Research Dialogue

Should we ask our children about sex, drugs, and rock & roll?
A different conclusion

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Abstract

Fitzsimons and Moore argue that survey questions for adolescents concerning risky behaviors can increase the degree to which participants will adopt those behaviors. This outcome is likely to be true for behaviors about which adolescents have negative explicit attitudes but positive implicit attitudes. Such a conclusion is extremely important for the health of research participants, for researchers, and for the field in general. I argue that it is premature to accept these conclusions, raising questions about process issues as well as conceptual, methodological, and empirical issues.

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Many of the empirical papers and theoretical chapters that we in the areas of experimental social psychology, judgment and decision-making, and consumer behavior write and read have little in the way of extremely important practical consequences. These papers may be about the psychological processes underlying some phenomenon, the choice of one theoretical account of some effect over a different account, or about how message framing impacts the extent to which one is likely to choose a particular consumer product.

The current target article (Fitzsimons & Moore, 2008) is clearly an exception. This is very serious stuff, and it is serious in several important ways. First, the authors argue that the simple act of asking, on a survey administered to adolescents, about engaging in risky behaviors actually increases the probability or the frequency with which these survey participants will engage in these behaviors in the future. Thus, typical surveys that psychologists administer may inadvertently increase the use of drugs, alcohol, unsafe sex, bad eating habits, etc. To be sure, this outcome is not true for any and all bad behaviors, but presumably only for those behaviors about which a person has some ambivalence—where the social norms are against the risky behavior, but implicit attitudes are positive toward this behavior. Of course, such ambivalence is likely to be the case for many behaviors, including drug use, eating unhealthy foods, risky sex, or engaging in any number of other risky or sensation seeking behaviors.

Second, if it is true that survey questions about risky behaviors actually increase the likelihood of these risky behaviors, then survey researchers who ask these kinds of questions are very much at risk for lawsuits once these effects become known and accepted. As a social psychologist, I have always wondered why our discipline has not been the target of major high-profile lawsuits. After all, we do change people’s attitudes, change their levels of stereotyping, make them more aggressive at times, and affect their consumer behavior. And, of course, we are not always up front to participants about the fact that the purpose of an experiment is to change attitudes, emotions, or behaviors. In this litigious society, I ask myself why there has not been a lawsuit brought by, say, a person who was in an attitude change study and emerged with different attitudes toward several social policy issues. As a plaintiff, this person might argue that the experimenter, with full knowledge of possible effects, and without announcing this possibility ahead of time, changed these attitudes. These changes caused the plaintiff to be in serious conflict with a spouse, friends, and co-workers. The plaintiff then lost the spouse to divorce, lost friends, and was isolated from co-workers. That sounds like a reasonable case to me—better than suing McDonald’s for making the coffee too hot or suing a cleaning establishment for losing a pair of pants. If the authors’ contention is correct, survey researchers and experimenters who ask questions about risk-seeking behaviors in the course of a laboratory or field experiment would, it seems...
to me, be open to lawsuits from people who subsequently smoked, used drugs, had an unwanted pregnancy, etc. This is serious.

Third, I would imagine that IRBs, which are notoriously conservative in their approach to research risks, would be hard-pressed to give approval to any questionnaire or instrument that had the real potential for increasing behaviors such as drug use or smoking. With this knowledge in hand, how can an IRB approve surveys such as the Monitoring the Future project (for an overview see Bachman et al., 2008), which has collected and disseminated incredibly useful data over a long period of time? Such projects have been important in identifying the prevalence of risky behaviors in populations and in helping to understand the antecedents and consequences of these behaviors. This, of course, better allows for the development of effective prevention and intervention programs. This concern would apply not only to studies of risky, health-relevant behaviors. The ideas apply to any behavior where there is ambivalence and an inconsistency between attitudes expressed on implicit and explicit measures. Take the area of stereotypes. We know that there are many people who, at the explicit level, express no degree of prejudice or stereotyping. Many of these people, even without awareness, also exhibit some degree of prejudice, often substantial, on implicit measures such as the IAT. Presumably, the current authors would suggest that asking these people questions about whether they do or whether they are likely to engage in discriminatory behaviors would substantially increase the frequency of these discriminatory behaviors. Would (should?) an IRB approve a project that asks such questions if they suspected such an outcome? Now, perhaps there are ways to prevent such outcomes, but it is not clear how difficult, how costly, or how effective such interventions (especially for a survey with many participants) would be.

In short, the implications of the conclusions drawn by Fitzsimons and Moore are extremely serious, for the health and well-being of research participants, for the experimenters who conduct such research, and for the discipline in general. Thus, it is essential that the conclusions suggested by Fitzsimons and Moore be on solid ground before they become part of our discipline’s knowledge base and before they are well-accepted as fact. With this in mind, and because, at the present time, I do not believe that such a solid foundation exists, I want to play devil’s advocate and suggest that, for both theoretical and empirical reasons, it is quite premature to claim that the conclusions suggested in the target chapter are strong or well-founded enough to accept or even to seriously entertain at the present time.

Process issues

Once again, the conclusion is that simply asking adolescents a question about a risky behavior may well inadvertently increase the frequency with which these adolescents will later engage in this risky behavior. Such an effect is likely to occur only when those who are surveyed recognize at the conscious, explicit level that the risky behavior is bad, but at the same time they are drawn to this behavior at a nonconscious or implicit level. Such an explicit/implicit ambivalence is relatively common for adolescents because it is an age of rebelliousness and independence-seeking. I have no problem with this assertion that explicit/implicit ambivalence regarding risky behaviors is relatively common in the teenage experience.

My confusion arises in trying to understand why simply asking ambivalent adolescents about some risky behavior should increase the likelihood and frequency of that behavior in the future. What is the process by which this important effect occurs? Here the authors are quite vague in their account. They do not claim, as some do, that asking about risky behaviors might encourage learning about these behaviors or might pique interest in the behaviors. Rather, presumably the effect is related to the likelihood that asking questions about risky behaviors activates the underlying implicit positive attitudes toward these behaviors.

Why does such activation of implicit positive attitudes lead to a subsequent increase in risky behaviors? Here again, the links are very unclear. The authors cite work by Dovidio and colleagues (Dovidio, Kawakami, & Gaertner, 2002; Dovidio, Kawakami, Johnson, & Howard, 1997) that shows there is a strong relationship between implicit measures and nonconscious behaviors, and that the impact of questions on behavior operates largely through automatic channels. Why this should be true in the case of risky behaviors is not apparent, especially given that the target behaviors (e.g., smoking a cigarette, using drugs) can hardly be characterized as nonconscious behaviors. The nonconscious behaviors that are typically predicted by implicit measures are behaviors such as nonverbal, clearly automatic behaviors, whereas explicit behaviors (e.g., perhaps agreeing to smoke a cigarette for the first time) are typically better predicted by explicit measures (Dovidio et al., 2002).

Let us for the moment even grant the authors’ assumption that asking a question about a risky behavior toward which there are ambivalent explicit/implicit attitudes will activate the implicit positive attitudes. The authors try to explain how this would then work to increase the likelihood of the risky behavior. They describe a teenager at a party with friends, and the authors state (as a fact) that highly deliberative, effortful self-regulatory behavior is not likely at such a party and that the more active implicit attitudes would guide the behavior. Although I disagree, let me also grant this assumption. I still do not understand why the activation of the positive implicit attitude sometime in the (distant?) past by a survey item plays any role as a triggering mechanism for the behavior at the party. How long did the activation last when answering the survey question? Did it lead to permanent change in the implicit attitude? Why? Implicit attitudes are typically not easy to change permanently. Weren’t there also literally hundreds of other prior situations that activated these positive implicit attitudes? For example, seeing another adolescent smoke, or reading an ad about smoking in a magazine, or seeing a movie star smoke on the screen should also activate the positive implicit attitude. Among this multitude of activations, why is the answer to a simple survey question so critical? More importantly, at the party wouldn’t the implicit positive attitude be strongly activated by the party events? Other adolescents will be smoking or
drinking. They are likely to ask the target person to have a drink or to smoke. Won’t these observations and activities be far more important to the activation of implicit attitudes and to the behavior than answering a survey question in the past? In addition, adolescents are likely to ask each other on many occasions whether they have ever smoked, had alcohol, used drugs, or had sex, and whether they intend to do so in the future. It is difficult for me to see how the effects of asking a question on a survey could be more important or could even be detected given all the more powerful, more frequent, and more contemporaneous activations of implicit positive attitudes toward risky behaviors.

My point is that the presumed process of implicit attitude activation by a survey question does not make much logical sense. But, of course, not all psychological effects are logical. Perhaps this is one of those “illogical” social psychology findings that are not easily predicted or initially understood. After all, cognitive dissonance theory has proven to be a powerful theory and a very good predictor of behavior even though it does not follow typical psycho-logic. However, if I am going to accept a process account for an important phenomenon that does not easily fit within my usual theoretical framework or conceptualization, then I need some solid direct evidence regarding this process. And here there seems to be a relative absence of such data.

Empirical issues

On the empirical side, there are actually only a small number of studies that have investigated the effects of answering survey items that found support for Fitzsimons and Moore’s contentions—especially real world studies that are longitudinal in nature. A study by Gould et al. (2005) reported that asking questions about suicide was not followed by an increase in suicide ideation over a three day period. Although this study did not look at actual behavior and was not longitudinal, the results are not congenial with the authors’ views, and I was not convinced by their attempts to dismiss the results (e.g., the study looked at suicide ideation, which Fitzsimons and Moore claim is an explicit measure, rather than real suicide behavior, which I suppose they would like to identify as an implicit measure (?)).

In our own longitudinal study of cigarette smoking (Chassin et al., 1984; Chassin, Presson, Sherman, & Kim, 2003), we asked the adolescents of an entire county survey questions about their smoking behavior. Many of the participants were, of course, nonsmokers at the time of the original survey. We have now followed up this sample for over 25 years. The smoking rates of our participants are no greater, and are in fact somewhat lower, than the smoking rates in comparable unsurveyed counties in the same state.

As empirical support, Fitzsimons and Moore cite several studies, all from their own laboratories. Williams, Block, and Fitzsimons (2006) asked undergraduates about future drug use and investigated whether this affected subsequent self-reports of actual drug use. Although the results were compatible with the authors’ hypothesis, the study can be criticized on the same grounds that Fitzsimons and Moore criticized the Gould et al. (2005) study—an explicit paper and pencil measure, and no actual measures of behavior. Another study (Fitzsimons, Nunes, & Williams, 2007) examined the impact of asking questions about skipping class on the accessibility of an implicit positive attitude toward this vice behavior. They recorded an increase in the positivity and accessibility of implicit attitudes. But this is, of course, only one step in the process. Did the activation last for any length of time? Similar findings are reported by Fitzsimons, Nunes, and Williams (2007), when they asked about drinking and about television watching behaviors and assessed reported behaviors one week later. Again, there was no assessment of actual behavior. In addition, one could ask whether it is the activation from asking a survey item that plays a causal role in any later decisions to engage in the vice behavior. Careful direct process data of this sort is needed before we accept such an important conclusion about the effects of answering survey questions on subsequent risky behaviors.

A couple of studies that constitute somewhat stronger support for the authors’ contentions (Fitzsimons et al., 2007; Williams et al., 2006) assessed actual behavior after a survey was administered. The behaviors involved distraction from studies and skipping classes. These experiments are certainly good beginnings. However, the kinds of behaviors utilized were not of a very high risk variety. In addition, no evidence of any initial explicit/implicit ambivalence was presented, and the direct role of the survey answers was not assessed.

In short, I would agree that there is some interesting preliminary evidence that is supportive of the authors’ ideas. However, this evidence seems to come exclusively from their own laboratories. In addition, there is at least some other data from other researchers that are not supportive of the idea that answering survey items about risky behaviors increases the likelihood of these risky behaviors.

Conceptual and methodological issues

In laying a foundation for their ideas and predictions, Fitzsimons and Moore start with the literature that investigates the effects of making self-predictions on people’s subsequent behavior. Work that I and others have done (Greenwald, Carnot, Beach, & Young, 1987; Sherman, 1980; Spangenberg & Greenwald, 1999) reliably shows that people are very poor at predicting their own future behavior. They very much over-predict (with respect to the actual behavior of people who are simply asked to do the target behavior) such behavior, especially when socially desirable. However, these mispredictions are self-erasing in the sense that the actual future behavior of the mispredictors is in line with their (wrong) predictions. Thus, these studies have found that, simply by asking people to predict what they would do in hypothetical future situations, one can increase the degree to which people are charitable, engage in health-promoting behaviors, or vote in an election. Rather than increasing the probability of negative behaviors such as drug use or smoking, these studies have actually increased the likelihood of desirable behaviors.

Although the results of these studies would seem to be contradictory to the contentions of Fitzsimons and Moore, these
authors point out that there is a difference in the target behaviors with which they are concerned and the target behaviors of these self-prediction studies. They assert that the behaviors employed by Sherman, Greenwald, Spangenberg, and others are behaviors toward which people have a straightforward and unambivalent positive or negative attitude (e.g., charity giving, exercise, voting, writing a counter-attitudinal essay). On the other hand, the risky behaviors investigated by Fitzsimons and Moore (drug use, smoking, unsafe sex) are claimed to involve conflicting explicit and implicit attitudes. I’m not so sure. If people have straightforward positive attitudes toward charity giving, voting, exercise, or eating healthy, why do so few people actually engage in these behaviors? Only four percent of the participants in Sherman’s (1980) study helped charity when asked directly. Many people explicitly endorse exercise or healthy eating, but they don’t do it. I would argue that these behaviors are also likely to involve an ambivalence between explicit and implicit attitudes. I am not aware of any studies that have examined the relation between explicit and implicit attitudes toward voting, exercise, or charitable behaviors, but I would wager that such ambivalence does exist. And if it does, then shouldn’t the results of these studies be in line with Fitzsimons and Moore’s predictions—a decrease in charity, less exercise, and less voting after survey questions?

More to the point, Spangenberg and his colleagues (Spangenberg & Greenwald, 1999; Spangenberg & Obermiller, 1996) have included behaviors in their self-prediction studies that I believe Fitzsimons and Moore would agree are of the ambivalent risky nature that they would include in their analysis. In one study, Spangenberg and Obermiller (1996) looked at self-predictions about cheating in a class. This is certainly a behavior similar to those that Fitzsimons and Moore discussed. In other work Spangenberg and Greenwald (1999) included health behaviors such as regular exercise and eating non-fat as opposed to tasty but high fat foods. Again, these are behaviors that are likely to involve implicit attitudes that are positive toward the unhealthy behavior but explicit attitudes that are negative. In all cases, Spangenberg and his colleagues found that participants over-predicted future healthy behaviors, and this over-prediction led to an increase in the subsequent frequency of these healthy behaviors. These results are exactly in opposition to Fitzsimons and Moore’s predictions about survey questions increasing the likelihood of health risk behaviors.

There are other aspects of these self-prediction studies that raise questions for Fitzsimons and Moore’s ideas. In the first place, these studies all involve predictions of future behavior. Although some surveys ask about behavioral intentions for the future, most of the surveys cited in the article by Fitzsimons and Moore ask about present behaviors in order to assess the prevalence of these behaviors. Recognizing this difference, the authors argue that past-looking, present-looking, and future-looking questions “have similar effects in activating implicit attitudes among respondents.” Now, in some of my work, especially in a chapter with Marcia Johnson (Johnson & Sherman, 1990), I have pointed out similarities in how people think about the past and think about the future. However, I would be the first to acknowledge that there are also very important differences and that it would be inappropriate to conclude things about the effects of asking about current behaviors from research that investigated the effects of asking about future behaviors. In fact, we know from work on temporal construal (Liberman & Trope, 1998; Trope, Liberman, & Wakslak, 2007) that even thinking about the near future versus the distant future makes very important differences for judgments and behaviors. Interestingly, although Fitzsimons and Moore want to talk about the possible effects of surveys that ask about present behaviors in order to assess prevalence, in their own studies, they almost exclusively ask forward-looking questions.

Another aspect of the self-prediction studies that seems critical to the current account is that the results of these studies depended greatly on how people actually answered the prediction questions. That is, respondents over-predicted socially desirable behaviors such as being charitable or voting. This was essential to the effects of asking the prediction questions. I would thus assume that the answers that participants give in survey responses should be very important. For example, in our smoking surveys, the number of nonsmokers who predicted that they would smoke or who intended to smoke in the future was far below the actual percentage of nonsmokers who went on to become smokers. The fact that our participants eventually reached smoking levels that were somewhat below average is consistent with the predictions of self-prediction studies. According to this work, the mispredictions should reduce subsequent smoking behavior. Thus, in this survey of risky behaviors participants again over-predicted the socially desirable behavior of not smoking and this seemed to be reflected in subsequent behaviors.

Would Fitzsimons and Moore predict the same kinds of effects? I think that the answer is “no,” because their idea is that it is the activation of implicit attitudes that matters rather than what participants might predict explicitly. Still, one would like to know what participants in their studies actually answered on the surveys. I would suspect that their respondents, like those in the earlier self-prediction studies, answered in the socially desirable direction. That is, they would say that they would not skip classes or be distracted or drink too much or use drugs. If so, why do we not observe the self-erasing error effect?—especially if it is the case that behaviors such as giving to charity or eating healthy have negative implicit attitudes? Recall that Spangenberg and Greenwald (1999) and Spangenberg and Obermiller (1996) reported that participants over-predicted the extent to which they would not cheat or would choose non-fat foods over fatty but tasty foods; and subsequently, these participants showed increases in the socially desirable behaviors.

So, what did participants in studies by Fitzsimons and colleagues predict? I was unable to find mention in the article about how participants answered the survey items or whether there was any relation between their answers and their subsequent behaviors. I also perused the most relevant studies from the authors’ laboratories. These were the experiments reported by Fitzsimons et al. (2007). I found data reporting participants’
actual predictions in only one of these experiments. In Study 1, participants predicted the number of classes they would miss in a particular course during the upcoming semester. The average prediction was 2.98 classes. This was almost exactly the number of classes actually missed by participants who made no prediction (2.95). The prediction participants went on to miss an average of 3.78 classes, significantly more than non-prediction participants. It is not clear why survey participants in this study did not over-predict the socially desirable behavior. The many self-prediction studies already reviewed consistently show such over-prediction. The other studies reported in Fitzsimons et al. (2007), dealing with skipped classes, distractions from studies, drinking, and watching television instead of studying, failed to report the predictions made by participants. Given the importance of such predictions in previous self-prediction studies, it would seem very important to assess the role of these self-predictions in subsequent behavior.

There may, however, be reason to think that the participants in the studies by Fitzsimons and his colleagues may not have over-predicted socially desirable behaviors, and herein may lie a concern. Why might they not show the usual over-prediction of socially desirable behaviors? The answer lies in the question that their participants were asked. Rather than simply being asked if they would use drugs or not, would skip classes or not, or would be distracted from studying or not, participants were asked about their predicted frequency of such acts (0, 1, 2, etc.). This can create problems. We know that respondents to surveys avoid the extreme responses in their answers. Thus, if you ask how many headaches they have had in the last two months, it matters how you form the response categories. If the possible answers are 0, 1, 2, 3, 4, 5 — 10, 11 or more, you will see fewer headaches acknowledged than if you ask 0–3, 4–6, 5–8, 9–11, greater than 11. Thus, their respondents may well have endorsed responses that were in fact more frequent than most comparable people actually do. And perhaps it is this over-prediction of bad behaviors that is responsible for some of the results. We certainly know that how a question is asked can very much affect current and possibly future responses. For example, by simply asking whether people did things “frequently” or “on occasion,” participants’ attitudes about the response domain change greatly (Salancik & Conway, 1975).

In any case, it would certainly be very important to analyze the content of participants’ responses and the role that these responses play in any subsequent effect on judgments and behaviors. Of course, one would also want to assess the effect of over-predicting some behavior on the accessibility (or change) of implicit attitudes and the role of this in subsequent behavioral and judgment change. Typical measures of ambivalence (both felt ambivalence and potential ambivalence; Conner & Sparks, 2002; Thompson, Zanna, & Griffin, 1995) would also be important to include in any predictive model because Fitzsimons and Moore assume that such ambivalence plays a central role in the effects. Finally, if their account is correct, the consequences of answering a survey item for a risky behavior where explicit/implicit ambivalence exists should emerge on implicit measures. This is not a prediction that would be made by those who have investigated self-prediction effects.

Prevention and intervention strategies

Because of the great importance of the conclusions that Fitzsimons and Moore draw, I raised some serious concerns about process issues, empirical issues, and conceptual and methodological issues that might call into question the strength of their conclusions. And because of the serious implications of their conclusions, if true, I suppose that I was a bit hard on them in raising these concerns. But there was also another part of their article that deals with suggestions for prevention and intervention measures for addressing health risk behavior. It is always nice to end a commentary on a positive note, and I must say that I fully endorse and support their ideas about prevention and intervention.

First, they suggest that a key component of successful interventions is that they be targeted toward specific populations. This is a very important observation. The fact is that different people engage in health risk behaviors for very different reasons. This might depend on gender, race, ethnicity, personality differences, genetic factors, or social environment factors. For example, some people smoke for the purpose of relaxation, some for the reduction of stress, others for positive sensory experiences, and others because they are addicted to nicotine. The same intervention simply will not work for these different groups of smokers.

Recently, we have identified three different types of young women smokers (working women who are daily smokers; light smoking college students; and heavy smokers who have children; Rose et al., 2007). We suggest that very different interventions be used to try to successfully achieve smoking cessation for these different groups. We have also identified several very different smoking trajectories (e.g., early onset smokers who reach a high level of smoking and persist; late onset smokers who stabilize at rather low levels of smoking—Chassin, Presson, Pitts, & Sherman, 2000; Chassin, Presson, Sherman, & Edwards, 1991). Again, successful interventions for quitting are going to have to be different for these different trajectory groups.

Finally, Fitzsimons and Moore suggest that parents can play an important role in successful interventions to reduce health risk behaviors among adolescents. I couldn’t agree more. Our work has found important implications of parental socialization practices, both general and specific to smoking, in both the initiation and the cessation of their adolescent children (Chassin et al., 2005; Chassin et al., 2002; Chassin et al., 1998). In other work, we find that parental factors may in fact be more important than peer factors in the smoking history of adolescents (Chassin et al., 1986). Current ideas and interventions very much underestimate the importance of parents in the process, and Fitzsimons and Moore are exactly right in pointing this out.

References


