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How to Study Choice-Induced Attitude Change: Strategies for Fixing the Free-Choice Paradigm

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Abstract

The theory of cognitive dissonance has been among the most influential theories in social psychology for the last 50 years. Support for the theory has come primarily from three experimental paradigms: free-choice, induced compliance, and effort justification. Recently, Chen and Risen (2010, *Journal of Personality and Social Psychology*, **99**, 573–594) have argued that although the free-choice paradigm reliably finds a 'spreading of alternatives' (i.e., after making a choice, participants' evaluations of the chosen item improve and evaluations of the rejected item decline), these results cannot be interpreted as evidence for dissonance reduction or attitude change. Unlike the other dissonance paradigms, participants 'self-select' how they are treated in the free-choice paradigm, making it impossible to know whether spreading is because of the choice process or the information that is revealed about participants' existing preferences. The current paper has two goals. First, we will describe the criticism developed by Chen and Risen (2010) and situate the criticism within the broader study of dissonance. Second, we will offer four suggestions for how researchers can isolate the effect of the choice process and properly test for choice-induced attitude change in the free-choice paradigm.

In the mid-1950s – during the heyday of consistency theories – Leon Festinger developed the theory of cognitive dissonance. Since its formalization in 1957, dissonance theory has become one of the most influential theories and most researched topics in social psychology, tested primarily within three paradigms: free-choice, induced compliance, and effort justification. Recent research suggests, however, that the very first paradigm used to test dissonance – the free-choice paradigm (Brehm, 1956) – is fundamentally flawed. Chen and Risen (2010) have argued that although the spreading of alternatives has been found reliably in hundreds of free-choice experiments, it arises as an artifact of a flawed methodology and cannot be taken as evidence of dissonance reduction or attitude change.

The current paper is written with two primary goals. First, we will describe the criticism developed by Chen and Risen (2010), situating it within the broader study of dissonance. We will explain how 'self-selection' occurs in the free-choice paradigm (FCP) and why the nonrandom treatment of participants is so problematic. Second, because the criticism is meant to improve the study of dissonance, we will offer four suggestions for properly studying choice-induced attitude change in the FCP. Researchers can isolate the effect of the choice *process* on subsequent preferences by (i) ensuring that all participants make the same choice, (ii) controlling for the information revealed by choice, (iii) removing the information revealed by choice, or (iv) manipulating the choices that people make.

Dissonance Theory

According to Festinger (1957, 1964), when two or more cognitions are inconsistent with one another, an uncomfortable state of 'dissonance' is produced. People are motivated to resolve this state by changing their cognitions so that they become consistent. Because knowledge about one's recent behavior is especially resistant to change, the study of dissonance reduction has typically focused on attitudes that shift in the direction of recent behavior. In other words, when an individual acts in a manner inconsistent with a previously held attitude, dissonance researchers predict that in the absence of a good reason for that behavior, the initial attitude will shift to become more consistent with the behavior.

Dissonance theory received early support from the results of three paradigms. Using the free-choice paradigm, Brehm (1956) found that after making a difficult choice between two items, participants' rating of their chosen alternative tended to rise and the rating of their rejected alternative tended to fall (the classic 'spreading of alternatives'). Using an induced compliance paradigm, Festinger and Carlsmith (1959) asked participants who had just finished a boring task to tell another student that the task was interesting. The researchers found that participants who were paid $20 to lie (high justification) did not change their attitudes because they could easily explain the inconsistency between their action and their attitude. However, participants who were paid only $1 to lie (low justification) expressed a more positive attitude toward the boring task. Along similar lines, Linder, Cooper, and Jones (1967) found that when participants were made to feel that they had freely chosen to write a counter-attitudinal essay in support of a campus speaker-ban, they came to agree with the advocated position more than those who were forced to write the essay. Finally, using an effort justification paradigm, Aronson and Mills (1959) and Gerard and Mathewson (1966) found that participants who had to go through a severe initiation to join (what turned out to be) a pretty dull group liked the group more than those who went through only a mild initiation.

According to dissonance theory, in each of these cases, attitude change occurred because people were motivated to undo the unpleasant state that was caused by the inconsistency between their attitude and their recent behavior. Thus, to reduce the experience of dissonance, participants came to like the chosen object more, formed a more positive attitude toward a boring task, and saw a dull group as more interesting.[1,2]

The Importance of 'Perceived' Choice

Early dissonance papers have become classics in the field, in part, because of the craftsmanship involved in each study. Researchers needed participants to believe that the task that they were randomly assigned to do was one that they were choosing to engage in 'freely'.

Let us pause to consider how the interpretation of the results would change if, in the effort justification paradigm, the experimenter had been unable to induce all participants to undergo the initiation. Imagine that when the initiation was mild, all participants agreed to participate, but that when the initiation was highly embarrassing or painful, only half of the participants agreed to participate. In this case, participants would be revealing their underlying preference for joining the group; it stands to reason that those who were willing to undergo the severe initiation were more interested in joining the group than those who were not. Thus, the severe initiation condition would only include people who were especially interested in joining the group, while the mild condition would include all participants – both those who were especially interested in joining the

group and those who were not. It would hardly be surprising, then, if those in the severe initiation condition liked the group more than other participants. In other words, if this sort of self-selection had played a role in the effort justification paradigm, then it would have been impossible to determine whether differences between conditions were because of dissonance reduction or whether they simply reflected differences in preexisting attitudes.

The same logic applies to the induced compliance paradigm. Imagine that all participants wrote the essay when it was required, but that only half of participants agreed to write the essay when it was described as optional. If participants had a real choice, then the experimental condition would only include participants who were sufficiently amenable to the topic to agree to write the essay. Again, if this sort of self-selection was to occur and all of the 'dissenters' who refused to write the essay were excluded from the experimental group, then we would have little reason to suppose that the difference between the conditions was the result of attitude change.

These thought experiments should make it clear why it is essential that all participants in the effort justification and induced compliance paradigms make the same choice. The ability of researchers to convince participants that they had a choice in writing the essay or undergoing the initiation – when they did not actually have one – is not only experimentally elegant, it is critical for the dissonance claim.[3] We suggest that unlike the induced compliance and effort justification paradigms, researchers who have used the FCP cannot make a case for dissonance reduction because participants in these studies *are* allowed to freely choose one item over another. And because people who choose item A over item B presumably have a stronger preexisting preference for A than people who make the opposite choice, researchers cannot determine whether the effects that they find are due to the process of making a choice or to the information revealed by the choice.

Free-choice paradigm

The FCP, originally developed by Brehm (1956), was designed to examine attitude change following choice. According to dissonance theory, dissonance is produced when an individual chooses one alternative over a close other because any negative thought about the chosen alternative or positive thought about the rejected alternative will be inconsistent with the decision (Brehm, 1956; Festinger, 1957, 1964). To reduce the inconsistency and the uncomfortable state of dissonance, the chooser can shift his or her preferences to be more consistent with the choice. Put simply, the goal of the FCP is to test whether the act of choosing affects subsequent preferences for the objects in the choice set.[4]

In the typical FCP experiment, participants are asked to rate or rank a set of goods. Next, they are asked to choose between two of the items in the original set. These two items are typically chosen so that they are close on a rating scale (e.g., 4.0 and 4.4) or close in ranking (e.g., rank #7 and #9). Finally, participants are asked to re-rate or re-rank the original set. *Chosen spread* is calculated by adding the amount the chosen item improves to the amount that the rejected item declines.[5]

Imagine that Jack and Liz rank 15 art prints and both rank the Monet print as #7 and the Van Gogh print as #9. They are then presented with a choice between those two prints. Following the choice, they re-rank the original set of 15. If Jack chooses the Monet, his *chosen spread* is calculated based on how much the Monet improves and how much the Van Gogh declines. If Liz chooses the Van Gogh, her *chosen spread* is calculated in the opposite way, based on how much the Van Gogh improves and the Monet

declines. *Chosen spread* is averaged across all participants, and if it is positive, researchers conclude that choice-induced attitude change is present. Because researchers are interested in how making a choice influences subsequent preferences, it can seem quite sensible to calculate the change score based on each participant's choice (so sensible, in fact, that more than 50 years passed before anyone noticed that there was a problem with calculating spread this way). Because participants are not randomly assigned to make their choice, however, this procedure results in participants 'self-selecting' how *chosen spread* is calculated.

When the FCP uses a control group (instead of simply comparing *chosen spread* to 0), control group participants rank twice, but they do not choose between any items in the set. Researchers cannot compare *chosen spread* across conditions because the choices of those in the control group are never learned. Thus, when comparing an experimental condition to a control group, researchers calculate *high-low spread* instead. *High-low spread* is calculated by adding the amount that the item initially ranked higher improves to the amount that the item initially ranked lower declines and it is compared across conditions. Note, however, that experimental participants who choose the item that they had ranked lower are excluded from analysis (on average, 25% of participants show this 'choice reversal').[6] In our example, then, the experimental condition would be made up only of people like Jack (because people like Liz would be excluded). Because participants in the control condition never make a choice, everyone is included in the analysis – even those who would have acted like Liz. Here too, then, participants in the experimental condition are 'self-selecting' how they will be treated.

Over the past 50 years, FCP studies have reliably found that after being asked to make a choice, people show a 'spreading of alternatives' (see, for example, Brehm, 1956; Festinger, 1964; Gerard & White, 1983; Lieberman et al., 2001). In other words, *chosen spread* is found to be positive. Or, if the experiment employed a control group, then *high-low spread* for experimental participants who chose the item initially ranked higher is found to be larger than *high-low spread* for participants in the control condition.

Although the spreading of alternatives has been found reliably in FCP experiments, it cannot be taken as evidence of dissonance reduction because participants are treated differently based on the choice they make in the study. The computation of *chosen spread* relies explicitly on participants' choices, and the calculation of *high-low spread* also relies on each participant's choice because it is used as a criterion for including or excluding participants. This nonrandom treatment of participants is extremely problematic. If participants choose different items because they have different underlying preferences for the two items, then it is unclear whether the documented effects in the FCP are the result of attitude change following choice (as it has been argued by dissonance researchers) or whether they are, at least in part, a reflection of existing preferences that are revealed by choice.

The Problem of Self-selection for 'Chosen Spread'

Remember that both Jack and Liz ranked the Monet print #7 and the Van Gogh print #9. When given the choice between them, Jack chose the Monet, but Liz chose the Van Gogh. If you had to guess, who prefers the Monet more?

It would be quite reasonable to assume that Jack prefers the Monet. And, it would also be reasonable to assume that Jack would re-rank the Monet higher than Liz would. Note that this assumption can be made without reference to dissonance theory. The information revealed by their choices suggests that Jack truly prefers the Monet to the

Van Gogh (he ranked it higher and chose it), while our information about Liz is more equivocal. Thus, we should not be at all surprised if, on the second ranking, Jack spreads his alternatives by moving the Monet up to #6 and Liz spreads her alternatives by dropping the Monet down to #9.

From the perspective of dissonance theory, this would occur because Jack and Liz are rationalizing their choices. But we hope that readers see that this is also precisely what we would predict based on the information that has been revealed by their different choices. Thus, it is unclear whether *chosen spread* is due to the choice process or to the fact that Jack and Liz had different preferences all along.

With a formal mathematical proof, Chen and Risen (2010) demonstrate that the FCP will measure positive *chosen spread* even if people have perfectly stable preferences. Specifically, the theorem predicts spreading based on three simple assumptions. First, people's ratings/rankings are at least partially guided by their preferences (i.e., we can learn something about participants' preferences when they rank goods). Second, people's choices are at least partially guided by their preferences (i.e., we can also learn something about participants' preferences when we observe their choices). Note that this does not imply that individuals must always prefer their chosen item. Rather, it assumes that an individual's choice provides enough information about her preferences to predict that she is more likely to prefer the chosen item than the nonchosen item (i.e., people are not choosing completely at random).[7] Finally, people's ratings/rankings are often not a perfect measure of their preferences. In other words, when we observe Liz ranks the Monet higher than the Van Gogh, we think it is more likely than not that she prefers the Monet to the Van Gogh, but we do not know this with certainty. All three assumptions were supported by the data (Chen & Risen, 2010). If positive *chosen spread* is predicted when participants' preferences are stable, then it is clear why positive *chosen spread* cannot be taken as a measure of choice-induced attitude change.

Note that Chen and Risen's (2010) criticism is equally applicable to studies that use fMRI technology to measure the spreading of neural activation after a choice (see, for example, Sharot, De Martino, & Dolan, 2009). This is because Assumptions 1 and 3 also apply to neural activation. If neural activation is related to people's preferences (Assumption 1), but is not a perfect measure of participants' preferences (Assumption 3), then the FCP will also find spread for neural activation (even if participants' preferences are perfectly stable). Thus, the spreading of neural activation can also not be taken as evidence of choice-induced attitude change.

The Problem of Self-selection for 'High-Low Spread'

Imagine that Jenna and Tracy are in the control condition and both initially rank the Monet and the Van Gogh as #7 and #9, but they are never given a choice between the two prints. Because they do not make a choice, we do not learn anything additional about their underlying preferences for the prints. Thus, while it was reasonable to assume that Jack would move the Monet up when he re-ranked and that Liz would move the Monet down, we have no meaningful information from which to predict the re-ranking of Jenna and Tracy. Thus, we should not be surprised if *high-low spread* for people like Jack is larger than *high-low* spread for people in the control condition.

This is analogous to our thought experiment for the induced compliance and effort justification paradigms. If the experimental condition only includes people who really prefer Monet to Van Gogh and the control condition includes people who may prefer Monet or may prefer Van Gogh, then we should not be surprised if the Monet is ranked

higher in the experimental group than the control group. There would be a difference between conditions, but it would not provide evidence of attitude change or dissonance reduction.

The Problem of Self-selection When Measuring Subsequent Choices

The traditional FCP has been modified in recent years to examine the effect of choice on subsequent choice. Egan, Santos, and Bloom (2007) developed this modification so that they could test whether children and monkeys display dissonance reduction following choice. The paradigm used in their 2007 paper, however, suffers from the same fundamental flaw as the traditional paradigm – namely, when participants make a choice, they self-select how they are treated in the experiment. Because choices are informative (rather than random), this makes the results impossible to interpret.

In this modified FCP methodology, participants rate several items and the experimenter chooses three that the participant has rated equally (A, B, and C). The participant is then given a choice between two of the items (A and B). Next, participants are given the choice between the rejected item (B) and the third item (C). According to dissonance theory, after choosing A over B, participants will reduce dissonance by liking A more and B less, which would lead them to choose B less often than C in their subsequent choice. But because the initial choice of A over B provides information about participants' existing preferences, we should expect participants to choose B less often than C even if there is no dissonance reduction or attitude change. In other words, once we include the information revealed by the initial choice, 50% is no longer the appropriate null. Let us consider why.

If participants' preferences for the three items are not exactly identical to start, then there are six possible orders of liking: A, then B, then C (ABC); ACB; BAC; BCA; CAB; and CBA. Three of the possible orders are contradicted by the initial choice (BAC, BCA, and CBA). Of the three remaining orders, two have C ranked above B and only one has B ranked above C. Thus, once we include the information revealed by the initial choice, we should expect that closer to one-third of participants will choose B and closer to two-thirds will choose C. Thus, even though fewer than 50% of children and monkeys chose B over C, this cannot be interpreted as evidence for dissonance reduction.[8]

The Problem for Studying Moderators and Mediators in the FCP

The problem identified by Chen and Risen (2010) also applies to the examination of moderators and mediators of choice-induced dissonance. If an experimental manipulation reduces spreading, for example, one cannot tell whether it is because the manipulation has influenced the dissonance process or whether it has affected the information that is revealed by participants' choices. Thus, we believe that studies that have examined moderators of spreading, such as the effect of making a decision between 'close' or 'far' alternatives (Brehm, 1956; Brehm & Cohen, 1959; Shultz & Lepper, 1992), the effect of culture (Heine & Lehman, 1997; Hoshino-Browne et al., 2005; Kitayama, Snibbe, Markus, & Suzuki, 2004), and the effect of self-affirmation (Steele & Liu, 1983; Steele, Spencer, & Lynch, 1993), are also undermined by the problem of 'self-selection' (see Chen & Risen, 2010 for an in-depth discussion of moderators in the FCP).

To be clear, then, the criticism developed by Chen and Risen (2010) applies to all forms of the FCP – i.e., those that measure a spreading of ratings, rankings, or neural

activation, those that measure subsequent choice, and those that examine moderators or mediators of these effects. The criticism does *not* apply to the induced compliance and effort justification paradigms (as long as all participants make the same choice). If researchers want to demonstrate that the process of making a choice influences preferences, they must isolate the process of choosing from the information revealed by choice. We will now discuss four methods that researchers can use to properly test for choice-induced attitude change in the FCP.

Ensure that everyone makes the same choice

The most straightforward way to fix the FCP is to ensure that all participants make the same choice. This is the approach that has protected the induced compliance and effort justification paradigms from the Chen and Risen (2010) criticism. If all participants make the same choice, then there is no information revealed about participants' preferences. Thus, if all participants in a FCP study choose the item initially ranked #7 over the item ranked #9, and spread is calculated identically for all participants, then one could reasonably infer that spreading is evidence of attitude change. To successfully use this approach, researchers need to solve two central problems. First, researchers must effectively get all participants to make the same choice.[9] Second, in doing so, researchers must avoid directly manipulating preferences.

One method would be to try the same subtle persuasion strategies that researchers have used in the induced compliance paradigm. For example, participants could rank 15 art prints and then be told 'We would like to give you an art print for participating today. Based on your ranking, we have selected this print for you (show them the print they ranked #7). We do have one other print available, though (show them #9). I want to make it clear that the decision is entirely yours. Please sign here to state that you have freely chosen this print (pointing to #7)'.

If all participants make the same choice, then researchers can be sure that spreading reflects attitude change. To be sure that attitude change is because of the choice process, however, researchers also need to make sure that the subtle persuasion techniques do not directly affect preferences. In other words, if telling participants that the experimenter has chosen a particular print for them makes them more positively disposed to that print, then attitude change found in the paradigm may not result from the choice process. Thus, even if all participants choose #7 over #9, it is still important to include a non-choice control condition, such that the only difference between conditions is that experimental participants believe they have made a free-choice.

Controlling for the information revealed by choice

Instead of getting all participants to make the same choice, Chen and Risen (2010) control for the information revealed by choice by finding out what participants in the control condition *would have* chosen. In other words, at the end of the typical FCP, participants in the control condition are asked to make the same choice that participants in the experimental condition made earlier. With this small modification, the information from choice is equalized across conditions. For example, Jack and Liz would rank, choose, and then rank again (RCR). Jenna and Tracy would rank, rank again, and then choose (RRC).

According to dissonance theory, *chosen spread* occurs because people are motivated to reduce dissonance that occurs after they make a difficult choice. Thus, dissonance theory

only predicts spreading for participants who make the choice before the re-ranking. If spreading occurs because information is revealed by choice, however (as suggested by Chen & Risen's mathematical proof), then, on average, there should be spreading for all participants who make a choice, regardless of when the choice is made.

In Chen and Risen (2010), all participants ranked 15 art prints, chose between two art prints in the set, and re-ranked the prints. The authors manipulated when the critical choice was made. Participants randomly assigned to the experimental condition (Rank-Choose-Rank; RCR) chose between the items initially ranked #7 and #9 *before* they re-ranked the prints. Participants assigned to the control condition (Rank-Rank-Choose; RRC) chose between the items initially ranked #7 and #9 *after* they re-ranked the prints. Thus, all participants revealed information about their preferences for items #7 and #9 by making a choice. The only difference was when the choice was made – RCR participants made the critical choice before re-ranking, and RRC participants made the critical choice after re-ranking.

In two studies, the authors found positive *chosen spread* for participants in both conditions. The spreading found for RRC participants demonstrates that spreading can occur in the absence of dissonance reduction, which provides empirical support for the authors' mathematical proof, and makes it clear why spreading that has been found in past FCP studies cannot be interpreted as evidence for attitude change or dissonance reduction.

If attitudes change because of the choice process as well (as dissonance researchers contend), then there should be significantly more spreading for RCR participants than RRC participants. In their first study, Chen and Risen (2010) found no difference in spreading for RCR and RRC participants. In their second study, they found marginal evidence for more RCR spread than RRC spread. Although these particular studies did not provide evidence for dissonance reduction following choice, the RCR versus RRC method can provide researchers with a tool for investigating whether choice-induced attitude change exists, and if so, what factors moderate and mediate the effect.

Note that the RRC condition only provides the appropriate control for RCR participants if the choices made by participants in the two conditions reveal the same information. Thus, researchers need to be sure that (i) participants make similar choices in the two conditions and (ii) the first and second rankings predict participant's choices to the same degree across conditions. Thus, we do not mean to suggest that the RRC control is fail-safe. The RRC condition *can* control for the information revealed by choice, but researchers need to be sure that it actually does. If there is true 'time-invariance' for the information revealed by choice, then researchers can compare spreading across conditions and make inferences about the difference.

To use RRC as a control, researchers do not need to double the number of conditions or the number of participants in a sample. Researchers can also use a within-subject RCRC design. For example, Risen and Chen (2010) had participants rank 16 art prints, choose between two prints in the set (e.g., #7 and #9), re-rank the 16 prints, and choose between another two prints in the set (e.g., #8 and #10). The choices were counterbalanced. With this method, researchers can calculate the RRC and RCR spread for each participant, allowing for a paired comparison.

Removing the information from choice

A third way to isolate the choice process from the information revealed by choice is to make sure that the choice does *not* reveal information about existing preferences. This can be performed, for example, by making the choice 'blind'. If participants choose

between #7 and #9 when the prints are behind a screen, then there is no information revealed by the choice.

Reacting to the concern developed by Chen and Risen (2010), Egan, Bloom, and Santos (2010) adapted their 2007 FCP paradigm so that choices were made 'blind'. For example, in their first experiment, children were shown three toys that differed only in color. The experimenter hid the toys in two socks, so the child saw two lumps in one sock (lumps A and B) and one lump in her other sock (lump C). In the experimental condition, the child was asked to pick (without peeking) either lump A or B and was given that toy. Then they were asked to pick again (without peeking) between the sock with the lump they did not choose the first time (lump B) and the sock with the other lump (C). The authors predicted that children would choose lump B less than 50% of the time and that was what they found.[10]

Earlier, we explained why 50% was the incorrect null for their 2007 paper. Because the choices were blind in the 2010 paper, though, 50% was the correct null. One concern we have with the new methodology is that the children's second choice was also blind – i.e., the second choice was also made with the objects stuffed in socks. It is hard to interpret blind choices as evidence for attitude change. If a child does not pick lump B, should we conclude that they have devalued the object underneath? One way to improve the methodology would be to make the first choice blind, but the second choice open. If children choose object B less often than object C when they are in plain sight, it would be easier to infer that their choice reflects their preferences for the objects.[11]

Perhaps the best approach would be to combine their methods from the 2007 and 2010 papers. First, participants rate several items and the experimenter chooses three that are rated similarly (A, B, and C). Second, participants are given a blind choice between two of the three items (A and B). Third, participants are given a single open choice between the rejected item (B) and the third item (C). If participants choose B less often than C, then we could be sure that the results were not because of the information revealed by choice and we could assume that their choices reflected their preferences for the actual items. Of course, it is not clear that dissonance theory or self-perception theory would necessarily predict attitude change following a blind choice. But the method would successfully eliminate the choice information confound.

Manipulating choice

A final approach that researchers can use is to manipulate the choices that people make and then calculate spread for each participant based on their randomly assigned condition (and not their actual choice). This approach is similar to the first suggestion. Instead of getting all participants to make the same choice, however, the goal is to randomly assign participants to make a particular choice. As with the first suggestion, there are two central problems that need to be solved. First, researchers must effectively manipulate choice. Second, researchers must avoid directly manipulating preferences.

How might a researcher manipulate choice? Imagine that participants are asked to rank 15 art prints. If half of participants are told that their #7 ranked print is considered better by art experts and half are told that their #9 ranked print is considered better, then it is conceivable that people will choose based on that information (this would solve problem 1). But if people are given this expert advice, the 'better' print might improve in participants' rankings even if they never make the choice. In other words, the expert advice might directly affect preferences (and therefore fail to solve problem 2).

Imagine instead that participants rank 15 art prints and before choosing between #7 and #9, they are asked to flip a coin. They are told that if the coin lands on heads, they will get $1 extra if they choose #7 and if it lands on tails, they will get #1 extra if they choose #9. It is likely that people will choose based on the coin flip. If the $1 shifts people's choices, but does not directly influence preferences, then researchers can test for attitude change following choice by calculating how much #7 improves and #9 declines if the coin lands on heads and how much #9 improves and #7 declines if the coin land on tails.[12] If spreading is positive, it suggests that the choice process influenced subsequent preferences.

Conclusion

Although evidence has supported dissonance theory for many years in the induced compliance and effort justification paradigms, the problem of self-selection has made it impossible to interpret the spreading of alternatives found in the FCP. To be clear, then, the Chen and Risen (2010) criticism is directed specifically at the FCP (and not at dissonance theory more generally). In the current paper, we offer four methods that researchers can employ to properly test for dissonance in the FCP. By isolating the effect of the choice process on subsequent choice and preference, researchers can appropriately study choice-induced attitude change as well as the moderators and mediators of the effect.

More broadly, we would like to urge experimentalists to be cautious of 'self-selection' that can occur in nondissonance paradigms (e.g., dropping participants for failing a manipulation check). And we would like to urge dissonance researchers to reconsider the types of choices that participants are asked to make in FCP studies. If FCP studies have largely found spreading in the past because of the information revealed by choice, then to find strong evidence for choice-induced attitude change, researchers may benefit from moving from (not particularly significant) choices between art prints and CDs to more significant choices that are more likely to prompt dissonance.

Short Biographies

Jane L. Risen is an Assistant Professor of Behavioral Science at the University of Chicago, Booth School of Business. She received her BA in Psychology from Harvard University and her PhD in Social and Personality Psychology from Cornell University. Her research focuses on judgment and decision-making and social cognition. Her work examines processes that give rise to intuitive beliefs and the interaction between intuition and reason.

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Endnotes

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[1] Self-perception theory (Bem, 1967, 1972) was introduced as an alternative explanation for the dissonance findings. Specifically, Bem argued that the same pattern of behavior could be predicted without invoking 'dissonance'. Instead, he suggested that just as people learn about other people's beliefs and attitudes from observing their behav-

ior, so too they learn about themselves. Self-perception theory dispenses with motivation and simply assumes that one's own attitudes are inferred from behavior using the same cognitive processes used to infer the attitudes of others.

Responding to Bem's cognitive explanation, dissonance researchers focused on variables that would only be relevant if the process of attitude change was indeed motivated – namely, arousal and negative affect. Researchers found that the conditions that produced dissonant cognitions (e.g., writing a counter-attitudinal essay under conditions of 'free-choice') induced more arousal and negative affect than situations that did not (Croyle & Cooper, 1983; Elkin & Leippe, 1986; Elliot & Devine, 1994; Harmon-Jones, 2000; Harmon-Jones, Brehm, Greenberg, Simon, & Nelson, 1996). They found that the more the participants experienced negative affect and arousal, the more their attitudes changed. (Cooper, Zanna, & Taves, 1978; Losch & Cacioppo, 1990). Finally, they found that if participants could attribute their arousal to something other than their dissonant cognitions (i.e., to a placebo pill that was said to produce tension), their attitudes no longer changed (Zanna & Cooper, 1974).

Dissonance theory was uniquely supported by the studies that examined arousal and negative affect. Nevertheless, self-perception theory remains central in the field because of its explanatory power in other paradigms (see Fazio, Zanna, & Cooper, 1977 for an integrative view on dissonance theory and self-perception theory). For example, self-perception theory (and not dissonance theory) can explain the psychology behind the foot-in-the-door effect (Freedman & Fraser, 1966) and the over-justification effect (Lepper, Greene, & Nisbett, 1973).

[2] Festinger's original version of cognitive dissonance theory (1957) has also been challenged by researchers who agree that the process is motivated, but disagree about the cause of the unpleasant state that people are motivated to reduce. For example, Aronson's self-consistency theory (1968, 1999) suggests that dissonance occurs when people's actions violate their sense of self. Steele's self-affirmation theory (1988) suggests that dissonance reduction occurs because dissonance can threaten people's positive self-image. Cooper and Fazio (1984) suggest that dissonance occurs when people feel personally responsible for producing aversive consequences. Harmon-Jones and Harmon-Jones's action-based model of dissonance (2002) suggest that people are motivated to reduce inconsistency because it has the potential to interfere with effective action.

[3] In Linder et al. (1967), all participants who learned about the essay agreed to write it, and in Gerard and Mathewson (1966), all participants agreed to the group initiation. One participant in one of Aronson and Mills (1959) initiation conditions did not agree to participate. In Festinger and Carlsmith (1959), three participants in the $1 condition refused to be hired or told the student that the task was boring (instead of interesting) and two participants in the $20 condition refused to be hired.

[4] To our knowledge, dissonance theory and self-perception theory have never been pitted against each other in the FCP. Thus, although there is evidence that attitude change produced in the induced compliance and effort justification paradigms can be the result of a motivated process, we do not know of evidence supporting that claim in the FCP. Nevertheless, because the field generally refers to the spreading of alternatives in the FCP as evidence for dissonance reduction, we describe the paradigm from the perspective of dissonance theory. Note that both dissonance theory and self-perception theory predict that the mere act of choosing induces attitude change. In other words, for both theories, spreading is a reaction to the choice process. It is also important to note that the four suggestions we make at the end of the paper for properly studying choice-induced attitude change in the FCP are not designed to test between dissonance and self-perception theory. Our suggestions are designed to isolate the effect of the choice *process* from the information revealed by choice. Because both dissonance theory and self-perception theory predict spreading as a result of the choice process, however, then even if researchers properly isolate the spreading that is due to the choice process, it is still not clear which theory would better explain the spreading. To test between the theories, researchers could examine arousal and negative affect (as was performed in the other dissonance paradigms). Taking a different approach, we are testing to see which model better fits participants' ranking and choice behavior within a modified FCP experiment (Risen & Chen, 2010).

[5] Although Brehm (1956) used a rating procedure, many subsequent studies have used the simpler ranking procedure. We will use the language of a ranking procedure in our description, but because the two are theoretically equivalent, the same argument holds for a rating procedure.

[6] For example, 21% of participants in Brehm's (1956) study, 21% of participants in Gerard and White's (1983) study, and 36% of participants in Lieberman, Oschner, Gilbert, and Schacter's (2001) study chose the lower-ranked item. Note that 'choice reversals' (choosing the item that was initially ranked lower) are different from preference reversals that occur after a choice is made. Early dissonance researchers explored factors that would lead to dissonance reduction (coming to like the chosen item more) and factors that might prompt regret (coming to like the chosen item less) (see, for example, Brehm & Wicklund, 1970; Walster, 1964). The examination of postchoice preference reversals is interesting, but separate from the issue explored here.

[7] Some critics have tried to claim that our argument only holds if individuals' choices perfectly reveal their preferences (see, for example, Sagarin & Skowronski, 2009). This is simply not true. Assumption 2 specifically states that an individual's choice provides enough information about her preferences to predict that she is *more likely* to prefer the chosen item than the nonchosen item. We do NOT assume that an individual will prefer the chosen item with certainty. Please see Chen and Risen (2010) for a simplified proof in the text and the full proof in the Appendix.

[8] For a more complete discussion of this issue, see Chen and Risen (2009) and Sagarin and Skowronski (2009).

[9] On the surface, it might seem that the easiest way to get participants to make the same choice is to make the choice easier (it would be easy to get all participants to choose the item they ranked #2 over the one they ranked #14). But note that dissonance theory applies most clearly to difficult decisions. And self-perception theory should only apply if participants have some uncertainty about their preferences. Thus, researchers need to get all participants to make the same *difficult* choice between *close* alternatives.

[10] In the no-choice control condition, children were told to reach into the sock with lumps A and B and take the toy that the experimenter put on top (A). Then they made the same choice as children in the experimental condition. Namely, they chose between the original sock, which still had lump B and the other sock with lump C. The authors predicted that children would choose lump B 50% of the time. Surprisingly, the children chose lump B marginally more than lump C. The authors suggest that these results may have occurred because the children reacted negatively to being forced to take A over B (see Brehm, 1966).

[11] In Study 2, Egan et al. (2010) tested whether monkeys experience dissonance using the 'blind, then open' method that we call for. In certain respects, Study 2 is an improvement over Study 1 because it examines subsequent preference using 'open' choices. Nevertheless, we are very troubled by a different aspect of how subsequent preferences were measured and calculated. Thus, we disagree with the conclusions that the researchers draw from the experiment. We will briefly describe the methods and our concerns.

In the experimental condition, a monkey is shown two different colored candies (say, red and blue). The candies are put into a box of wood shavings, and one candy is surreptitiously removed from the bottom of the box (say blue), so that when the monkey searches in the box, he 'chooses' the red one. The experimenter lets him eat it. Then a different experimenter comes in, and repeatedly (ten times) offers the monkey a choice between a blue and green candy. The experimenters tested seven monkeys, each choosing blindly from the box once and then making ten subsequent open choices. The authors hypothesized that because the monkey pulled red from the box (rather than blue), he would devalue blue and show a preference for green across the ten choices. Overall, the authors report that the monkeys chose the green candy 42 of 70 times, or 60% of the time, and they claim that this provides evidence for monkeys experiencing dissonance reduction.

We have two primary concerns. First, the results they report are not statistically significant at the level most scientists consider reliable. The experiment finds that of seven monkeys, four show a weak preference for green, while three show no preference or a weak preference for blue. Depending on how ties are handled, this should happen at random (i.e., even if there is no dissonance) between 38 and 45 percent of the time. Second, we are troubled by the dependent variable. We do not understand why the authors predict that the first blind choice will influence the next 10 open choices, but do not predict that the open choices would influence each other. According to dissonance theory, the first open choice between green and blue should influence choices 2–10, the second choice should influence choices 3–10, and so on. We suggest that only a monkey's first open choice offers a clean test of the authors' prediction. If the monkey 'chooses' red over blue from the box, then, according to the authors, he should prefer green to blue in his first open choice. But of the seven monkeys' initial choices, four actually prefer blue, which does not support their prediction (this data was provided by the authors). Although we disagree with the authors' conclusions from this study, we approve of the 'blind, then open' method (as long as subsequent preference is cleanly measured by a single open choice).

[12] As long as the $1 does not provide participants with sufficient justification for their behavior, then dissonance theory and self-perception theory will predict spreading following their choice.

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