intent ( $M = 2.43$ ) was significantly lower than perceived persuasive intent in the objective sponsor-intention 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-intention question conditions ( $M = 5.89; F(1, 68) = 73.05, p < .001$ ).

Results of the second pretest were consistent with the first. The three items were averaged to form an index of perceived self-interest ( $\alpha = .81$ ). As expected, on the topic of flossing, the Association of Dental Products Manufacturers was perceived to be more self-interested ( $M = 5.38$ ) than was the American Dental Association ( $M = 3.30; F(1, 21) = 27.48, p < .001$ ). Similarly, on the subject of eating fatty foods, the American Fruit Growers Association was perceived to be more self-interested ( $M = 4.74$ ) than the American Medical Association ( $M = 3.05, F(1, 21) = 17.72, p < .001$ ).

*Main Study Results.* Of the original 232 respondents in the main experiment, only 205 were present in class 1 week later—thus, the other 27 participants were not included in the following analyses. As we anticipated, a one-way ANOVA on the number of reported behaviors 1 wk. after intent was measured yielded a significant main effect of intention question type for the socially undesirable behavior, eating fatty foods ( $F(3, 201) = 6.15, p < .001$ ). The control group reported a base rate of eating fatty foods of 4.33 times over the 1-wk. period. Consistent with expectations, those in the intent-only condition (asked about intention to eat fatty food) reported a significantly reduced rate of fatty food consumption relative to the control group (1.95 times;  $F(1, 201) = 16.60, p < .001$ ). Respondents asked an intention question that had been sponsored by an objective source also reported a significantly reduced rate of fatty food consumption relative to the control group (2.85 times;

$F(1, 201) = 5.02, p < .05$ ). 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;  $F(1, 201) = 1.05, p = .31$ ). 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;  $F(1, 201) = 6.45, p < .01$ ). No other contrasts were significant.

Analogous results were obtained for the socially desirable behavior, flossing, with a significant main effect of intention question type ( $F(1, 201) = 31.49, p < .0001$ ; see table 1). The control group not asked a flossing intention question reported a baseline flossing rate of 6.41 times over the 1-wk. period. Those asked simply a flossing intention question reported flossing 10.00 times ( $F(1, 201) = 23.82, p < .0001$ ). Those asked flossing intent by an objective sponsor reported flossing behavior that was still significantly greater than the control group (8.30 times;  $F(1, 201) = 4.33, p < .05$ ). 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 vs. 6.41 times;  $F(1, 201) = 40.21, p < .0001$ ). 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, leads to a backlash effect. Again we find that, after having been asked an intention question by a self-interested sponsor, respondents' behavior was significantly different from those in the question-only condition (1.75 vs. 10.00;  $F(1, 201) = 87.67, p < .0001$ ) and the objective-sponsor condition (1.75 vs. 8.30;  $F(1, 201) = 40.40, p < .0001$ ). No other contrasts were significant.

*Summary.* Consistent with expectations, we observed the 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 mere measurement effects were not 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.

The results of experiment 1 demonstrate that a correction of the mere-measurement effect can occur when respondents' persuasion knowledge is activated. However, 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 (Campbell and Kirmani 2000). 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

### Method

Two hundred and twenty-one students participated in experiment 2, which was identical to experiment 1 with the addition of one other 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 that they blinked their eyes as they read and responded to the intention question. This manipulation is one that has been shown to reduce cognitive capacity successfully, 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. Thus, for each of the two behaviors (flossing and eating fatty foods), the design utilized was a 4 (intention question form: control, intent-only, sponsored-objective source, and sponsored-self-interested source)  $\times$  2 (cognitive capacity: normal, constrained) between subjects.

### Results

Of the 221 participants who completed the first phase of the experiment, 174 were present for the second phase 1 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 expect 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 ( $t(173) = 6.89, p < .001$ ).

A two-way ANOVA on flossing behavior revealed a significant interaction between intention question type and cognitive capacity ( $F(3, 166) = 5.19, p < .01$ ). Examining flossing behavior for those respondents whose cognitive capacity was not constrained showed that the control group flossed their teeth 2.66 times in 1 week. If simply asked an intention question this rate rose to 5.27 times ( $F(1, 166) = 26.16, p < .0001$ ). If asked an intention question by an objective sponsor, this rate was also significantly greater than the control flossing rate (5.33 times;  $F(1, 166) = 21.29, p < .0001$ ). 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 (1.92 times;  $F(1, 166) = 1.45, p = .23$ , relative to the control group). Overall, this pattern of results for participants that did not receive a divided attention task largely replicates that observed in experiment 1.

Of more interest is the degree to which constraining cognitive capacity had an impact on the effect of asking an intention question on flossing behavior. Consistent with our expectations, constraining the respondent's cognitive capacity did not significantly change the number of times that respondents flossed in two of the three intention question conditions, as well as in the control conditions where they were not asked intent to floss questions: 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 nonsignificant for these forms of intention question and the control conditions; all  $p > .10$ ). For example, participants asked about intent to floss their teeth by an objective sponsor under no cognitive load flossed 5.33 times, while those asked intent by an objective sponsor under cognitive load flossed 5.08 times ( $F(1, 166) < 1$ ). 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. Table 2 reports means by condition.

By contrast, however, when participants were asked an intention question by a self-interested source, constraining cognitive capacity led to different flossing rates. Under no cognitive load, respondents flossed 1.92 times when a self-interested source asked the intention question but flossed 4.94 times if their cognitive capacity was constrained ( $F(1, 166) = 18.99, p < .0001$ ). Comparing participants in

TABLE 2

EXPERIMENT 2 RESULTS: RATE OF FLOSSING OR CONSUMING FATTY FOODS BY INTENT CONDITION AND COGNITIVE CAPACITY

|                                | Control | Intention question only | Intention question by objective sponsor | Intention question by self-interested sponsor |
|--------------------------------|---------|-------------------------|---|---|
| Flossing rate:                 |         |                         |   |   |
| Normal cognitive capacity      | 2.66    | 5.27 <sup>a</sup>       | 5.33 <sup>a</sup>                       | 1.92 <sup>b</sup>                             |
| Constrained cognitive capacity | 2.70    | 6.25 <sup>a</sup>       | 5.08 <sup>a</sup>                       | 4.94 <sup>a, b</sup>                          |
| Fatty food consumption:        |         |                         |   |   |
| Normal cognitive capacity      | 6.88    | 3.78 <sup>a</sup>       | 3.27 <sup>a</sup>                       | 7.13 <sup>b</sup>                             |
| Constrained cognitive capacity | 6.96    | 4.18 <sup>a</sup>       | 3.80 <sup>a</sup>                       | 4.06 <sup>a, b</sup>                          |

<sup>a</sup>Indicates means that are significantly different from the relevant control condition mean at the  $p < .01$  level.

<sup>b</sup>Indicates means that are significantly different from one another at the  $p < .01$  level.

the constrained cognitive capacity–self-interested sponsor conditions to the appropriate control groups shows that imposing a cognitive load also leads to the reemergence of a significant mere-measurement effect. When capacity is constrained, asking an intention question from a self-interested source leads to a significant increase in flossing behavior (4.94 times) versus a control group (2.70 times;  $F(1, 166) = 17.36, p < .0001$ ).

We next report the results for fatty food consumption. Results of a two-way ANOVA reveal a significant interaction between intention question type and cognitive capacity ( $F(3, 159) = 2.83, p < .05$ ). For those respondents whose cognitive capacity was not constrained, control group fatty food consumption was 6.88 times. This was reduced to 3.78 for those in the question-only condition ( $F(1, 159) = 8.91, p < .0001$ , relative to control group) and to 3.27 for those asked a question by an objective sponsor ( $F(1, 159) = 14.18, p < .0001$ , relative to control group). As in experiment 1, 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;  $F(1, 159) < 1$ ).

As with flossing, when participants were asked an intention question by a self-interested source, constraining cognitive capacity led to different rates of fatty food consumption. 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 4.06 times if they responded to the question under cognitive load ( $F(1, 159) = 9.01, p < .01$ ). Constraining capacity did not affect responses in any of the other intention question conditions ( $p > .10$  relative to comparison groups with full cognitive capacity), again showing that in those conditions, no conscious awareness was required for the effect of an intention question on behavior to be observed. Moreover, as with flossing, a comparison of participants in the constrained cognitive capacity–self-interested sponsor condition to the appropriate control group shows that the imposition of a cognitive load leads to the reemergence of the mere-measurement effect. When capacity is constrained, asking an intention question from a self-interested source leads to a significant decrease in fatty food consumption (4.06 times)

versus a control group not asked intent (6.96 times;  $F(1, 159) = 12.27, p < .001$ ).

**Summary.** The results of experiment 2 provide support for our argument that the degree to which respondents to an intention question can correct for its potential effect on subsequent behavior depends critically on the availability of cognitive resources. Respondents must have adequate cognitive resources to activate their persuasion knowledge in order both to recognize the potential persuasion attempt and to respond to it. If cognitive resources are not available, respondents will behave very much like respondents who had been asked a simple intention question.

A meaningful criticism of the first two experiments is that each could be vulnerable to potential response effects. The dependent variable is a self-report, and while previous research has observed similar mere-measurement results whether based on actual behavior (e.g., Morwitz et al. 1993) or on self-reports, there may be biases in such data that drive the current results. Similarly, our pretest measures of whether persuasion knowledge has been activated were, consistent with previous literature, direct measures of the perceived persuasiveness of the question and, as such, could be influenced by response bias. In experiment 3 we attempt to address these potential limitations.

### EXPERIMENT 3: THE MEDIATING ROLE OF PERSUASION KNOWLEDGE

#### Method

Experiment three utilized a 2 (cognitive capacity: constrained, normal)  $\times$  3 (intention question form: control, intent-only, or sponsored–self-interested source) between-subjects design. Our choice of the behavior to use for this study was driven by the need to have an immediate action occur naturally in a laboratory setting. Therefore, we focused only on the socially undesirable behavior, eating fatty foods. In addition, as we had found no differences between responses to the intent-only and the sponsored-objective-source conditions in experiments 1 and 2, we did not include the latter condition in this experiment. One hundred and

ninety-one participants first completed a brief paper-and-pencil booklet in which they received the capacity and question manipulations, took part in a 10-min. filler task, and then were instructed to proceed to a second room for a second experiment. Cognitive capacity was manipulated using the same blink counting distraction task as described in experiment 2. The form of the intention question was manipulated as follows: in the no-intention-question (control) condition, participants answered an intention question that was unrelated to the target behavior: "How likely are you to watch television in the next week?" In the intention-question condition, a simple intention question was asked: "How likely are you to eat fatty foods in the next week?" Finally, if assigned to the self-interested sponsor condition, participants were asked the fatty food question, with the sponsor identified below the question as the "Organic Food Growers of America."

In the second room, participants were asked to take part in a taste test. They were instructed to choose one of two foods, taking as many of their chosen alternative as they liked, and then to taste it as they would be asked to evaluate its taste subsequently. The participants were asked to choose between one of two "mini-sized" versions of snack foods: healthy rice cakes or much less healthy chocolate chip cookies. An experimental assistant surreptitiously recorded whether the participant had elected to consume rice cakes or cookies, as well as how many. Participants then moved back to the original room, and after logging on to a computer were instructed that they would participate in a series of short studies, one of which would ask them about the food they had just tasted.

On completion of the taste test, participants took part in a computer administered response task (Fazio 1990) that was used to measure indirectly the degree to which persuasion knowledge had been activated. Single words were presented on the computer screen, and participants were asked to indicate whether they thought the word was good or bad. They were asked to be as accurate and fast as possible in pressing the appropriate key. Participants were then told to place their left index finger on the 1 key and their right index finger on the 0 key. They were instructed to press 1 if the word they saw was a good or positive word and 0 if it was a bad or negative word to them.

Although participants responded to a list of 10 words, the actual indirect measure of whether persuasion knowledge was activated was how quickly they responded to items in the fourth, sixth, and ninth positions (suspicious, manipulate, and coerce). Items in positions 3, 5, 7, 8, and 10 were used to form a baseline response time for the individual to words unrelated to the activation of persuasion knowledge. Items in the first and second presentation position were not used as these served as practice responses for the participants, and response times to these early responses had high variance. Finally, participants responded to a series of demographic measures and a funnel-debriefing task to examine whether they had inferred the true goals of the study.

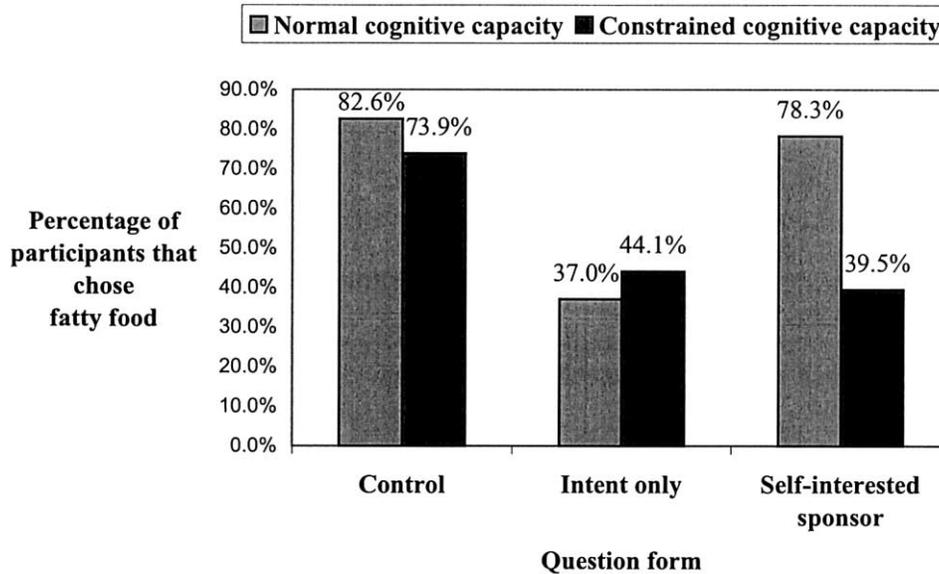
## Results

*Pretest Results.* Twenty-eight undergraduates participated in a pretest to assess the degree to which the cognitive capacity manipulation affects the ability to process information regarding the sponsor of an intention question. All participants received a single intention question identical to that used in the main study, sponsored by the Organic Food Growers of America. In addition, participants either completed a blink counting task (reduced cognitive capacity) or not (full cognitive capacity).

After responding to the intention question, participants turned the page where they saw the same intention question repeated, with a blank underneath in the place where the sponsor's name had previously appeared. They were asked to recall the name of the sponsor and to write it in the blank below the question. Answers were strictly coded, with participants receiving "correct" scores only if their answers were precisely right. Analysis shows no effect of capacity manipulation on recall of the sponsor ( $\chi^2(1, N = 28) < 1, p = .68$ ). In both capacity conditions, recall was relatively low, with 29% of those in the full capacity condition and 33% of those in the reduced capacity successful. After completing the recall question, participants turned the page and saw a list of potential sponsors (American Vegetable Growers Association, Organic Food Growers of America, American Medical Association, American Fruit Growers Association, Association of American Farmers, and Association of Organic Food Growers). The order of the items was randomized with the target name appearing in each location across participants. Respondents were asked to circle the correct name of the sponsor of the intention question. Analysis shows no effect of capacity manipulation on recognition ( $\chi^2(1, N = 28) < 1, p = .62$ ). Participants had much better recognition of the sponsor, with 86% of those in the full capacity condition successfully recognizing the correct sponsor, while 79% of those in the reduced capacity condition did so. Overall, these results show that there were no differences in the degree to which participants in the full versus divided capacity conditions processed the name of the self-interested sponsor.

*Main Study Results.* A 2 (cognitive capacity)  $\times$  3 (intention question form) logistic regression was performed with choice of the unhealthy snack (cookies) as the dependent variable. Results showed a significant main effect of question form ( $\chi^2(2, N = 191) = 16.48, p < .0001$ ) driven by a significant mere-measurement effect: when intention questions were asked, choice of the unhealthy snack was significantly lower (64 of 122, 52.5%) relative to the control condition (53 of 69, 76.8%;  $\chi^2(1, N = 191) = 16.34, p < .0001$ ). There was no main effect of cognitive capacity ( $\chi^2(1, N = 191) = 2.08, p = .15$ ). As anticipated, there was a significant two-way interaction between question form and cognitive capacity ( $\chi^2(1, N = 191) = 6.63, p < .05$ ), as illustrated in figure 1. This two-way interaction can be straightforwardly interpreted as follows: when no intention question was asked, there was no effect of cognitive capacity

**FIGURE 1**  
EXPERIMENT 3 RESULTS: FATTY FOOD CHOICE BY INTENT CONDITION



NOTE.—Both control conditions and the self-interested sponsor–normal capacity conditions are significantly different from both intent-only conditions and the self-interested sponsor–constrained capacity conditions at the  $p < .05$  level.

on the choice of the unhealthy snack (normal capacity, choice = 82.6%; constrained capacity, choice = 73.9%;  $\chi^2 = 0.64, p = .42$ ). Similarly, when a simple intention question was asked, there was no effect of cognitive capacity on the choice of the unhealthy snack (normal capacity, choice = 37.0%; constrained capacity, choice = 44.1%;  $\chi^2 = 0.31, p = .58$ ). By contrast, when asked intent by a self-interested sponsor, there was a significant difference between normal capacity (choice = 78.3%) and constrained capacity conditions (choice = 39.5%;  $\chi^2 = 7.98, p < .01$ ), consistent with the results of experiments 1 and 2.

A 2 (cognitive capacity)  $\times$  3 (intention question form) ANCOVA was performed with average response time to the three persuasion-knowledge related words as the dependent variable, and with average response time to the five baseline or control words as the covariate. Prior to this analysis, response latencies were cleaned to remove outliers following the procedure outlined in Bargh and Chartrand (2000). This yielded 167 usable latency responses that could be used in the ANCOVA. Results provide support that our indirect measure of whether persuasion knowledge had been activated was successful. The covariate, baseline response latency, was highly significantly related to persuasion knowledge response latency ( $F(1, 166) = 122.98, p < .001$ ). Analysis revealed a significant two-way interaction between question form and cognitive capacity ( $F(2, 166) = 8.12, p < .001$ ). Among participants whom we hypothesized would have persuasion knowledge activated by an intention question (i.e., those who received an intention question from a self-interested sponsor and had sufficient cognitive ca-

capacity to recognize the question as a persuasive appeal), significantly faster response times were observed to the persuasion knowledge-related words. All five pair-wise planned contrasts had  $p$ -values  $< .02$ ; a planned contrast comparing the response latency for the self-interested sponsor intention question–normal cognitive capacity condition to the other five conditions was highly significant ( $F(1, 166) = 17.85, p < .001$ ). Consistent with expectations, when participants who were asked intention questions by self-interested sponsors have normal cognitive capacity, they demonstrate significantly faster response to persuasion knowledge words ( $M = .94$  sec.) than the participants in other conditions (i.e., those in the intention only–full capacity condition;  $M = 1.12$  sec.). Similarly, they appear to be better able to correct for the potential influence of the question than their constrained capacity counterparts, as demonstrated by their greater resistance to choosing a healthy food option.

We tested for the mediating role of persuasion knowledge activation in the mere-measurement effect (Baron and Kenny 1986). This analysis was performed on the 61 participants who responded to an intention question by a self-interested sponsor, in both capacity conditions. First, cognitive capacity was significantly related to choice of an unhealthy snack ( $\chi^2(1, N = 61) = 7.98, p < .01$ ). Second, cognitive capacity was significantly related to persuasion knowledge activation ( $F(1, 60) = 11.47, p < .001$ ). Third, persuasion knowledge, the proposed mediator, was significantly related to the choice of an unhealthy snack ( $\chi^2(1, N = 61) = 12.42, p < .001$ ). Finally, in a logistic regression including both persuasion knowledge and capacity,

persuasion knowledge was significantly related to the choice of an unhealthy snack ( $\chi^2(1, N = 61) = 10.31, p < .001$ ), but cognitive capacity was not ( $\chi^2(1, N = 61) = 2.22, p = .14$ ).

**Summary.** The results of experiment three provide support for our argument that persuasion knowledge mediates the mere-measurement effect. Using an indirect measure of persuasion knowledge based on response latencies, we rule out the alternative explanation that our prior results can be partially explained by response biases. Experiment 3 also extends the findings from experiment 2 by replicating the effect on actual behavior rather than self-reports of behavior.

In experiment 4 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 4: STIMULATING A CHANGE OF MEANING

### Method

In this experiment, only a socially desirable behavior, volunteering for a charitable organization, was examined so that this experiment provides the complement for experiment 3. One hundred and eight 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 article (Fitzsimons and Morwitz 1996), while the control abstract was taken from a second, unrelated article prepared by one of this article’s authors. Both abstracts were matched as closely as possible for length (word count) and familiarity to an undergraduate student. 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. Participants then read a short overview of the organization’s mission and short quotations from several volunteers about their experiences. 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 on 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 con-

rol condition received the unrelated abstract but were not asked an intention question.

All participants were asked questions about their past volunteering behaviors (the frequency of volunteer activities, which specific organizations they had volunteered for, and the 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 e-mail or mailing address and would be contacted with additional information. Participants in the two conditions in which intentions were measured also completed the paper-and-pencil questions used in experiments 1 and 2 designed to measure the degree to which their persuasion knowledge had been activated.

### Results

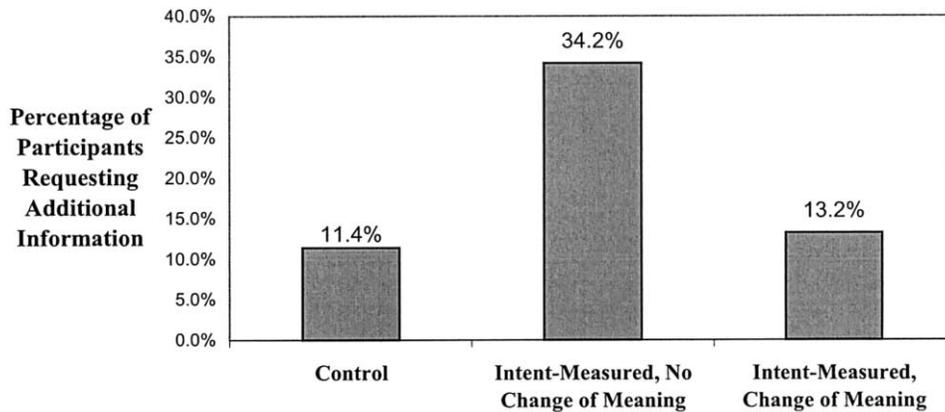
The manipulation check of the data showed a successful change of meaning—the ability to interpret an intention question as a persuasive tactic—among those who received an intention question ( $N = 73$ ). After reading an abstract describing the mere-measurement effect, the change of meaning condition participants believed that 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 ( $M = 1.77$ ;  $F(1, 71) = 287.46, p < .001$ ).

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 ( $\chi^2(2, N = 108) = 6.80, p < .01$ ; see fig. 2). Among those not asked intent, 11.4% (4 of 35) provided their e-mail or mailing addresses, asking for additional information about volunteering for Teach for America. In contrast, among those asked an intention question but not receiving change of meaning information about the mere-measurement effect, 34.2% (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 13.2% (5 of 38) of participants who were asked about intent to volunteer requested additional information about Teach for America. This proportion was significantly different from that among the group asked intent questions but not receiving change of meaning information ( $\chi^2(1, N = 108) = 4.28, p < .05$ ), but is not significantly different from the proportion of respondents in the control condition who requested additional information about volunteering for Teach for America ( $\chi^2(1, N = 108) < 1$ ). 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 per-

FIGURE 2

EXPERIMENT 4 RESULTS: REQUESTS FOR ADDITIONAL INFORMATION BY INTENT CONDITION



NOTE.—Both the control condition and the intent measured, change of meaning condition are significantly different from the intent measured, no change of meaning condition at the  $p < .05$  level.

suasive 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 criteria must be satisfied (Baron and Kenny 1986). First, 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, whether change of meaning information was provided or not was significantly related to perceived persuasive intent ( $F(1, 71) = 298.0, p < .001$ ). Third, 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, when both perceived persuasive intent and whether or not change of meaning information was provided were regressed against likelihood to request information, perceived persuasive intent remained significant ( $\chi^2(1, N = 73) = 8.17, p < .01$ ), but whether change of meaning information was provided was not ( $\chi^2(1, N = 73) = 2.10, p = .15$ ). 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.

*Summary.* 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. This study permitted us to demonstrate again the mediating role of perceived persuasive intent in attenuating the mere-measurement effect: only when respondents felt that 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 four studies presented here provide compelling support that mere-measurement effects occur because intention questions are perceived as benign queries, incapable of influencing behavior. In experiments 1 and 2, we show that when persuasive 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 that 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). In experiments 3 and 4 we document the mediating role of persuasion knowledge and, notably, demonstrate that our effects hold on actual behaviors and not just self-reported behaviors. Finally, in experiment 4, 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 suggest that the mere-measurement effect works when intention questions are able to slip below the level of our defenses, causing automatic or nonconscious changes in cognitive structure and leading to behavioral

changes of which the respondent is often not aware. Langer (1989) has argued that while individuals are capable of processing mindfully, or thoughtfully, they very often do not do so and are more likely to engage in mindless processing. Under such conditions, they may successfully pick up very subtle environmental or experimental cues that engage a sequence of behavior and proceed with that sequence in a mindless fashion. In fact, she argues, a mindless course of processing may be the most expedient strategy for moving through the environment, unless a situation is either relatively novel or mindlessness is deemed costly. Our results are consistent with this and suggest that most respondents process intention questions mindlessly, unaware of their potential influence on subsequent behavior. Only when persuasive intent is overtly subscribed to those questions does more mindful processing seem to be the norm among respondents.

Previous research has suggested that mere-measurement type effects will occur only when socially desirable behaviors are the focus of attention. The experiments reported here suggest that this 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 they may 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 article 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 on what they perceive the goal of the persuasion agent to be. Such behaviors offer a rich domain for future investigations.

While nearly every article on 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 on their answers to questions regarding their likelihood to engage in at-risk behaviors. This research suggests that very well-meaning organizations may have serious and negative impact on those they intend to help, inadvertently provoking more of these risky behaviors. It is only when respondents perceive intention questions to have manipulative intent and to have sufficient cognitive capacity to correct for this fact that the intention question does not result in unwanted changes in behavior. This may describe only a small portion of the situations in which respondents

find themselves when responding to intention questions from researchers. Finally, 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.

[Dawn Iacobucci served as editor for this article.]

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