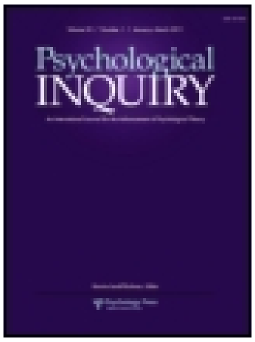

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TARGET ARTICLE

The Return of the Repressed: Dissonance Theory Makes a Comeback

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In 1957, Leon Festinger's theory of cognitive dissonance burst on the scene and revitalized social psychology with its deft blend of cognition and motivation. For the next two decades, the theory inspired an extraordinary amount of exciting research leading to a burgeoning of knowledge about human social behavior. The theory has been referred to as "the most important single development in social psychology to date" (Jones, 1976, p. x). But, by the mid-1970s the allure of the theory began to wane as interest in the entire topic of motivation faded and the journals were all but overwhelmed by the incredible popularity of purely cognitive approaches to social psychology. Recently, social psychologists seem to have rediscovered motivation and several mini-theories have emerged blending cognition with motivation—in much the same way that Festinger did some 35 years ago. This article traces the history of these developments and attempts a synthesis of some of the newer theories with the dissonance research of the late 1950s and early 1960s.

My students get a kick out of teasing me—by saying that, whatever else I might or might not be, I am primarily, a chronic and habitual storyteller—and I think they're right. So what I want to do (primarily) is tell you a story. Part of the story will be fairly traditional for this kind of occasion; that is, it will contain some brand new data fresh out of my laboratory. But, in addition, the story will include an homage to my dear friend and mentor, Leon Festinger, who died last year, marking the end of an important era in social psychology. It will also include a brief history of an idea—cognitive dissonance theory—as well as a central aspect of my philosophy of science (such as it is!). But mostly this story is a celebration of social psychology, a field that I have been madly in love with for the past 35 years.

I was not always in love with social psychology. As a matter of fact, when I entered graduate school in the mid-1950s, it was not my intention to become a social psychologist. I had read a little social psychology as an undergraduate, and it struck me as pretty boring stuff. The hot item at the time was the Yale research on communication and persuasion which, among other things, demonstrated that, if you present people with a message indicating that nuclear submarines are feasible, it is more effective if you attribute it to a respected physicist like J. Robert Oppenheimer than if you attribute it to an unreliable source like *Pravda*. I can see now that this was important and necessary research, but at the time, it seemed so obvious that, to an undergraduate, it hardly seemed necessary to perform an elaborate experiment to demonstrate that it was true.

In those days, almost everything done in the field was inspired by a rather simplistic derivation from reinforcement theory. Thus, in the previous example, it is clearly more rewarding (in the sense that it is more likely that one's opinions will be correct) to be in agreement with a trustworthy expert than to be in agreement with a biased newspaper run by a totalitarian government. Even classic experiments that were not specifically inspired by reinforcement theory (e.g., Lewin's work on democratic and autocratic leadership and the Asch experiment on conformity) could easily be recast and explained in terms of that simple and ubiquitous concept. The problem wasn't that there weren't other theories around. The problem was that there weren't other theories around that could make predictions that couldn't somehow be subsumed under the dominant and apparently more parsimonious wings of reinforcement theory. For example in the Asch experiment, because it was dealing with something as trivial as the size of a line, a reinforcement theorist might suggest that it is simply more rewarding to go along with the unanimous judgment of four other people than to defy that opinion and brave their scorn and ridicule.

Because the field was so thoroughly dominated by this simplistic brand of reward/reinforcement theory, whenever an individual performed a behavior it had to be because there was a concrete reward lurking somewhere in the background—so the name of the game, in those days, was let's find the reinforcer. It goes without saying that there are a great many situations where reinforcement works well as a way of increasing the frequency of a response, but is that all there is to social behavior? One suspected that the human heart and mind were more interesting than that—but, if they were, it didn't seem to be reflected in the bulk of the research that was being done by social psychologists.

Then along came Leon Festinger, and social psychology has not been the same since—thank God. It was my great good fortune to have arrived at Stanford to do my graduate work the same year that Leon arrived there as a professor. I did not apply to Stanford because of Leon—I did not even know he was going to be there (needless to say, he didn't know I was going to be there either!). I've been very lucky in my life as a social psychologist; I've managed to work with a lot of brilliant and wonderful people—some were my teachers, many were my students—but, in all that time, I've met only one person that I would call a flat-out, honest-to-goodness genius, and that was Leon Festinger.

Interestingly enough, Leon was not attracting large numbers of graduate students in those days; his reputation had preceded him and he was considered a very aggressive, harsh, devastating individual, possessed of rapierlike wit, who apparently was capable of devouring tender young graduate students like me for breakfast. I subsequently got to know Leon pretty well—indeed, he was to become one of my closest friends—and I want to tell you that, well, he *was* fully capable of devastating anyone in sight—and often did—and, as I subsequently discovered, he was also capable of enormous sensitivity, warmth, and tenderness.

But I didn't know that then, so it was with great trepidation that I walked into Festinger's office during spring quarter and told him I was thinking of enrolling in a seminar that he was teaching. It turned out to be a very small seminar (as I recall, there were four of us) because most of the students were so scared of him. I told Leon I did not know much about social psychology and I asked him if there was anything I could read in preparation for the seminar. He grunted, rolled his eyes toward the ceiling (as if to say, "just look what they're sending me these days"), and handed me a typed manuscript of a book he had just sent off to the publisher. He told me it was his only copy and he made me promise, under pain of death or dismemberment (whichever I preferred), not to let my young kids get blueberry jam all over it. Needless to say, I kept it well out of their reach.

The manuscript was called *A Theory of Cognitive Dissonance* (Festinger, 1957). I read it in one sitting. It knocked me out! It was the most exciting thing I had ever read in psychology. That was almost 35 years ago; it's *still* the most exciting thing I've ever read in psychology! Leon started with a very simple proposition: *If a person held two cognitions that were psychologically inconsistent, he or she would experience dissonance and would attempt to reduce dissonance much as one would attempt to reduce hunger, thirst, or any drive.* What Leon realized, in 1956, was the importance of forging a marriage between the cognitive and the motivational. Those of us who have survived the more recent era dominated by pure cognition in social psychology are well aware of the fact that, for a great many years, it has become fashionable to pretend that motivation does not exist, but, of course, that was merely a convenient fiction, as we shall see.

But I am getting ahead of my story. Let us go back to dissonance theory: It is essentially a theory about sense making—how people try to make sense out of their environment and their behavior—and thus, try to lead lives that are (at least in their own minds) sensible and meaningful. Inventing a theory that combined motivation with cognition led Festinger to the most amazing set of predictions, which produced a revitalization of social psychology. In its heyday, the theory generated over a thousand separate experiments, many of which were startling at the time, teaching us hundreds of new things about human behavior. It got us to look in places that we would never have dreamed of looking had it not been for the existence of that theory. When surveying the scope of the research generated by dissonance theory, no less a social psychology maven than Ned Jones (1976), characterized what he called the dissonance movement as "the most important single development in social psychology to date" (Jones, 1976, p. x).

I won't argue with that characterization. Indeed, I'll carry it a step further; the impact of dissonance theory went even beyond the generation of new and exciting knowledge. Because of the nature of the hypotheses we were testing, we were forced to develop a new experimental methodology, a powerful, high-impact set of procedures that allowed us to ask truly important questions very precisely. As you know, the laboratory tends to be an artificial environment. But the hypotheses we were generating made it necessary to overcome that artificiality by developing a methodology that would get the subjects enmeshed in a set of events—a drama, if you will—that made it impossible for subjects to avoid taking these events seriously.

In my writing on research methods (Aronson & Carlsmith, 1968; Aronson, Ellsworth, Carlsmith, & Gonzales, 1990) I have called this tactic "experimental reality," where within the admittedly artificial confines of the laboratory, real things are happening to real people. Because of the nature of our hypotheses, we could not afford the luxury of having subjects passively look at a videotape of events happening to someone else and then make judgments about them. Our hypotheses required the construction of an elaborate scenario that the subject became a part of. Several years ago, at an American Psychological Association symposium on ethics, I heard a well known social psychologist say that dissonance researchers resembled nothing so much as frustrated playwrights, directors, and actors. I am told he meant this as a criticism. I see it as high praise; the hypotheses to be tested demanded a high degree of realism and we rose to the occasion—with a great deal passion, I might add.

In addition, dissonance theory provided us with a powerful vehicle for challenging reinforcement theory on its own turf and led us to expose its limiting conditions and, on occasion, to discover that it was flatly wrong in some of its predictions. For example, reinforcement theory would suggest that, if you reward individuals for saying something, they might become infatuated with their statement (through secondary reinforcement). But Festinger and Carlsmith's (1959) experiment exploded that simplistic notion by showing that people believe lies they tell only if they are underrewarded for telling them. Also in 1959, Jud Mills and I (Aronson & Mills, 1959) performed an experiment demonstrating that people who go through a severe initiation to gain admission to a group come to like that group better than people who go through a mild initiation. Reinforcement theory would suggest that we like people and groups that are associated with reward; Mills and I showed that we come to like things for which we suffer.

I know this stuff is old hat now, but let me tell you that in the 1950s, simplistic assumptions from reinforcement theory were so dominant that when Jud and I floated our hypothesis and procedure past our fellow graduate students, they laughed. They knew that things become attractive through association with pleasure—certainly not through association with suffering. In 1957, dissonance theory sounded the clarion call for taking cognition seriously in social psychology; dissonance theory produced experimental research that demonstrated convincingly, like no other theory before it, that people think; we are not simple reinforcement machines. And, because we think, we frequently get ourselves into a tangled muddle of self-justification, denial, and distortion.

In a similar vein, dissonance theory challenged psychoanalytic theory, or more specifically, the notion of catharsis. Let me remind you that in the 1950s, the notion of the catharsis of aggression was widely accepted as axiomatic; that is, most psychologists believed that, if you are feeling hostility toward Sam you should get it out of your system (yell at him, call him names, or beat him to a pulp) and this will release your pent-up anger and you'll feel better about old Sam afterward. Dissonance theory said no—that, although this kind of behavior might release tension, as psychoanalytic theory suggests, it doesn't reduce your negative feeling about Sam. On the contrary, as several experiments have subsequently shown, if we hurt someone, it causes us to try to justify our actions by derogating our victim. This impels us to feel more hostility toward him, opening the door for still further aggression (see Davis & Jones, 1960; Glass, 1964; Kahn, 1966).

Perhaps most important, prior to dissonance theory the general wisdom among laypersons and psychologists was that, if you want people to change their behavior, you must first get them to change their attitudes. To take a dramatic example, in 1954, following the U.S. Supreme Court decision on the desegregation of schools, a great many psychologists argued that desegregation could not and should not take place, especially in the South, until after some of the prejudiced attitudes had been changed. Dissonance theory burst on the scene and suggested that, although that's one way to go, a more powerful way is to induce people to change their behavior first and their attitudes will follow. So, our advice would be, the best way to to change prejudiced attitudes is to desegregate. Several laboratory and field experiments, as well as the history of desegregation itself, have confirmed this prediction.

Although Leon once told me that he thought the theory was perfect as originally stated, almost from the very beginning, some of us felt it was a little too vague. Several situations arose, in the minds of those of us working closely with the theory, for which it was not entirely clear what dissonance theory would predict, or indeed whether or not dissonance theory even made a prediction. Indeed, among Leon's graduate students it was frequently said, with tongue only partly in cheek, "If you really, want to know whether *X* is dissonant with *Y*, ask Leon!" In short, it was becoming increasingly clear that the theory needed its boundaries tightened a bit. Accordingly, just 3 years after the publication of *A Theory of Cognitive Dissonance,* I suggested that dissonance theory makes its strongest and clearest predictions when the self-concept of the individual is engaged (Aronson, 1960). That is, in my judgment, dissonance is greatest and clearest when it involves not just any two cognitions but, rather, a cognition about the self and a piece of our behavior that violates that self-concept.

This modification retained the core notion of inconsistency but shifted the emphasis to the self-concept. I believe that this attempt to tighten dissonance theory was valuable inasmuch as it increased the predictive power of the theory without seriously limiting its scope. In a subsequent article, Merrill Carlsmith and I argued that Festinger's original statement (and all the early experiments) rested on the implicit assumption that individuals have a reasonably high self-concept; but if an individual considered himself to be a "schnook," he might expect himself to do schnooky things—like go through a severe initiation to get into a group, or say things that he didn't quite believe (Aronson & Carlsmith, 1962). A few years later, I carried my reasoning a step further (Aronson, 1968; Aronson, Chase, Helmreich, & Ruhnke, 1974), elaborating on the centrality of the self-concept in the experience and reduction of dissonance and suggesting that, in this regard, most individuals strive for three things:

1. To preserve a consistent, stable, predictable sense of self.
2. To preserve a competent sense of self.
3. To preserve a morally good sense of self.

Or, in shorthand terms, what leads me to perform dissonance-reducing behavior is my having done something that (a) astonishes me, (b) makes me feel stupid, or (c) makes me feel guilty. Needless to say, the three strivings can be in conflict with one another. For example, if, over the years, as a weekend basketball player, I have been a consistently poor free-throw shooter, sinking about 40%, and suddenly, in one game, I sink 12 in a row, do I feel strange (how could I have done so well when I am really awful at this?) or do I feel wonderful (at last, my true competence is emerging, hurrah!)? My guess is that, on the one hand, I would feel wonderful about having performed so well; but, at the same time, there would be some discomfort based on my inability to have gauged and predicted that performance. If our measuring instruments were sensitive enough, we would be able to pick up the discomfort lurking underneath the elation.

Moreover, in the real world, whether one or the other of these strivings dominates would depend on the details of the situation. As I have said often in the past (e.g., see Aronson, 1991), in both the laboratory and the real world the details of the situation are terribly important. To illustrate, extending the previous example, if my self-concept as a 40% shooter were not based on much prior experience and my 12 consecutive successful free throws were accomplished in an important game on national television, the rewards associated with my unexpected competence would all but overwhelm any negative cognitive consequences due to its unexpectedness. But, if my self-concept as a 40% shooter were based on years of experience, and the 12 consecutive successful free throws were accomplished while alone in the gym at the end of a practice session, the discomfort might well predominate.

Because, in our culture, success is such an important and gratifying thing, it is not easy to demonstrate the phenomenon of "discomfort in the face of unexpected success." This does not mean it is not important—only that people have a hard time admitting that they are uncomfortable while they are busily basking in glory or taking bows. To demonstrate this phenomenon in the laboratory it would be necessary to exercise a high degree of experimental artistry. Fortunately, the late Merrill Carlsmith was just such an artist. In an experiment (Aronson & Carlsmith, 1962) that worked largely because of his ingenuity in the laboratory, he and I demonstrated that individuals with low performance expectancy on a given test were made uncomfortable by performing superlatively on that test. As a result of this discomfort, when given the opportunity to redo the test, they changed significantly more of their answers (most of which were correct) than those who actually performed poorly on the test. Let me state again that the details of that experiment were very important. For example, Carlsmith and I selected a task that was of no great importance to the subject so that the discomfort with unexpected success could be measured.

One of the great advantages of this model is that it opens up the possibility of finding the conditions under which different ways of reducing dissonance are more or less likely to occur. Applying the self-concept model to the Festinger and Carlsmith (1959) experiment, what is dissonant is not the cognition that I believe "*X*" and I said "not *X*"; what is dissonant is that I see myself as a decent and clever human being and find that I have lied to another person in the absence of adequate justification. This makes me feel both guilty and stupid, so I rush to convince myself that the lie is really true. Note that if I knew myself to be both incompetent and immoral, I would have experienced little or no dissonance in that situation. Similarly, in the Aronson and Mills (1959) experiment, what is dissonant is that my cognition about my behavior is dissonant with my self-concept as a sensible, competent person. To have gone through hell and high water to get into a boring discussion group makes me feel stupid. Thus, I try to convince myself that the group was really pretty exciting. If I experienced myself as a generally stupid or incompetent person, I would have experienced little or no dissonance.

For approximately two decades, dissonance theory proved to be an extraordinarily fruitful and powerful explanatory concept both in and out of the laboratory. By the mid-1970s, it had transcended the boundaries of academic social psychology and was widely cited in scholarly journals in a variety of disciplines including economics, philosophy, political science, and anthropology. The concept also managed to seep into the popular culture, being featured in articles in the *New York Times, Newsweek, Playboy,* and alas, the *National Enquirer*. It even found its way into daytime soap operas.

But, ironically, just as a wide range of intellectuals and the general public were beginning to embrace the notion of cognitive dissonance, the pendulum started to swing and interest in dissonance theory among social psychologists began to wane. Indeed, by the end of the decade, dissonance experiments all but disappeared from the social psychological literature. Increasingly, during this period, I found myself the reluctant recipient of a great many invitations to appear on symposia entitled "What Ever Became of Dissonance Theory?" How did this come about?

There are several interlocking reasons, which I will mention very briefly:

1. Deception, which was an important ingredient in this kind of experimentation, was called into question. This was due, in part, to the blatant lying our government officials were doing about Watergate, about the Vietnam War, and so on. This made social psychologists very queasy about doing things in the laboratory that seemed similar to what Richard Nixon was doing on national television.

2. Dissonance experiments almost always required high-impact procedures; high-impact procedures frequently cause subjects some discomfort. Although dissonance experiments were not as extreme as some (e.g., the Milgram, 1965, experiment), they came under attack by those who felt that some subjects could, conceivably, be harmed by these procedures.

3. At a time when jobs in academic psychology were scarce, young professionals found themselves under a great deal of pressure to increase the quantity of their empirical publications. In this context, high-impact methodology seemed particularly forboding because it is difficult to pull off, time-consuming, and labor-intensive.

4. At the same time, just down the corridor of the psychology building, cognitive psychologists had been making enormous strides; it was natural for social psychologists to want to incorporate some of their theories and methods into the area of social cognition. When they did, they discovered that the methodology was easier and less time-consuming and it didn't present ethical problems. The motto of the experimentalist shifted from "How do we invent a scenario to convince subjects that such and such is going on?" to, "If it moves, prime it!" The video camera became the major presenter of the independent variable; subjects became the audience and judge rather than the participant in a given set of events in the laboratory.

I have strong feelings about this topic—and could go on and on (and, I'm afraid, on and on and on)—but I'll spare you that harangue. Let it suffice to say, that, in my judgment, most social psychologists abandoned high-impact experimentation prematurely and much too docilely and that the discipline lost something very precious in the bargain. In abandoning high-impact experimentation, we, unwittingly, all but ruled out the testing of several very interesting hypotheses, ones that simply could not be tested without using this kind of methodology. Moreover, almost an entire generation of graduate students did not receive training in this vital skill or, worse still, developed a negative attitude about it, as something that is *schmutzig,* problematic, and difficult.

With the rising tide of social cognition, the concept of motivation, and hence the theory of cognitive dissonance, simply became unfashionable like last year's hemline. People were simply not thinking in those terms anymore, not because dissonance theory was replaced by anything better but only by things that were newer and by a methodology that was quicker and easier.

In his brilliant and influential article on the intuitive psychologist and his shortcomings, my good friend and favorite interlocutor, Lee Ross (1977), suggested that it might be a good idea to temporarily abandon motivational constructs to concentrate on the purely cognitive influences on attributional judgments. This is the "convenient fiction" I mentioned a moment ago. I think this was a useful temporary strategy. But there are unfortunate consequences to this strategy. One of them is that we tend to forget that it *was* simply a convenient fiction and nothing more. Alas, social psychology has a long history and a very short memory.

During the cognitive revolution in social psychology, researchers not only lost interest in the concept of motivation, they seemed to forget that it existed. Interestingly enough, a great many social psychologists began to reinvent experiments to test cognitive notions that could easily have been done under the rubric of dissonance theory, but, now, there were different, nonmotivational terms for the phenomena under investigation. Most important, the connection between this new body of research and the older research was not noted and was therefore severed. There are dozens of examples of this phenomenon in the literature; I discuss only one. It is a particularly cogent example because it was a fine piece of research done by people I respect a great deal. Moreover, because the researchers are also friends of mine, I know they will not take offence at my singling them out—at least I *hope* they won't!

This is an experiment done by Charlie Lord, Lee Ross, and Mark Lepper (1979); they call the phenomenon under investigation "biased assimilation." Let me quote from the abstract of that article:

> People who hold strong opinions on complex social issues are likely to examine relevant empirical evidence in a biased manner. They are apt to accept "confirming" evidence at face value while subjecting "disconfirming" evidence to critical evaluation, and as a result to draw undue support for their initial positions from mixed or random empirical findings. (p. 2098)

Clearly, that experiment could have been done in 1957; it is easily derivable from dissonance theory. Indeed, in his 1957 book, Festinger made the identical prediction. Here's what Festinger said about what would happen to a person if he or she were forced to read a persuasive communication which went against a strong belief:

> One might expect to observe such things as . . . erroneous interpretation or perception or the material . . . . [for example], it is only among smokers [not nonsmokers] that one would expect to find skepticism concerning the reported research findings [linking smoking to cancer]. (p. 134)

The wording is almost identical.

The article by Lord et al. contains 32 references but not one to dissonance theory or any of the dissonance experiments. That is Lord et al. were content with a purely cognitive-heuristic explanation for their results. But just because it is possible to explain those results without recourse to motivational constructs does not mean that a motivational explanation is incorrect.

Don't get me wrong—I'm not making a dispositional attribution but a situational one; that is, I'm not accusing my friends Charlie Lord, Lee Ross, and Mark Lepper of shoddy scholarship—far from it. I single them out precisely because they are such irreproachably *good* scholars. I use this experiment solely to illustrate what happens when artificial barriers are erected and related theories get insulated from each other: We decrease our ability to forge vital syntheses and, consequently, our discipline becomes unnecessarily fragmented and disjointed.

During the past few years, many of the researchers who had been enthralled by social cognition in the 1970s and early 1980s have gradually come to the realization that pure cognition can carry us only so far. Accordingly, several social psychologists seem to have rediscovered the idea of motivation and have come to the conclusion that it might be interesting to try to combine cognition with motivation—in other words, exactly the strategy Leon Festinger employed so brilliantly 35 years ago. In short, the dreaded pendulum has started to swing again. Thus, in the past few years, a plethora of interesting minitheories has sprung up bearing such intriguing names as:

1. Self-affirmation theory (Steele, 1988).
2. Symbolic self-completion theory (Wicklund & Gollwitzer, 1982).
3. Self-evaluation maintenance theory (Tesser, 1988).
4. Self-discrepancy theory (Higgins, 1989).
5. Action identification theory (Vallacher & Wegner, 1985).
6. Self-verification theory (Swann, 1984).
7. Self-regulation theory (Scheier & Carver, 1988).
8. The concept of motivated inference (Kunda, 1990).

Each of these theories is a worthy and interesting effort at combining cognition and motivation, but each has a limited scope; in my judgment, with a little work, every one of them can be contained under the general rubric of dissonance theory, as modified in 1962. This is not meant to imply that they do not add something important. They do. My question is: Does it advance the science when we have seven or eight little theories doing the work of one? It doesn't seem very parsimonious, and it's a bitch to remember.

Among the most generative of the neodissonance approaches is one developed by Joel Cooper and Russ Fazio (1984) which they sometimes refer to as the "new look" dissonance theory. In examining the early forced-compliance experiments, like the Festinger–Carlsmith (1959) experiment and several others, Cooper and Fazio made an interesting discovery: In these experiments, not only was inconsistency present, but aversive consequences were also present; that is, lying to another person is usually aversive. In a bold theoretical statement, Cooper and Fazio asserted that, in this paradigm, dissonance is not due to inconsistent cognitions at all, but rather is aroused only when an individual feels personally responsible for bringing about an aversive or unwanted event. The astute reader will note that this resembles the third part of my self-concept analysis presented earlier in this article: specifically, the commission of an immoral act that makes a person feel guilty.

Although I always appreciated the boldness in Cooper and Fazio's theorizing, I could never bring myself to buy into the notion that aversive consequences are essential in this paradigm; that is, I couldn't believe that the other two parts of my "three-part" theory (predictability and the need to feel competent), under the proper conditions, wouldn't be sufficient to arouse dissonance in the forced-compliance paradigm. So I went back over the early experiments and found, much to my astonishment, that Cooper and Fazio were right—that in the early experiments on forced compliance, aversive consequences were always present. At the same time, this doesn't prove that aversive consequences are a necessary component. Is it possible to have dissonance without aversive consequences? This is the crucial question. Is there any way to test it? Or, are the two factors hopelessly intertwined?

I struggled with this one for a long time, to no avail. And then something interesting happened. I had placed the Cooper–Fazio model on the back burner of my mind while I was working on a challenging problem in the application of social influence processes to problems in the real world. Specifically, I was trying to find a way to convince sexually active college students to use condoms as a way of stemming the epidemic of AIDS and other sexually transmitted diseases. I had tried several of the traditional persuasive techniques, with very little success, and then I thought about using the forced-compliance dissonance paradigm.

In thinking about it, I constructed the following scenario: Suppose you are a college student and you are induced to make a persuasive videotape (to be shown to an audience of sexually active high-school students, as part of a sex education course) in which you proclaim your belief that sexually active people should always use condoms to prevent AIDS. Will you experience any aversive consequences? My guess is that Cooper and Fazio would have to say no. Quite the contrary; far from causing harm, your speech might even save the lives of some of those who hear it. But wait a minute—there is no dissonance in this situation either, is there?

It seems not. But suppose, in one condition, just after you make the speech, you are made mindful of the fact that there are some situations in which you yourself do not use condoms while having sex. Here we have an interesting situation. According to my version of the theory, this would produce dissonance because you are not practicing what you are preaching. That is, for most people, their self-concept does not include behaving like a hypocrite. Thus, in this situation, we can disentangle what I would call dissonance from any aversive consequences: Your cognition that you are advising others to do things that you yourself do not do, would be dissonant with your self-concept as a principled person who practices what he or she preaches. This would cause dissonance even though the algebraic sum of the consequences of your action are overwhelmingly beneficial. How would you reduce dissonance? By resolving to change your behavior to bring it into line with your statements so that you will now be practicing what you just got through preaching. In this case, you would increase your resolution to use condoms.

In an experiment I recently completed with two of my students, we followed the plan I have outlined (Aronson, Fried, & Stone, 1991). In a 2 × 2 factorial design, in one condition, college students were induced to make a videotape in which they urged their audience to use condoms; they were told that the video would be shown to high-school students. In the other major condition, the college students simply rehearsed the arguments without making the video. Cutting across these conditions was the "mindfulness" manipulation: In one set of conditions, our subjects were made mindful of the fact that they themselves are not practicing what they are preaching, by being asked to think about all those situations where they found it particularly difficult or impossible to use condoms in the recent past. Other students were not made mindful of their past failures to use condoms.

The one cell we expected to produce dissonance is the one high in hypocrisy—where subjects made the video and were given the opportunity to dredge up memories of situations where they failed to use condoms. Again, how did we expect them to reduce dissonance? By increasing the strength of their intention to use condoms in the future. And that is precisely what we got. Those subjects who were in the high-dissonance condition showed the greatest intention to increase their use of condoms. Moreover, 2 months later, there was a tendency for the subjects in the high-dissonance cell to report using condoms a higher percentage of the time than in any of the other three cells.

These results are provocative but not conclusive. Although the data do show a significant difference in the change in intentions to use condoms, they do not, by themselves, fill us with enthusiasm. There are some problems with this experiment, not the least of which is our choice of a dependent variable. As an old "high-impact" experimenter, I'm more than a little uncomfortable with a dependent variable that involves the self-report of intentions and behavior rather than a behavioral measure of actual condom use. Unfortunately, when you are dealing with sexual behavior, you don't have much choice; after all, I (or should I say *even* I) would not be so bold as to try to follow our subjects into their bedrooms to see if they really do use condoms. Believe me, I thought about doing it, and if there were a reasonable way, I think I would have found it.

One way to increase our confidence in the efficacy of the "induction of hypocrisy" paradigm as a way of increasing condom use is to try to test the paradigm in a different situation, one where we stand a chance of demonstrating the phenomenon using a more convincing dependent variable. We found one in the shower room of our campus field house. As you may know, central California has a chronic water shortage. On our campus, the administration is constantly trying to find ways to induce students to conserve water. So we decided to test our hypothesis by using dissonance theory and the induction of hypocrisy to convince students to take shorter showers. We discovered that whereas it is impossible, within the bounds of propriety, to follow people into their bedrooms to observe their condom-using behavior, in our society, one can easily follow them into the shower room and watch them take showers.

In this experiment (Dickerson, Thibodeau, Aronson, & Miller, in press), conducted at the university field house, we intercepted college women who had just finished swimming in a highly chlorinated pool and were on their way to take a shower. As in the condom experiment, it was a 2 × 2 design in which we varied commitment and mindfulness. In the commitment conditions, each student was asked if she would be willing to sign a flyer encouraging people to conserve water at the field house. The students were told that the flyers would be displayed on posters; each was shown a sample poster—a large, colorful, very public display. The flyer read: "Take shorter showers. Turn off water while soaping up. If I can do it, so can you!" After she signed the flyer, we thanked her for her time, and she proceeded to the shower room, where our undergraduate research assistant (blind to condition) was unobtrusively waiting (with hidden waterproof stopwatch) to time the student's shower.

In the mindful conditions we asked the students to respond to a water conservation "survey," which consisted of items designed to make them aware of their proconservation attitudes and the fact that their showering behavior was sometimes wasteful.

The results are consistent with those in the condom experiment: We found dissonance effects only in the cell where the subjects were preaching what they were not always practicing. That is, in the condition where the students were induced to advocate short showers and were made mindful of their own past behavior, they took very short showers. To be specific, in the high-dissonance cell, the length of the average shower (which, because of the chlorine in the swimming pool, included a shampoo and cream rinse) averaged just over 3½ minutes (that's short!) and was significantly shorter than in the unmindful–uncommitted condition.

Both these experiments produced changes in important behavior that were beneficial to society. Moreover, when taken together, the results indicate that aversive consequences may not be a necessary component of dissonance in the forced-compliance paradigm.

Why do I think it's better to have one big theory rather than seven or eight little ones? Is it simply a matter of aesthetics or what? No, it's much more than that. As Leonard Berkowitz and Trish Devine (1989) have recently indicated, social psychologists have been much more inclined toward analysis than synthesis. By *analysis,* Berkowitz and Devine referred to the careful delineation and differentiation of the theoretical concepts and propositions that lead to the prediction of different outcomes. By *synthesis,* they referred to the bringing together of apparently disparate observations under a common theoretical umbrella. It goes without saying that both orientations are vitally important to any discipline. But, in my judgment, a problem has arisen in social psychology because there seems to be much more payoff for analysis than for synthesis; a good analysis simply seems more original and creative than a good synthesis. Among other things, this has led to a huge imbalance in the analysis–synthesis ratio during the past several years, resulting in a plethora of small theories with hardly anyone taking the trouble to try to find the common ground among these theories. But, as Berkowitz and Devine pointed out, this has been costly because synthesis offers great advantages in terms of economy of thought and connectivity among approaches, which can serve to help us discover the full meaning of any given theory. Let me give you a few examples from dissonance theory to illustrate why I think synthesis might be particularly important here.

As mentioned previously, Merrill Carlsmith and I (Aronson & Carlsmith, 1962), working from the "self-concept" revision of dissonance theory, did an experiment in which we found that under certain conditions, college students would rather be able to predict and confirm their own behavior than succeed on a test. Specifically, students who believed themselves to be inept at a given task ended up changing their answers on a successful performance (testing their abilities on that task) as a way of restoring their self-predictability. Twenty-six years later, working from Bill Swann's notion of self-verification, Swann and Pelham (1988) found that people prefer to remain in close relationships with those friends and roommates whose evaluations of their abilities are consonant with their own (sometimes negative) self-evaluations. In other words, people prefer to be close to someone whose evaluations of them are consonant with their self-concept as opposed to someone whose evaluations of them are more positive than their self-concept.

I see this as not merely an interesting new finding; on the contrary, these results assume great importance precisely because of their linkage to and extensions of the earlier findings already described. That is, when theorists and researchers build on previous theory and data, it enhances our discipline by highlighting its continuity. These two pieces of research have in common an aspect of dissonance that was identified in some of our earliest thinking on this issue—the need people have to form a stable self-concept and to predict their own behavior. Thus, individuals will try to behave in predictable ways (as Carlsmith and I found in 1962) and they will be most comfortable around people who neither expect too much nor too little from them (as Swann and Pelham found in 1988). Twenty-six years is a long time in social psychology. Given our field's proclivity to avoid synthesis, it is little wonder that Swann and Pelham failed to recognize the full meaning of this connection.

Working in the same framework, Swann and Steven Read (1981) found that people elicit behavior from others that will lead to the verification of their own beliefs about the self. To take a central example used by Swann and Read, if a man believes himself to be highly tractable, he will seek out others who treat him as if they expect him to be tractable. This is very interesting, and it assumes even greater importance when we compare it with what Festinger wrote almost 35 years ago. In 1957, Festinger stated that one way to preserve consonance is by changing an environmental cognitive element: "For example, a person who is habitually very hostile toward other people may surround himself with persons who provoke hostility. His cognitions about the persons with whom he associates are then consonant with cognitions corresponding to his hostile behavior" (p. 20).

The two examples are essentially identical. In view of this sameness it makes one wonder why we need a separate theory called "self-verification" to account for the phenomenon being described. As I have said elsewhere (Aronson, 1989), our zeal for the analytical approach (at the expense of the synthetic) tend to blind us to these similarities and induces us to read some of the older theories carelessly—as if they were ancient history and, therefore, of little value to contemporary researchers and theorists. Indeed, as if he were intentionally trying to provide me with data to bolster my feelings about the high price our discipline has been paying for being overly analytical, Swann (1990) recently casually dismissed dissonance theory for having ignored the self-verification tendency in humans and, therefore, being "nothing more than a cleverly disguised version of self-enhancement theory" (p. 413). A careful reading of Festinger (1957) might have led Swann to a more generous conclusion.

Let us look briefly at Claude Steele's (1988) notion of self-affirmation in the context of some of the older work. First, the older work: Dave Mettee and I (Aronson & Mettee, 1968) did an experiment, inspired by dissonance theory, in which we demonstrated that if we raised individuals' self-esteem, it would serve to insulate them from performing an immoral act like cheating. We found that the higher self-esteem served to make the anticipation of doing something immoral more dissonant than it would have been otherwise. Thus, when our subjects were put in a tempting situation, they were able to say to themselves, in effect: "Terrific people like me don't cheat!" And they succeeded in resisting the temptation to cheat to a greater extent than those in the control condition.

Recently, working from his concept of self-affirmation, Steele and his students (Steele, 1988) found that if people are put in a dissonant situation—a situation where they misled another person (as in the Festinger–Carlsmith experiment)—there was one condition under which they did not reduce dissonance in the usual way by changing their attitudes. Specifically, those subjects who were given an opportunity to affirm some important aspect of their self-concept (e.g., that they were a kind and generous person or a good scientist or whatever) were able to maintain their original attitudes without caving in to the pressures of dissonance to soften their original attitudes. How exciting! The connection between this experiment and the Aronson–Mettee experiment is obvious. Taken together, they show that bolstering the self-concept is both a way of helping the individual avoid performing behavior that will produce a truckload of dissonance (Aronson–Mettee) as well as a way of reducing dissonance that already exists (Steele). The new findings are interesting and important, but I do not think that we need a new conceptualization to account for them. Indeed, it is precisely by keeping both sets of findings under the same roof that we can fully appreciate the interrelatedness of the two experiments and thereby gain a richer understanding of the dissonance phenomenon.

I realize that I'm beginning to sound like the worst kind of smarty-pants: I'm taking interesting research and theorizing that's been done in the past few years and claiming that we did something very similar 25 or 30 years ago. What's worse, I fear I may be coming on like an old curmudgeon, who seems to be longing for the good old days and who is apparently claiming that there is nothing new under the sun. Some might even accuse me of trying to make of dissonance theory an all-purpose explanation for everything—precisely what I criticized reinforcement theorists for trying to do in the 1940s and 1950s. Let me reiterate that I'm not simply saying that there is nothing new under the sun. I don't believe that. Moreover, I hope it is obvious that I don't believe that dissonance theory does or should account for everything. Far from it. I see the scope of dissonance theory as being limited to a clearly defined set of psychological situations (see Aronson, 1969). But where there *are* related phenomena, I believe that it can be of great value to view them under the same rubric—at least until their similarities and differences can be empirically investigated and explored.

In this sense, then, dissonance theory *is* making a comeback, but under a variety of different names. I think the time has arrived for a grand synthesis; now that social psychology has rediscovered the richness in the hypotheses to be generated by combining the cognitive with the motivational, I believe that it would be a serious mistake to diffuse that energy into a series of unconnected minitheories. I believe that it is appropriate to reach back into our fertile past to achieve continuity as we continue to discover new and interesting things.

Establishing the kind of continuity I recommend will not only make our discipline more understandable, it will also generate richer hypotheses. This thinking reflects my basic philosophy of science, such as it is. Needless to say, not everyone will agree with me. For example, Bill Swann (personal communication, June 14, 1990) recently criticized my approach. Swann acknowledged that my old tripartite view of dissonance theory managed to embrace both the major assumption of self-enhancement theory and the major assumption of self-verification theory. But for Swann, this is disadvantageous. He wrote, in part:

> Here then is the problem with your suggestion that Steele and I are saying the same thing that you said back in '68. I predict one thing, he [Steele] predicts the opposite, and you predict both. How can you say that three theories that have such different properties are saying the same thing? (Swann, 1990, personal communication, June 14, 1990)

I replied, in part:

> Regarding the issue of whether people strive for a consistent sense of self or a good/competent sense of self, I would suspect that most sentient social psychologists would agree that, it's not a matter of whether Bill is right and Claude is wrong or vice versa. Rather, we know that both in the real world and in the laboratory, either phenomenon *can* occur and frequently *does* occur—depending on the precise details of the situation. As I've said (and said, and said—throughout my whole career, it seems) *the details are always important.* (July 24, 1990)

In sum, to my mind, the synthetic approach, as illustrated here, is attractive because it highlights a philosophical credo that, as a researcher, I hold dear: The task of the scientist is not to prove one proposition right and true and the other utterly false, but to painstakingly find the conditions under which one or the other is more likely to occur (in this case, the conditions under which the individual will seek out self-enhancement or stability). So, it is not that my formulation leads me to predict both, as Bill Swann indicates; instead, I am suggesting that both a stable sense of self and a competent, moral sense of self are desirable and can be sought out, each under a specific set of conditions. The task of the researcher is to find and demonstrate precisely what those conditions are. The astute reader will notice that this approach opens the door for analysis *after* the necessary synthesis has taken place.

This philosophy of science has some advantages, most notably that it turns the research endeavor not into a contest to see who is smarter or "righter" but into a mutually beneficial, cooperative endeavor to get closer to an understanding of human thought and behavior. Because I firmly adhere to this philosophy of science, I also believe strongly that, whenever possible, we should try to build on one another's work rather than continually strive to strike out in "original" new directions. This is precisely what I mean by creative synthesis.

Notes

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